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PART II

EPIDEMIOLOGICAL SECTION	LARYNGOLOGICAL SECTION
MEDICAL SECTION	NEUROLOGICAL SECTION
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COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE
SESSION 1908-9

EPIDEMIOLOGICAL SECTION



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EPIDEMIOLOGICAL SECTION.

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Epidemiological Section.

October 23, 1908.

Dr. A. NEWSHOLME, President of the Section, in the Chair.

Season and Disease : A Preliminary Study.

By OWEN H. PETERS, M.D.

PART I.

THE study of disease, as regards its variations in prevalence, is always a complex one. The continual interaction of many factors must be reckoned with, and the exact determination of the part which any one of them plays will depend largely upon the success with which the disturbing influences of the others can be eliminated. Three of these agencies which are particularly important are as follow: Changes in the infectivity of the specific organism; changes in susceptibility of the general population; and seasonal conditions, such as heat or cold, rain or drought. While investigating the third only of these factors it is well to remember that the variations in prevalence of most diseases are probably influenced by all three, and at the outset a method must be sought which will as far as possible avoid the disturbing influence of the other two agencies. For this reason the statistical method of gauging the seasonal influences by weighing the aggregate prevalence of a number of hot or dry years against that of a number of cold or wet years has not always given satisfactory results. Nevertheless this method, so extensively applied by Dr. Newsholme and other observers, has a special and wide sphere of applicability, allowing comparisons to be made and general principles to be established, giving results which could not be arrived at in any other way. Such important general truths are, however, commonly found to be based upon particular principles, which are not a whit less important, and an attempt has been made in this paper to attack the question from another standpoint, affording, if

possible, a more exacting analytical method. It was noticed that the date of onset of the seasonal epidemic, in diseases of the so-called summer-autumn type, differed widely in different years. The question then arose, Were these variations merely haphazard, or in conformity to some definite law? It was decided to test this matter by reference to meteorological and other data, and, as a matter of fact, in the course of this research some very important facts were discovered as to the relations of temperature and disease. A second line of research was also instituted into the effect of variations of temperature upon variations in prevalence of disease when the seasonal epidemic has already been established. At present this inquiry will be confined to the study of epidemic diarrhœa and enteric fever, both of which commonly show a sharply defined seasonal rise in the late summer or autumn; and in these it is hoped to exhibit certain fundamental principles which may possibly be of general application in the solution of the problem of seasonal prevalence in many other infectious diseases.

Epidemic diarrhœa and enteric fever will be mainly investigated from the data of the last eighteen years at Nottingham, at which place this paper was originally prepared. The conclusions arrived at will be further confirmed by reference to a large collection of English and foreign statistics. The subject is best pursued by a close study of the charts, which set forth as large a part of these data as it has been found possible to include.

Chart I sets out the whole eighteen years, one under the other. Two heavy zigzag lines pass down the chart, connecting the points of onset of the diarrhœa or typhoid epidemic in each year. These will be referred to as the "D and E lines." Fine lines passing down the chart show the dates at which certain temperatures were reached in the different years; the temperature plateaux included by these lines above 55° F. and 60° F. have their respective cross-hatchings.

Chart II shows the weekly averages of the one-foot earth thermometer at Nottingham up till the point of rise of enteric fever. After that the air temperatures are shown. The points of onset of the diarrhœa and typhoid epidemics are joined, and are the same as those similarly connected in Chart I.

Chart III shows the yearly curves of diarrhœa deaths (dotted line) and enteric fever notifications (continuous line) in weekly records at Nottingham, and the manner in which the more or less arbitrary matter is settled as to the positions of the D and E points, representing the dates of the *main* rise of the diarrhœa and typhoid epidemics. It will

be seen that a careful effort was made to apply the same rule all round by continuing the general slope of the curve down to the base line and by deciding to include, as the first week, only one with a total of more than three deaths in the case of diarrhoea or of more than two cases in typhoid. Where an abrupt rise is not present and the point of rise is not therefore certain, as sometimes happens in typhoid fever, the D or E is accompanied by a query.

Charts IV and V give similar facts for London. The other charts will be dealt with at a later stage.

It will be noticed that in the first part of the paper the date of death is made use of in one disease and that of notification of sickness in another. These will be adhered to because, in the first place, they at least yield recognized fixed points at which to start investigations, and so are to be preferred to mere estimations of the date of infection. Again, if the dates in which infection was communicated are estimated, diarrhoea would be carried back two weeks and enteric fever about three weeks; their positions with respect to each other would not then be altered by much more than a week. Again, the D and E points were determined in great measure by reference to the slope of the curve during the first two or three weeks of the established rise; thus they are, for present purposes, best referred to the data chosen.

Turning now to Chart I, there are deduced from a study of the relations of the D and E lines with the temperature lines three important facts:—

(1) That the time of the epidemic rise varies considerably in different years. Both in diarrhoea and typhoid the rise is ten weeks later in the latest year than in the earliest.

(2) That the time of attainment of summer temperatures also varies very widely in different years.

(3) That, even after only a superficial examination of the chart, a very general correspondence is undoubtedly observed between the earliness or lateness of the epidemic rise and the earliness or lateness of the development of summer temperatures.

What, then, is the precise nature of this correspondence? There is, perhaps, an exact relationship; and, if so, with what particular temperature influence or combination of influences is there an exact relationship? It is well known that it is not the first time this question has been asked, and strenuous efforts have long been made to provide some satisfactory answer. Efforts have, however, been practically confined to an attempt to demonstrate a definite relation between the date of the

epidemic rise and the attainment of a certain fixed temperature of air or soil. This attempt has, however, been far from successful, and a reference to Charts II and IV will show how impossible it is to entertain such a theory. Two hostile facts will there be observed: firstly, the seasonal rise takes place at widely differing temperatures; and, secondly, in the weeks preceding the rise considerably higher temperatures have often been passed through than those at which the rise occurs. As regards the air temperatures, in both diseases the rise is seen to take place at anything from 65° F. down to 55° F., and many instances are seen of weeks of higher temperature preceding the week of rise. A reference to one-foot earth temperatures shows that they present the same two difficulties, only not to so marked a degree, being under the steadying influences of the deeper earth temperatures. Lastly, the four-foot earth temperatures may be taken as an example of these deep earth temperatures, and it can be demonstrated that here again, though to a greatly diminished extent, on account of their still steadier course, the same two objections still hold good. The correspondences and irregularities of the three temperatures are well illustrated in the year 1902, Chart VIII. The subject of four-foot earth temperatures will, however, be reserved for further discussion.

If, then, it is not the mere arrival at a certain fixed temperature that is related to the date of the seasonal rise, investigations need not be abandoned at this stage, for the temperature may be conceived to act in still another way: the seasonal rise may be related to the cumulative effect of certain degrees of temperature extending over a certain time. This solution will now be followed up, and, as a matter of fact, it will be found to yield a very satisfactory and elastic theory which will fit in with every variety and irregularity of seasonal temperature development. It will be well at the outset, however, to clearly comprehend what this theory implicitly states: firstly, the maintenance of certain temperatures is demanded; and, secondly, the existence of a certain period of time is implied, so that it is suggested that the point which marks the *main* rise of the seasonal epidemic of diarrhoea or enteric fever marks also the end of a period of several weeks' duration, during which the influences of certain early summer temperatures were in evidence. Before, however, proceeding to test this question by a careful examination of the charts appended, sidelights may be thrown upon the actual probabilities of this theory of accumulated temperatures by comparing the relative effects of the seasonal temperatures upon the rise of disease with that upon the various phenomena of the vegetable world. Although such a

comparison may appear rather unprecedented, it was as the result of detecting the curious analogy between these two that the investigations were commenced which led to the writing of this paper.

In addition to the D and E lines in Chart I, a heavy dotted line—the “H line”—will be seen passing down the chart and connecting points which indicate the dates of commencement of the corn harvest [5] in a limited area on the municipal boundary of Nottingham. The precise relation of these three lines to one another may now be examined. The most striking feature of Chart I is the marked tendency to parallelism between the D and E lines. The beginning of the typhoid is also seen to average about four weeks later than the diarrhoea, or about three weeks if a reference be made in each case to the date of infection. Where this interval is larger or smaller, far from providing embarrassing exceptions such years point to certain important and specific differences in the behaviour of the two diseases which will be hereafter dealt with. Again, quite as interesting a feature of Chart I is a certain tendency on the part of the H line to be parallel to the D and E line. This tendency is, however, confined to one particular—the H line bends forward and backward in correspondence with the movements of the D and E lines. It lacks complete parallelism, however, in the fact that these oscillations are always less, a five weeks' interval separating the extreme points between which it oscillates, while a ten weeks' interval obtains in the case of the other two. Thus, in passing down the diagram from the earliest to the latest year, the D and E lines cross over the H line, so that while in the earliest year (1893) the diarrhoea and typhoid epidemics both commenced before the harvest, in the latest year (1907) they were both found to follow it. Of course it is unavoidable, as the outcome of such remarkable correspondences, that the question should be raised as to how far these corresponding effects are the result of similar or even common causes. With regard to the harvest, the ripening of the grain represents a long period of development; and the date of harvest, other things being equal, is to be wholly referred to the cumulative effect of temperatures throughout each stage of that period. A relatively higher average temperature in one or all stages means, to a corresponding extent, an earlier harvest. It would seem, then, that the commencement of diarrhoea and typhoid is similarly determined by the accumulated temperatures of a preceding period, for the relation of the D and E lines to the H line are precisely what they would be if they represented the completion of the life-cycle of two plants with a much shorter period of growth, and requiring slightly

higher temperatures than the wheat plant. These two differences would in effect furnish a complete explanation as to why the D and E lines cross the H line.

The wheat plant is known to have a period of growth extending over three or four months, which only yields to the forcing effect of high temperatures to a very limited extent. Hence it happens that, a start not generally being made before the end of April, the earliest year (1893) did not succeed in bringing the harvest earlier than the third week in July. The shorter periods of development of the hypothetical diarrhoea and typhoid plants would, however, allow them to take full advantage of even a few early weeks, and thus the date of completion of their life-cycle might be carried much further forward. In a cold and late year, however, the wheat plant, which can mature in much colder countries than this, is least affected, but the maturing of the diarrhoea and typhoid plants would be very much delayed, and so would tend to come after the harvest. The full significance of the relations and irregularities of the D, E, and H lines will, however, be better appreciated when the temperature conditions preceding the two epidemics have been fully investigated; it might, however, be further suggested that if it is impossible to conceive of a certain summer temperature being able to convert a sown field into a ripe harvest during one particular day or week, it is perhaps just as unlikely that the establishment of an epidemic might be referred to the mere attainment of a certain fixed temperature of air or soil.

That there is actually a period in both diarrhoea and typhoid intervening between the arrival at favourable temperatures and the main seasonal rise is at once evident on reference to the various charts. Two illustrations will, however, suffice for the present: in the year 1899, at Nottingham (Chart I), the temperature stood at practically a dead level of 60° F. for four weeks preceding the rise in diarrhoeal deaths; in 1906, at Blackburn (Chart IX), a constant level between 56° F. and 58° F. was maintained for the preceding ten weeks. If the temperature had any influence at all, the only conclusion that can be drawn from these examples is that not only is the degree of temperature important, but a period of time must also be allowed for, its length evidently depending on the height of the temperature, for the latter was evidently much more potent at 60° F. Before, however, proceeding to a more complete demonstration of these facts, a brief reference will be made to the views of former writers on these subjects.

In the first place Dr. Ballard [1], in seeking some explanation for the marked pause which occurs before the reaction to temperature becomes

evident, formulated a theory which will be here set forth in his own words: "The summer rise of diarrhoeal mortality does not commence until the mean temperature recorded by the four-foot earth thermometer has attained somewhere about 56° F., no matter what may have been the temperature previously attained by the atmosphere or recorded by the one-foot earth thermometer." "I do not wish it to be inferred that the atmospheric temperature and the temperature of the more superficial layers of the earth exert no influence in diarrhoea. Their influence, however, is little, if at all, apparent until the temperature recorded by the four-foot thermometer has risen as stated above. Then their influence is apparent, but it is a subsidiary one." Although in his provisional hypothesis Dr. Ballard suggests that "the essential cause of diarrhoea resides ordinarily in the superficial layers of the earth," yet it cannot be denied that the above insistence on the predominant influence of the four-foot earth temperature led to the general impression, whether intended or not, that the organisms must be supposed to be developed in the situation where they can be exposed to the predominant temperature influence. To obviate the difficulty of explaining how the organisms succeed in reaching the surface from such depths, Dr. Tomkins [20] has urged that the one-foot earth thermometer should be read in conjunction with the deep one, but that the former should be considered the more important of the two. As a matter of fact, as the deep temperatures are rejected for the more superficial ones the correspondence of the diarrhoea rise becomes still less exact. This matter has been previously referred to, and the statement was made that the rise of diarrhoea could not be referred to the mere attainment of a certain fixed air or soil temperature, for the two reasons that the rise was actually seen to take place at widely different degrees of temperature and that it often occurred at a temperature lower than that of several days or weeks previously passed. These two irregularities were plainly visible in the case of the air temperatures, were less so in the superficial earth records, and were considerably reduced, but none the less demonstrable, in the four-foot earth records. In fact, the attention paid to the correspondence of the diarrhoeal rise with a fixed temperature of the four-foot earth records is wholly due to a purely fortuitous combination of circumstances, the full explanation of which seems to be peculiarly difficult, and does not lie primarily, as is commonly stated, in the fact that the deep earth temperatures are records of a cumulative temperature effect. Incidentally, the latter idea appears to be one which has been read into the theory since its original exposition. The two circumstances, accidentally

combined, which are of real importance are as follow: The irregularities of the surface temperatures, which presented an insurmountable obstacle, are at a depth of 4 ft. more or less smothered or reduced to a minimum. The four-foot earth record thus usually presents an uninterrupted ascent. It happens also that the period of accumulated temperatures preceding the rise in diarrhœal deaths is comparatively short, so that the completion of that period may generally be referred to a point on the still rising four-foot earth record. If, however, as not infrequently occurs in this insular climate, a summer standard is only maintained for two or three weeks, the four-foot thermometer begins to decline before the diarrhœa aggregate is complete and the rise in deaths has occurred. The inherent fallacy and the inelasticity of the theory of the 56° F. limit is thus exposed. In the charts, the years 1902 and 1907 suitably illustrate these discrepancies, but in more northerly towns, with still cooler summers, such occurrences are more often the rule than the exception. And, lastly, 56° F. is not reached in some of these, and still the diarrhœa may occur if the autumn is sufficiently extended (*see also* Chart VIII, 1902, and Chart IX, 1906 and 1907, Blackburn). The fortuitous arrangement of circumstances on which the theory rests is further illustrated by considering the case of typhoid, which it will be demonstrated has also a preceding period of accumulated temperatures, but some weeks longer than that of diarrhœa. It is evident that there is no suggestion of referring the seasonal rise of this disease to the four-foot earth temperatures, mostly owing to the longer period and the necessarily constant occurrence of the same kind of discrepancies which are met with in diarrhœa when the season is unusually short. A reference to a deeper earth temperature might, however, be suggested which would continue to rise some time after the four-foot thermometer became stationary. As a matter of fact, in the case of cholera [9], the rise and decline have been referred to the reading of the six-foot earth thermometer, and so we might be led on further and further into the depths of the earth, from where it becomes less and less possible to imagine how organisms can ever get to the surface. It is evident that this pursuit of deep earth temperatures has been a means of leading the study of epidemiology further and further from the real scene of action, which is above ground, and, literally speaking, burying it deeper and deeper in difficulties.

The subject has been touched upon by various writers, some of whom have commented upon the existence of the latent period and the temperature conditions that accompany it. Turner's theory [22] that certain

temperatures must be maintained for three weeks before diarrhoea becomes excessive does not relate to this period, but simply expresses the fact that, owing to the average length of fatal illness being about a fortnight, the effect of temperature upon the number of cases attacked is chiefly evidenced in the mortality of the week but one following. The most interesting of these observations, however, is a remark made by Fodor [21] many years ago: "He states the disease usually asserts itself on a sudden, but only after a certain degree of warmth has continued for a longer or shorter period, 'as if,' he adds, 'the virus in the soil had first to ripen.'"

Turning now to the detailed consideration of the temperature charts, Chart I will be found to provide an ocular demonstration of how the different temperature conditions of the preceding weeks combine to determine the precise date of onset of the seasonal epidemics of diarrhoea and typhoid. Directing attention to the seven years at the top of the chart, which are of the most regular type, it is seen that none of the temperature lines is quite parallel to the D line, indicating that no particular temperature is all-important. They exhibit, however, a kind of concerted effort to together make up the sum of aggregate temperatures required before each epidemic. Thus, where a stay at some temperature is short or does not occur, the stay at others is correspondingly increased; a higher value, it will be noticed, being allowed for higher temperatures. Thus, where the 60° F. line is bent in unusually close to the D line, another line—perhaps the 55° F. line—is bent out, and in this case it would be bent out much more in comparison to the 60° F. line, in order to make up for the lesser potency of such temperatures.

In this way the varying temperature conditions in each year can be examined in detail, and a similar temperature aggregate proved in each case. It will be noticed, however, that satisfactory references may be made in diarrhoea as far back as the 55° F. line, or a little further, but that beyond those limits no such comparable results are obtained, which suggests that those temperatures take little or no part in the matter. As regards the typhoid period, rather different conditions will be found to hold. These will be referred to later on, but attention might be called in passing to the striking parallelism of the 52° F. line to the E line. Again, it might be added by way of further illustration that the temperature aggregate determining the date of harvest might be worked out in a similar way. Indeed, the influence of accumulated temperatures has long been recognized in connexion with the flowering of plants, and phenologists, though their method of measuring it is imperfect, have

demonstrated the general truth of this law. Thus Professor Hoffmann, of Giessen [7], has shown that in one particular season the common lilac bloomed first at Giessen on April 29, but did not flower at Upsala till seven weeks later. In each case, however, the insolation sums were practically the same, viz.: $1,482^{\circ}$ and $1,433^{\circ}$ respectively.

In following out the various observations and arguments of this paper it will be well to attach a very definite entity to the period of accumulated temperatures preceding the main epidemic rise. For want of a better one, the somewhat ill-derived term "præ-epidemic" will be used as referring exclusively to this particular period. It may be held that theoretically no such period exists, nevertheless for practical public health purposes it has a very real existence. The temperature conditions of this period will be finally investigated by a study of Chart II, showing the one-foot earth records for Nottingham. These records are preferred to air temperatures in studying the question of accumulated temperatures because they give a more reliable idea of the amount of heat received, and are more convenient to study on account of their more even course. On all other occasions, however, the air temperatures will be preferred, for they alone keep time with the temperature changes upon the surface of the earth, which situation, I feel convinced, is the one where the effect upon the infective processes of diarrhœa is brought about.

Præ-Epidemic Period.

A glance down Chart II, then, will convey more than many written pages could possibly do. The temperature conditions of the præ-epidemic period for diarrhœa deaths may now be briefly stated. Both earth and air temperatures are used in this chart, but, with due allowance for various factors to be hereafter mentioned, the following may be held to fairly represent those conditions as related to the air temperatures in most English towns:—

(1) From the time when the rising seasonal temperatures at the surface of the earth have exceeded certain limits, influences are set up which go to determine the date of the epidemic outburst of diarrhœa.

(2) These influences can be definitely traced back to the attainment of 55° F., or a slightly lower temperature, and it is possible that some slight influence is produced from a point as low as 50° F. There is no reason, however, for regarding even this temperature as an absolute limit.

(3) The intensity of the influence is in proportion to the height of the temperature: 60° F. is the point at which what may be regarded as

favourable temperatures obtain. Where, as in 1899, at Nottingham, the præ-epidemic period for diarrhoea deaths practically begins and is completed at this temperature, the length of the period is about four or five weeks. Descending below 60° F. the various temperatures become less and less potent and the period becomes proportionately increased in length, till at 55° F. more than ten weeks might elapse before the main rise in diarrhoea deaths occurs. On the other hand, the potency of temperatures appears to increase for some distance above 60° F.

A glance at Chart II will show that in the first six years practically the whole of the præ-epidemic period is contained in an abrupt plateau, where the temperature is maintained at about 60° F. Where this temperature is attained, as in 1899 and 1896, the period extends four weeks. In 1895 the average height of the plateau falls a little below 60° F. and still lower in 1901, the periods in these years being correspondingly increased to five and six weeks respectively. And so we may go on through all the years which are of a fairly regular type. In fact, in such years there is an opportunity of making a rough calculation as to the relative values of the different temperatures. Thus, taking the præ-epidemic periods of 1899, 1895, and 1901, two weeks above 60° F. are common to all three, so that we have left two weeks in 1899 above 60° F., which are equal to three weeks in 1895 at an average of about 59° F., and to four weeks in 1901 at an average of about 58° F. The first group of regular years is set out for a comparison of this kind in Table I, where under each temperature the number of weeks spent at or above that point is given, the roman figure above referring to the number of weeks spent at that particular temperature alone. One week at 59·8° F. and another at 59·9° F. in 1899 and 1901 respectively are taken as 60° F., which they are for all practical purposes. The increasing totals in the 55° F. column of years with fewer and fewer weeks above 60° F. is well shown; 1890 at Nottingham and 1906 at Blackburn pass the whole period well below 60° F., and consequently take nine or ten weeks to complete it.

In order to fix the lowest limits of the præ-epidemic period, Table I may again be referred to. Beside the fact which has already been demonstrated of the diarrhoeal rise sometimes taking place from temperatures as low as 55° F., it is evident in the table that, in order to make up the required aggregate, temperatures at least as low as 55° F. must be included in years where the number of weeks spent at higher temperatures are insufficient. Thus, in the 60° F. column the small totals in the last three years evidently do not balance the totals of those above.

Descending degree by degree, we find that in the 57° F. column totals of four, five, and six weeks are found which mutually balance one against another, except in 1893, where the total is still only three weeks, only one of which was spent above 60° F. Lower limits must therefore be taken to suit such years as this. In the 56° F. column a comparable total is still absent, but in the 55° F. column two more weeks are added, and in each year taken a fairly satisfactory aggregate is obtained. Below 55° F. it will be seen that a rough balance may still be struck almost to 50° F., but at still lower temperatures the totals increase in a very irregular manner, the inference being that these temperatures exert little or no influence upon the præ-epidemic period of diarrhoea. A third fact which possibly yields evidence as to the temperature limits of the period is that, as will be shown later on, epidemic prevalence is commonly brought to an end on the fall of the air temperature below 50° F. Practical demonstrations of the lower limits are frequently found in northerly towns, such as Edinburgh and Glasgow, where, though the diarrhoea curve presents a sharp enough rise in hot years, in the coldest years it is to all intents and purposes a straight line throughout.

So much for the years of a regular type. It will be noticed that they yield a particularly reliable comparison, since not only are they all of one type, having an abrupt rise to the summer level, but it will be seen that they are all the years to be found of that type in the whole eighteen. With regard to the years of irregular type, there are firstly those showing small irregularities. 1906 is such a year, showing continual remissions at weekly intervals (*see* Chart IX). It is possible that these latter are responsible for the unusual length of the præ-epidemic period. The second type of irregular year is the abortive form, where the temperature falls away below 60° F. before the period is complete; 1902, 1903, and 1891 are interesting examples. The rise, which was beginning just before the fall, aborts while the temperature continues to fall, a very low level of diarrhoea being maintained. However, when the fall of temperature ceases, and particularly if a slight rise supervenes, the main rise of diarrhoea becomes established; the higher the temperature the sooner does this happen. Although the one-foot earth temperature for 1902 appeared to be well maintained for four weeks above 60° F., yet the air temperature will be seen to have been falling all the time. It was commonly stated that the rain was the chief cause of the abortive diarrhoea of this year, but a study of the charts on the principles suggested shows that the temperature conditions alone appeared to be a sufficient explanation. In Chart VIII the year 1902 is given, with all

particulars as to rain and temperature. It will repay a close study. In the two weeks of main diarrhoeal rise, the rainfall will be seen to be excessive, but the air temperature was rising. A few weeks before this the rising epidemic was aborted during a week of heavy rainfall, but here the fall of temperature was even more marked than the amount of rain. The fact is that when there is rain there is generally cooling of the air, and less frequently, when there is an onset of cool weather, there is accompanying rain. The London chart makes an interesting comparison in this year. 1907 was another irregular and abortive year; in addition, it was particularly cool. The record lateness of diarrhoea and typhoid are evidence of this. Finally, there are a few years, such as 1904 and 1905, where the period was a week or two longer than it should have been, considering the height of the temperature, and for apparently no obvious reason. This matter will be referred to again.

Turning now to the study of the præ-epidemic period of enteric fever, various reasons will be detailed supporting the assertion that its rise, usually so late in the season, has really something to do with the temperatures of the early summer.

In the first place it must be remembered that in temperate climates enteric fever is really no more an autumn disease than diarrhoea, in the sense that its rise depends in some special way upon autumn conditions. This is merely an illusion resting on the shortness of the English summer. Where, however, the summer temperatures are prolonged for some time, and the necessary period is complete, the typhoid rise is seen to occur from the summer plateau, and sometimes even before the maximum summer temperatures have been reached. In warmer countries, where the thermometer stands for a great many months over 60° F., the typhoid rise will be seen to occur regularly from the summer plateau, and, as a matter of fact, from the examination of a series of English charts alone, it is evident that the general truth is established that the typhoid rise is definitely related not to the autumn decline, but to the beginning of the early summer temperatures. Again, diarrhoea has been taken as a summer disease, simply because its particularly short maturation period has generally placed its rise and prevalence well within our summer time, but in countries that enjoy a still cooler and shorter summer, in elevated towns and in exceptionally short and cool summers with us, the diarrhoea curve is seen to rise from the declining autumn temperatures, and becomes as much an autumn disease as typhoid does under other conditions. Thus, in the case of diarrhoea and typhoid, the

appellation "summer" or "autumn" disease is not an exact term, being founded only on the mere chance of their seasonal position in any given locality. As a matter of fact, it will be found that enteric fever as well as diarrhoea presents a "præ-epidemic period." This period is generally complete in normal years about three or four weeks after that of diarrhoea, and its beginnings can be traced at least as far back as the attainment of 52° F. by the rising summer temperatures.

Before, however, proceeding to a particular demonstration of these facts, a second general but equally decisive argument will be introduced, arising out of the observation of the somewhat longer præ-epidemic period of enteric fever. If the latter fact be indeed true, then, if years of an irregular type be found where the temperatures fall away just as the diarrhoea rise is well established, the final weeks of the typhoid period must be greatly extended and a gap of much more than the ordinary four weeks be made between the typhoid and diarrhoea rise. It is interesting, and strikingly confirmatory of the view already taken, that this is just what takes place in such circumstances (Cf. 1903 and 1890, Chart II).

In the third place, the temperature conditions of the præ-epidemic period will be determined by reference to the charts, and as in the case of diarrhoea they are at once apparent from a glance down Chart II and a brief reference to Chart I and Table II. In Chart II the E line will be found to be often divergent from the D line, and at once it becomes clear that there is an essential difference between the respective periods of the two diseases. For while in diarrhoea the higher the temperature the more potent is it in shortening the length of the period, in typhoid the ideal temperature seems to be one gradually sloping upwards from about 52° F. to 60° F., or more, in the course of about ten weeks. Nothing is apparently gained by a sudden rise to high temperatures at the beginning of the period, as in 1899. In fact, if anything, the process, whatever its nature may be, appears to be hindered in such a case, and a longer period taken. Confirmatory evidence is given in the first four years, which, it should be further noted, bear out the same conclusions both in London and Nottingham. Thus 1899 and 1895, with their abrupt rise at the very beginning, may be compared with 1896 and 1901, each of which possesses a very gradual rise and in which the præ-epidemic periods are even inclined to be shorter than in the former.

To the foregoing, then, may be ascribed the want of true parallelism between the D and E lines, although it has been shown that owing to

their similar temperature limits both diseases should begin to be influenced about the same time. Where, then, the early abrupt rise takes place, while the typhoid is unaffected, the diarrhoea rise is moved forward several weeks.

Another slight difference may be further noticed. In the gradually rising type of year it has been seen that the interval between the two diseases is decreased, owing to the increased lateness of the diarrhoea. In 1898, Nottingham, it is reduced to two weeks, and in 1907, London, the two points practically coincide. From comparison with other years it is seen that this is wholly due to the lateness of the diarrhoea, the typhoid being really not at all late. This year, then, has, by the course of its temperature curve, succeeded in picking out in a most marked manner whatever differences exist between the temperature conditions of their respective periods. We gather from it that the typhoid period can be completed at temperatures slightly lower than those necessary to produce a vigorous rise of diarrhoea.

Table II is constructed in the same way as Table I for diarrhoea. Proceeding in the same way as in that disease, it is found that the totals in the 60° F. column are altogether unequal. Lower temperature columns being then referred to, it is found necessary to descend to 52° F. before the totals in every year are at all comparable. The totals are from nine to eleven, excluding 1900, which falls just before the typhoid rise. There is, it must be noticed, no indication in this disease that higher temperatures have greater potency than lower ones. Below 52° F. the totals become again irregular, and it is probable from this that very little influence is exerted at these temperatures. Turning to the irregular years, it is found that, as in the case of diarrhoea, abortive and low temperatures postpone the seasonal rise. But here we have a curious fact that, although in the first weeks of the period 52° F. was as good as other temperatures, yet, for the completion of the period, temperatures of about 60° F. are unmistakably demanded, otherwise the main rise is considerably delayed. Upon glancing at the irregular years from 1902 downwards on Chart II this fact will be sufficiently appreciated. It will be seen there that the total weeks above 52° F. greatly exceed ten in number.

In comparing the London charts with those of Nottingham it is seen how important a difference of 2° or 3° proves to be in the effect upon the diarrhoea rise when that difference is exhibited in temperatures about 60° F. The London summer is generally 2° or 3° hotter than that of Nottingham, and as a result similar plateaux of temperature

reach 60° F. in London where they would only reach 57° F. at Nottingham. It will be at once understood from this why the diarrhoea is often several weeks earlier in London. It will be seen also from a comparison of the charts that the London data bear out all the conclusions arrived at from a study of the Nottingham charts.

Thus far, then, efforts have been completely confined to a demonstration of a period of accumulated temperatures preceding the main outburst of diarrhoea and typhoid fever. These facts have become apparent from the meteorological charts. The allusions to phenology and to the parallelism of the harvest were simply references to vital records of the very same meteorological facts as inscribed upon the phenomena of the vegetable world. And these amply confirm the conclusions arrived at.

Epidemic Period.

An inquiry will now be initiated into another phenomenon of seasonal prevalence, which may or may not prove to be distinct from that which has been already considered in connexion with the onset of the seasonal rise. It has been observed that in diarrhoea, in the period of established prevalence, the latter varies markedly with the temperature, a dip in the temperature, for example, being followed by a fall in the diarrhoea mortality in the week but one following or in a slightly shorter period. From this it is inferred that the drop is due to the effect of temperature, immediate, or almost so, upon the spread of infection, the interval of a fortnight or ten days being found to accord roughly with the average duration of fatal cases. Further, it will be found that this effect of temperature has also something to do with the ending of the epidemic, for although a considerable fall may have already taken place, the drop to the winter level does not finally take place till the air temperature passes below 50° F. In order to avoid misunderstanding, the term "epidemic period" will be hereafter used to refer definitely to the period from the end of the præ-epidemic period until the date of fall to the winter level.

In following out in detail the effect of temperature upon variation in prevalence, the existence of another and equally important factor must be recognized, which combines with temperature to decide the precise outline of the epidemic curve in any given year. While a study of individual curves, particularly in years where the meteorological conditions are developed in an irregular manner (*e.g.*, 1900 and 1890, Chart V, might lead one to believe that they conform to no law save

that of temperature, yet, if the prevalences of a number of years are combined, there will emerge a curve whose proportions at once suggest some other factor or factors beside that of temperature. Chart VII shows a composite curve of eight years for London, and it will be seen that it closely resembles what we are accustomed to regard as the explosive curve of a typically infectious disease. The outline of such a curve may be held to be determined by an access of, and subsequent regular decrease in, infective virulence of the causative organism of the disease, or to follow the attainment of a high degree of susceptibility of the population, with subsequent exhaustion of the same.

There is not space to deal with the question whether one or both of these factors are responsible. Theoretically they may both be held to exert some influence in the spread of most infectious diseases. It is evident that the prevalence of disease is continually being decided by the balance held between the highness of the infectivity of the organism and the lowness of the resistance offered by the amount of insusceptibility of the population. With respect to their effect in starting and arresting an epidemic, it is evident that their relative values are alone important; and the question as to whether it is the infectivity that is higher or the resistance that is lower than the normal may be avoided by the use of the term "epidemic potential" to denote the head of power for epidemic spread that is so produced. I might, however, mention that I have carried out extensive personal observations upon this disease during the past summer, obtaining the complete records of sickness throughout several small areas. These show that in some localities nearly half the whole population was attacked, including adults and children; that very few second attacks occurred in the same season amongst adults; and that, having swept such an area, a subsequent rise to favourable temperatures evoked little response from the disease, although neighbouring areas were still in the height of their epidemic. A detailed account of these observations will shortly be published, which will be to some extent complementary to the remarks upon diarrhoea in the present paper. Such facts, then, suggest that exhaustion of susceptible persons should receive careful consideration as a cause of epidemic decline. It is interesting to note that these investigations also point to the conclusion, suggested by the characteristic rise and fall of the diarrhoea curve, that the bulk of cases are caused by case-to-case infection, whether direct or indirect.

There is no time to follow out the detailed effect of variations in temperature upon each year's prevalence. A large number of charts

are provided, so that these may be followed up at leisure. The London curves are best suited for this study. It is evident from the charts that the amount of fall of diarrhoea depends upon the point to which a fall of temperature takes place. Further, it becomes obvious from a study of such years as 1895, London, that in any given season, for each degree of temperature above the before-mentioned limits there is, theoretically, a corresponding height at which the diarrhoea prevalence will stand.

The typical features of a diarrhoea curve may best be studied on the composite tracing of a large city. Chart VII is constructed from an average of eight years in London (1897-1901 and 1904-6). The weeks of main onset are made to coincide, as well as the corresponding weeks of temperature. The effect of this is to bring out the rise of the curve as steep as it is possible to get it, and to afford an opportunity of comparing it with the average temperature curve of the corresponding period. It is obvious, on examining the diarrhoea rise, that the increase is not in accordance with ordinary geometrical progression, doubling the number of cases over each unit of time, but as the curve rises there is a constant acceleration of the rate of increase. We may, however, look for an explanation of this in the temperature curve. It will be seen that this has been constantly rising, the influence of temperature on the rate of increase being continually more and more potent. With, however, a constant temperature of a little over 60° F. it is probable, from a study of this and other charts, that the epidemic would have risen in a fairly regular manner, doubling its total every week. It will be noted, however, that at the summit of the curve, though the temperature has already begun to fall, the curve continues to rise notwithstanding. After the summit is passed, however, the reverse conditions hold. The effect of temperature must always, then, be viewed in relation to the tendency of the curve at the particular moment examined, for the rising curve will always show a decided tendency to rise in spite of the temperature, and the falling curve to fall in spite of the temperature. So that to bring a rising curve to a standstill a constantly falling temperature is required, and to keep the falling curve at a given level a constantly rising temperature is necessary. A rising temperature always appears thus to be more potent with a rising epidemic than a falling one; while with a falling temperature the exact converse holds good. Owing to these temperature effects and to their relation to the temperature curve the peak of the fifty years' diarrhoea curve for London is curiously asymmetrical, being too steep on the upward slope and too slanting after the summit is turned.

It will be seen that these facts are not so noticeable in the Nottingham charts, the plateau of summer temperatures as far north as this being generally too short to allow of epidemic exhaustion being very clearly exhibited. In London, however, with the longer stay above 60°F. , it can generally be distinguished in a hot year. In warmer countries, where the summer is very much longer, again, the diarrhoea is often declining weeks or months before midsummer is reached. The curve for Melbourne in Chart VI illustrates this point in a most unmistakable manner. It will be further noticed, in the latter, that although the decline is so comparatively early, it is bolstered up throughout the maintenance of high temperatures, and it does not reach the winter level till a temperature of about 50°F. is reached, as in England. Similar *præ*-epidemic temperature conditions are also evident, the diarrhoea rising when the temperature has averaged 60°F. for about four weeks.

If the curve for a large city be analysed by setting up the curves for each district separately, an interesting example is given as to how the general outline of the large curve is determined. Chart VIII shows the curves for the five districts of Nottingham. It is seen that the epidemic rises and works itself out at different parts of the epidemic season in different districts. Some curves therefore show very marked reactions to notches in the temperature curve, while others appear to show none, from the fact that they are at different stages in the evolution of their epidemic when the check takes place. There is also the unequal development in still smaller foci to be considered. While the variation in point of time in the evolution of different local epidemics may take place largely without respect to relative temperatures, yet in elevated suburbs, such as Bulwell in Nottingham, an examination of a number of years will show a confirmed tendency to lateness, corresponding to the relatively lower summer temperatures, as was to be expected.

Incidentally it may be mentioned that there is also a tendency for the magnitude of the outbreak to vary in different parts of a city in different years. Again, it is apparent in the Nottingham chart that the total prevalence varies from year to year, irrespective of the aggregate of temperature influences. In all these facts, of course, diarrhoea again exhibits the characteristic behaviour of a typical infectious disease.

As regards the promptness, or otherwise, with which the increase of diarrhoea cases reacts to a rise in temperature, the matter might be settled by inquiries as to the dates of attack of fatal cases, referring these to the daily temperature chart. Dr. Niven did this for the year

1899 at Manchester [11] and noticed a reaction following the movement of the air temperature at an interval of one to three days. The date of attack gathered in this way is not always reliable. Again, the effect on the death returns is most pronounced in the week but one following the change in temperature, and often to some extent in the next week. So that if we subtracted the average length of fatal illness, with an allowance of a day or two for registration, from a period of about ten days, the interval elapsing before the reaction would be again arrived at. But no very exact result can be thus obtained. As regards observations upon the dates of attack of non-fatal cases, the reaction to temperature in my own observations appeared to commence at once, only that a gradual increase continued extending over one to four days, so that the main effect would thus be produced about three days later than the rise in temperature. It is probable that in these calculations there is an incubating period of a half to one and a half days to be allowed for.

There will not be space for more than a brief survey of the effect of temperature upon the prevalence of enteric fever. It is evident that in this case the effect of temperature is not so marked, and that there is generally considerable winter prevalence. Nevertheless, when notifications are referred back three weeks to the date of infection, the curve for a large city, such as London, is generally found to show a very sharp drop following on the frosty weather that sets in at the end of November. To what extent this fall is due to temperature and to what extent to mere exhaustion of epidemic potential it is impossible to say; but the important fact about which there cannot be much doubt is that temperatures between 50° F. and 60° F. seem to exert as favourable an influence upon prevalence as those above 60° F. A reference has already been made to the necessity of a rise to 60° F. towards the close of the epidemic period. After the epidemic, however, has been, so to speak, given this send-off, temperatures under 60° F. appear to be just as favourable, if not more so, as those above. Indeed, the ideal season seems to be one where the temperature in the *præ*-epidemic period rises to a fairly high level and then slips into the fifties as soon as the epidemic is established; 1899 at London was such a year. Following on the unusually hot summer during which the typhoid appeared to be inhibited, a sharp fall beneath 60° F. occurred, but a plateau of about 50° F. was maintained up to the middle of November, the result being a record season for typhoid. The typhoid curve for Shanghai (Chart IX) seems to illustrate this point. The returns are, however, only drawn from a very small white population.

As regards the extent of observations upon the temperature conditions of the two diseases in other towns, from ten to thirty of the largest towns in England, Scotland, and Ireland have been examined for each of the last ten years. Glimpses of European and American statistics have also been obtained, and have left little doubt in my mind as to the existence of the temperature conditions set forth in this paper, particularly in the case of diarrhoea. While comparing the returns of any given city, one year with another, has always given satisfactory results, comparing the dates of onset of different towns in the same year, along with their respective temperatures, is not so satisfactory, for various reasons, and on that account I could not include examples of such in the charts without elaborate explanations. Firstly, there is the correctness of the temperature records to be considered, then the height of the observing station above the heart of the city, where most of the diarrhoea occurs. Thus, although the behaviour of disease in both Nottingham and Sheffield conforms remarkably to well-marked standards in each place, those standards vary considerably when the respective temperature records are compared. Thus, 58° F. at Sheffield is equivalent to 60° F. at Nottingham in its effect upon diarrhoea, as judged by the one-foot earth temperature. This difference, then, makes comparison difficult, when it is remembered what a difference 2° about the temperature of 60° F. makes upon the præ-epidemic period of diarrhoea. At Liverpool either the diarrhoea is always too early or the temperature is too low compared with other towns. The latter is probably the correct solution, as the observations, I understand, are taken 202 ft. above the sea, whereas the bulk of diarrhoea occurs almost at sea-level. It will be a sufficient test of the principles laid down if each town conforms to its own temperature standards. Again, the dates of onset of the smaller towns are not reliable tests, for if the deaths are irregularly distributed amongst the attacks there is a possibility of a very large error.

It must be remembered that the death returns of diarrhoea are very unreliable indications of the extent and distribution of the disease. It is surprising that even so much can be done with them; they are not very safe guides outside the statistics of large cities. It must be remembered that the deaths are almost confined to infants under 1 year of age, who only form a small part of the total number of persons attacked. Many of these infants die more from other affections than from the direct effect of the diarrhoea. Moreover, there are probably a hundred non-fatal cases of diarrhoea to every death.

Again, there is the fact that diarrhoea is an infectious disease, with

all the uncertainties and vagaries that accompany the movements of such a phenomenon. The unequal development in the districts of a large city has been shown, and the same irregularity holds true in small towns. The various reactions to temperature are not conveniently studied in towns with a population of fewer than 100,000. Irregularities are also visible in the Nottingham and London charts. Thus, in 1894, the diarrhœa is rather early in the latter. In such a case it must be remembered that diarrhœa, just as typhoid, might happen to be loosed too early, and, at once taking advantage of favourable temperatures, quickly spread. These few exceptions cannot detract from the truth of the general law which is backed up by such a mass of evidence. They serve to show, however, that there is no reason to hastily conclude that temperature is the only factor.

PART II.

An effort has been made so far to avoid all the theoretical aspects of the question, and simply to set forth, as clearly as possible, what appear to be the bare facts of the relation between temperature conditions and the prevalence of disease. The interpretation of these facts must, however, always remain by far the more important consideration, at least from a practical standpoint. In such an inquiry there is no more approved method of advance than the setting up and subsequent demolition of various provisional theories. In addition to different theories, however, there are a large number of facts still to be introduced. In the first place, some adequate explanation must be sought of the phenomena of the præ-epidemic period.

Præ-Epidemic Period.

As regards this period there is one explanation which must inevitably be the first to present itself, in accordance with the present tendency of epidemiological research. The belief is fast gaining ground that most infectious diseases are propagated by a continuity of case-to-case infection, and that the summer epidemic of such a disease as diarrhœa has its origin in the few cases which contrive to keep alive the infection throughout the winter. In this case the latent period preceding the outburst would represent the interval elapsing before the steady increase from the winter cases reached what might be considered to be epidemic proportions. It will not be contended that

such a theory will not eventually prove to be the correct one in the case of diarrhoea, and the various arguments for and against must each come up for careful consideration. In the first place, it will be recalled that the temperature conditions of the two periods considered were really very similar; so much so, indeed, that it might easily be held that there is but the one phenomenon—the response of the disease to the energizing influences of temperature. This seems the more probable from the fact that not only are the temperature conditions of each period closely allied in diarrhoea, but they also closely resemble each other in typhoid fever, though differing wholly again from those of diarrhoea. Incidentally it will appear here that, whether the phenomena of the præ-epidemic period are distinct from or identical with those of the epidemic period, the arbitrary marking off of the first period and demonstration of its temperature conditions was a justifiable and useful procedure.

It will be remembered again that the first temperatures which were definitely found to exert some influence upon the date of rise of diarrhoea were those about 55° F., while the limiting temperature of the falling epidemic was usually about 50° F. It might be suggested that this is merely the result of a kind of inertia, the true limiting temperature being intermediate. Thus, if 52° F. represent the latter, it might happen at the end of the epidemic, when, owing to the large amount of infection on every hand, the chance of infection occurring is above the average, that the epidemic would be continued till 50° F. is reached. On the other hand, at the beginning of the season, though 52° F. might be sufficient to maintain a certain low rate of infection, yet in order to set up an increase in cases a temperature of two or three more degrees might be required. It might further be held that the pause of a whole fortnight or more can also thus be referred to a purely mechanical effect. As regards the 50° F. limit, the presence of a small proportion of chronic cases tends to postpone the fall of the curve.

Lastly, there are the essentially gradual nature of the rise itself, as indicated in the returns of a very large city, and the doubt as to the actual existence of the interval referred to still remaining to be dealt with. Hitherto no notice has been taken of the minor and abortive rises that sometimes precede the main one. It might be contended, however, that they are in reality the small increases out of which the main epidemics arise.

The exact nature of the rise from the winter level may be studied in Chart VII, where the huge death totals this curve represents should

give some reliable idea of the real nature of the rise in actual cases. In this chart, it must be noted, the diarrhoea returns have been brought forward two weeks to show the correspondence with temperature. Two other London curves are given in Chart VI, representing composite curves of two and three years each, selected from those forming the eight years curve. They group the years' showing the most level temperatures during the præ-epidemic period. If this period be now examined in the three composite curves it is evident that, whether it be two weeks at about 60° F. in the highest, or four or five weeks in the lowest, there is still the apparently quiescent period before any definite rise takes place, varying in length according to the temperature. This is also confirmed by years of still more opposite type, *e.g.*, 1899 at Nottingham and 1906 at Blackburn, already alluded to. Lastly, there is the evidence of the abrupt rise in actual cases obtained in my own investigations.

It is true, however, that some slight rise is to be observed for several weeks previous in the winter cases, and this will require some adequate explanation. As regards these winter cases, it might be inferred from the statements of various observers that there is no difference between those fatal cases occurring at that season and those of the epidemic period. A reference to one London year, however, has shown that there is at least a considerable difference in age-incidence; for while from the beginning of the epidemic period the deaths under 1 year of age increased up to thirty times the weekly winter rate, the deaths of those aged over 40 only increased four or five times. If, again, a composite London curve be examined, it will be found that the winter cases do not continue at one and the same level, but present a constant tendency to decrease to a minimum about the mid-week of April, with a subsequent gentle rise from that date. It is a point of perhaps some significance that the winter levels for typhoid and scarlet fever reach a minimum in a similar way about the same week in April, although the lowest point of the temperature is reached in January. It is true that this gradual rise becomes correspondingly more marked as the temperature rises above 50° F., but it is still very gradual, and must be considered as being due to the relative effect of variations in temperature on winter cases, and altogether apart from the true seasonal increase; for at the point where the period of quiescence terminates the rate of increase is abruptly altered, although the temperature may remain at the same level. After this abrupt change of rate, however, with equal temperature conditions, the curve appears to ascend at a

very constant rate of increase, which again suggests the continuity of case-to-case infection if the difficulty at the point of rise could be explained away. It is not impossible, indeed, that this might be done, if we attribute the summer cases to a few only of the winter ones. In that case the epidemic curve could easily be referred back two weeks before the junction with the London winter level of twenty cases to a few fatal cases, which would yet represent many hundred foci. For, as far as my own observations have gone, considerably more than 100 non-fatal cases were found to every fatal one. This point, then, is one which must be left for further research.

If with some difficulty these explanations be held to account for the *præ*-epidemic period of diarrhoea, the interpretation of the same phenomenon in enteric fever will be found to be still more difficult. This follows from the greater length of the period and the fact that in spite of this the rises are often more abrupt; whereas, if derived from the winter level the slower rate of increase should make the presence of a gradual rise more evident. Further than this, it will be seen in many of the Nottingham years where there can be no doubt about the date of rise that during the ten weeks or more of the period of accumulated temperatures the winter level continues to fall, as if, failing the seasonal rise, it would within six months or a year reach the base-line, and the prevalence of the disease completely cease. Such behaviour of the typhoid curve might indeed be held to show that there is very little necessary connexion between the mass of infection which contributes to the dimensions of the seasonal rise and the winter cases of the weeks immediately preceding, the winter cases apparently pursuing their declining course, the foci of the seasonal rise being mostly new as regards their high degree of infectivity.

Particularly abrupt illustrations of the typhoid rise are given in the Manchester Health Reports, where the weekly notifications have been referred back to the dates of attack. For the five very abrupt rises in the Nottingham charts there are eight even more abrupt rises in these Manchester figures for the corresponding period; the rise in 1906 being most remarkable, the weekly attacks about that time reading: 2, 6, 4, 4, 13, 20, 19. On the other hand, it must be noticed that in about half of the years at Nottingham there is a minor rise preceding the main one by several weeks (*e.g.*, 1899, Chart II). This almost invariably does not appear till half of the period of accumulated temperature has already gone by. It must, then, for the time remain a question as to which should be regarded as the typical method of seasonal rise

—the abrupt or the gradual one. Appearances from the charts are perhaps in favour of a modified variety of the latter.

The question of continuity of case-to-case infection will now be left for the consideration of an explanatory theory of a widely different kind. It has just been suggested that in enteric fever appearances are on the whole in favour of the introduction of a large mass of infection, which is something superadded to, and not necessarily connected with, the preceding diminishing numbers of winter cases. It has also been shown that this introduction occurs at the end of, and is dependent upon, a period of about ten weeks of certain accumulated temperatures. If it can hereafter be demonstrated that there is continuity and a gradual increase of case-to-case infection throughout this period, and that there is also no change in infectivity or virulence apart from the direct effect of temperature or animal carriers, the phenomenon may be considered as explained. If, on the other hand, it can be shown to be unlikely that such continuity of infection and uniformity of infective virulence obtains, then we are at a loss to explain the lengthy period of accumulated temperatures. Above all, it is difficult to see how these temperature influences could get at the organism within the body, where the temperatures are notably high and constant. There remains, then, only the alternative explanation that during the whole of this period the organisms which produced the seasonal rise were *existing without the body* in a purely saprophytic state. If this be so, the disconnexion of the true seasonal cases from the winter cases of the immediately preceding weeks was not clear in such a gradually rising year as 1899, Nottingham, because the more gradual introduction of the former veiled the actual trend of the winter cases towards a still lower level. In 1900, however, the delayed and abrupt introduction of the seasonal cases laid bare this tendency to the uttermost week. Such a theory, then, presupposes the existence of two alternating phases in the life of the pathogenic organism—an active phase, within the animal body, where it is continually passing from case to case; and a resting phase, as a saprophyte.

It will be later shown that in several other diseases besides enteric fever there is evidence that when the seasonal rise is past, the level of cases shows a constant tendency to fall until checked by the next seasonal rise, as if the very existence of the disease depended on the next renewal of its forces from the seasonal additions. When we reflect upon the possible origin of bacterial life from the vegetable world; upon the fact, of which we can now have no doubt, that its stay within the animal tissues can

only be maintained by constant battle; it is not difficult to believe that continued passage from case to case must ultimately exhaust its forces and necessitate a return to its true home in the saprophytic world, there to renew its depleted energies.

If, then, it be allowed that such a rest is necessary, we are at once provided with a satisfactory explanation of the phenomenon of endemicity. A disease, then, would be considered endemic where the external conditions of climate and human surroundings are so favourable to the saprophytic phase of the organism as to allow of a seasonal supply from that source which will be sufficient to maintain the ordinary prevalence of the disease. Thus cholera appears incapable of sufficiently renewing itself outside a few limited endemic areas in the East. Having attained, however, a high grade of infectivity, it can become epidemic and extend into colder countries. Here, notwithstanding the fact that yearly renewals appear to some extent to occur, these become less and less, and the disease eventually disappears. Even diphtheria has been shown by Dr. Newsholme [13] to prefer its Continental home, and to maintain itself only with some difficulty under the influences of this insular climate.

As regards the situation of these organisms in their saprophytic state, it is evident that, being unencumbered by a belief in the necessary connexion of deep earth temperatures, we should first seek them in those situations where organisms are most often found to lie. That is generally found to be rather on the surface of things than in their depths. Thus they are most common in the dust that lies upon the ground and decrease rapidly in the depths of the soil. The question of origin from dust or soil can, however, be avoided by using the term "ground infection" to include infection from organisms *on* the ground or *in* the ground; and, indeed, it might be used, for want of a better, to apply to all infection derived from organisms which have for some time existed in a saprophytic state. For whether they lie upon objects at the surface of the earth or at the bottom of the sea, it is for all practical purposes on the ground. All infection of the commoner diseases may, then, be conveniently divided into two classes, ground infection and personal infection, whether direct or indirect, in each case. As regards temperature influences, it is evident that the organisms, while being in intimate relation with air temperatures, will nevertheless share to some extent the temperatures of the solid objects on which they lie; hence the records of a very shallow earth temperature, particularly as it also gives some idea of the amount of heat received, might still be useful.

The renewal of the ranks of the ground organisms would no doubt be brought about by the vast number of bacteria continually being shed or voided from the patient. It is not, however, suggested that the dust is everywhere loaded with every variety of living pathogenic organisms, for there must be enormous destruction continually going on. But it is probably only when they chance to be deposited in some nook or corner, where specially suitable conditions obtain, that they can successfully carry out their saprophytic phase. The assertion that dust in a general way is a cause of diarrhoea is too loose a one to receive general acceptance. In my own observations I found that the progress of the disease was subject to various limitations and restrictions, to which the dust was not subject, and that the liability of infection depended not so much upon the existence of dust or dirt in a house as upon the fact that it lay in the path of infection. In the case of enteric fever, Dr. Boobyer's spot maps of Nottingham [2] show that the seasonal rise is accompanied by the appearance of numerous foci of infection, not occurring in a general manner, but being more marked in some districts in one season than in another.

Further evidence will now be introduced which appears to lend support to the theory under discussion. Certain evidence, for example, might be deduced that such a theory is necessary to explain the occurrence of various phenomena in the seasonal curve of both scarlet fever and diphtheria, as well as enteric fever and diarrhoea. If the outline of the composite London curve for each disease is examined, it will be found that two distinct slopes are more or less definitely made out in the upward rise. There is the level of the epidemic season, which tends to show the gradual rise already referred to from the middle of April. At the point, however, where the seasonal epidemic begins, there is an abrupt change of direction, forming another slope which makes a very definite angle with the former. It may be held from this that the seasonal rise is to some extent different from, and independent of, the inter-seasonal level. In other words, the yearly curve might be held to be compounded of two curves—one representing the introduction of the ground infection and the other the course of continuous personal infection; and it might be further contended that the comparative height of the inter-epidemic level in two diseases such as scarlet fever and diphtheria gives some idea of the comparative amounts of personal infection and ground infection in the two diseases, or at least of the length of time the infective virulence of the respective organisms persists.

Since making these observations I have found that Dr. Brownlee [4],

in testing the fit of various theoretical curves upon the curves of different infectious diseases, has traversed a similar field of inquiry. He made the observation that, on the assumption that there were two factors in action, the one deciding what he terms the endemic level and the other determining the seasonal curve as something superadded to the former, a very good fit was obtained. But on the assumption that the intermediate level was simply the hollow formed by the tail of one epidemic running into the beginnings of another, no satisfactory fit could be brought about. In this paper he also states his conviction that the form of the epidemic curves of most diseases, including diarrhoea and enteric fever, indicate exhaustion of infectivity rather than of susceptibility of the population.

Although no definite evidence is afforded by scarlet fever and diphtheria as to the presence of a period of accumulated temperatures, they nevertheless present a second peculiarity which may be held to be further evidence of the access during the seasonal rise of a distinctly different type of infective material. Sir Shirley Murphy [10] has published the case mortality of scarlet fever, corrected as regards age and sex, for several years in London, and has shown that it is least when the epidemic is highest, and vice versa. If now an inverted curve representing the fatality of scarlet fever be placed above the curve of prevalence, it will be found that onwards from the first rise in May the diminution in fatality is in very close proportion to the height the epidemic curve stands above the inter-epidemic level of cases, as if the fall in fatality was in correspondence with the dilution of the personal infection cases with the somewhat less deadly organisms derived from ground infection. This can again be followed out in detail in each year in the charts accompanying the Annual Reports for London, both in the case of scarlet fever and diphtheria.

If it be held that the lengthy period of accumulated temperatures preceding the typhoid rise must be regarded as referring to a preparatory process amongst organisms existing in a saprophytic state, it may be explained that this process consists either in a maturing of a high degree of infective virulence or in mere multiplication of organisms. With regard to the former it may be held that in typhoid fever this is a process extending over several weeks, and that as a rule the high degree of infectivity is generally attained rather suddenly at the end of this time, the disease bursting forth on every hand, just as a garden plot which has quietly developed its leaves and buds suddenly bursts forth into flower. Such an idea is not altogether founded on pure fancy; it

is a serious attempt to explain the remarkable character of the præ-epidemic period of enteric fever. It has already been shown that a period of about ten weeks is necessary, but that a temperature of 52° F. or a little over is as favourable as any other during the first few weeks. Again, temperatures of 60° F. were found to be demanded at the latter end. A temperature sloping gradually up from 52° F. to 60° F. appeared to be most favourable. Apparently there are, then, at least two phases in the period. A period, however, which can be shown to be divided into stages may for all practical purposes be considered a process. And it seems very possible that the whole phenomenon is a specific process, requiring certain temperatures, rising in a certain manner. Whether accidental or not, the comparison with the successively higher temperature conditions required for the consecutive stages of growth and maturity of plant life in the Temperate Zone is remarkable. The fact that the seasonal rise is perhaps more often gradual than abrupt is not a decisive argument against a theory of the epidemic properties being suddenly developed at the end of a period of quiescence. The various situations in which the organisms would lie might easily vary by several degrees of temperature. And thus a curve could be produced of very similar form to the diarrhoea curve, not reaching its acme for several weeks, where the infection was wholly of extra-corporeal origin. Again, there are many favoured spots in a manufacturing town where the germs might, even in winter, be exposed to the requisite temperature influences, and so the winter cases might also be explained without reference to direct personal infection.

Evidence has already been produced in scarlet fever and diphtheria as to the possible introduction into the prevalence, at the time of the seasonal rise, of a mass of material of a high grade of infectivity. It may be remarked that there are many practical observations that will fit in with such a theory. It is a matter of everyday experience with those who have investigated at first hand the origin of cases of scarlet fever, diphtheria, or enteric fever that, particularly at the beginning of the seasonal rise, absolutely no connexion can be traced between many cases occurring at this time and those of preceding weeks. There was apparently never a stronger tendency than at present to attribute all cases of infective disease to more or less direct personal infection from previous sufferers, nor more justification for so doing in view of the remarkable evidence lately brought forward as to human and animal carriers, but before such a theory is generally adopted the great mass of these untraced cases surely demand some further explanation. The importance of carrier

cases in such a disease as diphtheria must be admitted, and the fact that for every case of diphtheria occurring in a school there are generally to be found two or three cases at least of bacillus carriers in apparent health. Nevertheless, it seems difficult to accept these cases as a complete explanation of all, and this opinion has not been formed without giving the matter frequent severe practical tests. Thus, with an exaggerated idea of their importance, two years ago at Nottingham I followed up, for a considerable time, the possibility of rooting out the disease in an isolated district of about 50,000 population, inquiring into each case and taking swabs. To this was added the experience gained by examining the throats of several thousand school children and adults. It was discouraging to find in how large a number of cases a personal source of infection could not be traced.

There is again the curiously sporadic nature of the cases at the beginning of the season. In the usual inquiries into infectious cases at Mansfield, whose isolated position and limited population of about 33,000 should render the inquiry into the origin of infectious cases a particularly hopeful one, an excellent illustration of this sporadic feature was afforded this autumn. After a period of two months, in which only one doubtful attack was notified, a sudden outburst of scarlet fever occurred, five cases occurring within a period of as many days and in as many distinct parts of the town. No connexion could be traced as far as previous cases, milk supply, schools, or family associations were concerned. There had been, however, a sudden drop from summer temperatures, with rainy weather, and a certain prevalence of "colds" and sore throat appeared at exactly the same time as the scarlet fever. Whether the fact of the two being together was accidental or not need not be considered, but such occurrences sometimes suggest an influence of a symbiotic nature between organisms of a different kind.

The cases were all unusually infectious and of a mild type. This incidentally recalls the fact that, in the London statistics as to case mortality of scarlet fever, the fall in fatality was more marked in proportion to the amount of increase in cases during May, the first month of rise, than throughout the remaining epidemic months. One practical outcome of a theory of ground infection is that such a phenomenon as school influence might be explained to be due, not altogether to the mere association of the children, but partly to the building in which they are gathered. Further practical support is given to the theory by the observations on enteric fever contained in the Manchester Report for 1905. The largest number of cases not traced

to personal infection or other definite causes occurred in the third quarter, that in which the seasonal epidemic arose. These cases numbered about 30 per cent., as against about 22 per cent. in the first and second quarters.

The alternative theory of ground infection referred to the multiplication of organisms outside the body. It might be suggested that a certain period elapses before the requisite mass of organisms are present to produce infection, and, multiplying by a process of geometrical progression, they would in this case certainly have the appearance of inert agencies suddenly springing into life at the end of the period. Again, a further extension of this theory might hold out the possibility that the function of multiplication exhibits a yearly outburst following the winter's rest, the function, however, subsequently becoming exhausted; the form of the curve of infected human cases being simply an incidental reflection of what is occurring on a grand scale in the saprophytic world.

As regards the existence of the *Bacillus typhosus* under perfectly natural conditions in the soil, two particularly interesting observations may be quoted. Dr. Robertson, of Birmingham [14], in 1896 and 1897 grew typhoid bacilli in the soil of a field from one season to another. The important part of the investigation, as far as the subject of this paper is concerned, was the fact that the bacilli, though easily found in the summer following, were not discoverable after the onset of frosty weather in the intervening winter. The tests for the bacillus, which were apparently applied in the same manner on each occasion, included "its reaction with typhoid serum of known strength." From the fact that the organisms were found to gradually grow upward to the surface the question might be raised as to whether the præ-epidemic period is not partly occupied by such an occurrence in the natural history of the disease. The other observation was made by Mr. Spear [18] in an inquiry into outbreaks of enteric fever at Mountain Ash, and communicated by him to the Epidemiological Society. He very satisfactorily proved that the infection was derived from leakage from the soil into certain water mains, and was apparently continuous for two seasons; nevertheless, the disease was practically absent in the intervening winter, but produced an epidemic towards the end of each summer, of the typical form, the outbreak being one month earlier in one season than the other, apparently in correspondence with the prevailing meteorological conditions. Assuming that the intermission of the water supply was not responsible for this latter occurrence, it is difficult to see what explanation can be offered in this and

Dr. Robertson's observation of the apparent inertness, whatever its nature may be, of the typhoid bacillus in the winter time. Further observations must test whether the above occurrences were mere coincidences or not. In the meantime, however, another possible explanation is added to the foregoing, to the effect that during the period above referred to the accumulated temperatures act by arousing the organism from a certain state of passivity to one of renewed activity. Finally, it may be noted that the occurrence of such an inert state in the ground organism is no argument against the occurrence of epidemics in the winter time. These may be caused by the direct passage of infective matter into a water supply or on to oyster-beds, from thence being promptly conveyed to the human consumer. These cases may still be placed under the heading of indirect personal infection, since it may be held that the infective material has not been exposed long enough to external temperature conditions to be greatly altered. As a matter of fact, outbreaks during the winter and spring are almost invariably traced to such special causes, and they may therefore be described as "accidental" as opposed to that of the true seasonal rise. It follows from this that the nature of an outbreak can be tested by seeking whether the necessary period of accumulated temperatures preceded it. Applying this test to London returns, it was stated in a draft of this paper, presented for acceptance in February last, that the outbreaks recorded on the London charts in May, 1895, and June, 1902, would be classed as "accidental." Dr. Hamer kindly informed me subsequently that this was actually found to be so, infection of milk and cockles being demonstrated as the respective causes. As regards the possibility of the specific bacilli being subjected to the seasonal temperature conditions as they lie on the submerged oyster-beds, it appears from records of sea temperatures, which I owe to the courtesy of Dr. Forbes of Brighton, that in an ordinary summer the sea water in that locality commonly exceeds 60° F. The writer also found it above 60° F. upon the Lincolnshire coast.

It is possible that where the winter level is high, with irregular outbreaks constantly marked upon its surface, the extraordinary sources of infection are there more than ordinarily abundant. Portsmouth has a curve of this kind. Edinburgh again and again presents a curious double rise, the last one of which appears to be the true seasonal one, and the first, generally occurring about May, is usually falling when the former occurs. In other towns, though the seasonal rise is particularly great, the winter curve is fairly "clean."

The analysis of the curve of any particular town, though interesting, is still very complex. Shellfish seem to be a cause of rise in winter, as also in the epidemic season, but that they are no necessary cause of the latter is evidenced by the fact that typhoid presents its seasonal characteristics far inland, in new countries, where oysters are not obtainable. It might be mentioned that there is no suggestion that the bulk of cases entering into the seasonal rise derive their infection from without the body. It is possible that though a few cases of this kind might be originally necessary, yet the bulk of subsequent cases might arise by personal infection, as the result of the high infectivity which the former possess.

A detailed study of the factors which determine the prevalence of enteric fever has been for several years undertaken by Dr. Niven, the results of which, as published in his Annual Reports, are exceedingly valuable. In his latest Report he discusses all the possible factors: the mussel season, personal infection, missed cases, and fly carriers. He adds that "the abrupt rise in a particular week is difficult to explain, and, in fact, cannot be said to be explained." He is inclined, however, to look for an explanation to "individual cases in the period immediately preceding" and to "the intensification of local infective processes." It is possible that success will eventually attend the effort to trace continuity of infection, but there still remains this intensification of infective processes to clear up. The question is, Can changes in infectivity definitely related to a long period of meteorological influences take place within the body? Until this difficulty is cleared up, there is still plenty of room for a theory of infection from external sources.

There remains yet another possible explanation of the seasonal renewals within the organism. It is the only other way in which there seems any likelihood of explaining the correspondence with external meteorological conditions. The flora of throat and intestine contains large numbers of saprophytes. Again, the flora of the throat is known to exhibit seasonal changes [3]. It is possible, then, that by a kind of symbiotic action the seasonal influences, in working their effect upon the true saprophytes, might indirectly exert an action upon pathogenic organisms.

The influence of rainfall upon the total prevalence of diarrhoea and enteric fever has not been examined. Its effect, however, upon the date of the seasonal rise has been studied, but no clear deterrent effect has been made out, except in so far as the rain is accompanied by a fall of temperature. On the other hand, diarrhoea has been seen to rise against

a heavy rainfall. The greater irregularities of the *præ*-epidemic period and the frequent occurrence of those very abrupt seasonal rises in enteric fever suggest that it is more subject than diarrhoea to changes in external conditions, one of which may be rainfall.

Fruit and heat as specific causes of diarrhoea may be dismissed. The latter could hardly be urged where the epidemic has a late autumn incidence.

Finally, there is the subject of geographical distribution of disease and the question of endemic areas. All this, as has been submitted before, is difficult to explain apart from some influence upon the organism in its saprophytic state, a phase which, in the case of tetanus and anthrax at least, is admitted without question.

Epidemic Period.

The next subject for consideration will be the effect of temperature in producing variations in, and limitations of, the epidemic prevalence. This effect was found to be very marked in diarrhoea, and some reasonable explanation of this phenomenon will now be sought. The suggestion might be incidentally borne in mind that it might be one and the same effect of temperature which was also responsible for the characteristic rise of the disease towards the end of the *præ*-epidemic period.

In what way, then, does the temperature exert its influence? There are only two ways in which there seems at present any likelihood of this being explained. Either it is the effect upon some carrier of the disease or it is by direct effect upon the infective quality of the hypothetical organism, in the short but usually definite period in which it is exposed to the air in transit from person to person. The carrier most under suspicion at present is the fly, and the influence of flies—a matter on which I have myself made some practical observations—might here be briefly referred to.

Dr. Niven [12] has since 1904 done pioneer work upon the subject, and Dr. Hamer [6], in 1907, also conducted a special investigation into the relation of flies, diarrhoea, and temperature. We are thus provided with four charts illustrating the observations in Manchester and London. The first impression gained from a study of these is that there is in a general way a very close relationship between the prevalence of flies and diarrhoea. It would appear, however, as far as these charts go, that owing to the relative conditions that determine their prevalence the diarrhoea curve will always fall within the period of fly prevalence. Thus, in accordance with the natural history of the

two, it will be impossible by observations and charts of this kind to show that epidemic diarrhoea can be propagated without the help of flies. It may be that diarrhoea cannot become the wholesale scourge it does without the help of the fly, any more than plague in its bubonic type without the wholesale inoculations of the flea. This point, however, must be settled by recourse to other methods. As regards the general correspondence between the two curves, however close this may appear after the prevalence of both flies and diarrhoea has become established, the evidence of 1906 at Manchester seems to make it doubtful that the rise in diarrhoea has an exact relation in point of time to the rise in flies. For while the respective rises in 1904 and 1905 appeared to almost coincide, in 1906 the diarrhoea follows at an interval of about three weeks. There is not necessarily a difficulty in the fact that the onset of diarrhoea lags behind the flies, owing to the fact that the flies can only act as carriers where there is something to carry, and a little time may be required for the dissemination of infection; but this tendency to lag behind should be equally displayed in every year, otherwise it must be concluded that though both appear as the result of temperature, there is no causative connexion between them, but that there is one set of temperature conditions for flies and another for diarrhoea, 1906 succeeding in bringing out this difference just as certain years brought out the different temperature conditions preceding the diarrhoea and typhoid epidemics.

Another difficulty, again, lies in the fact that the reaction of the fly prevalence to temperature tends to be distinctly later than that of diarrhoea, as pointed out by Dr. Hamer. A delay of about a week is noticed in each of the four charts in the fall from the point of greatest prevalence. This might be attributed to exhaustion of epidemic potential, just as the tendency of the diarrhoea, when once established, to outstrip the rise of the fly curve might be attributed to a well-known characteristic of the rising prevalence. But, again at the early period of the epidemic, I find that there is also evidence that the diarrhoea reacts to temperature a week earlier in point of time than the flies, *e.g.*, the rise of diarrhoea deaths in the London chart referred to the week ending August 10, and the rise in fatal cases of the week ending July 30, 1904, at Manchester. However, a great deal more work must be done on this subject before the matter can be finally settled. Unfortunately, in most of the charts observations were commenced too late or stopped too soon for the earliest and latest relations of the fly and diarrhoea curves to be studied. The balance of epidemiological evidence seems so far

unfavourable to the fly theory, but it is a question if the principle of testing the matter by the curve representing the number of flies caught within buildings is a sound one. Direct personal infection, which undoubtedly exists, may explain the passage of infection through a household; but it may be that flies, by virtue of their constant journeyings to and fro, are chiefly responsible for the wide and rapid spread of diarrhoea throughout a district. In this case the movements of the flies may be more important than their numbers. Now, while the effect of change of temperature, which appears to be instantaneous upon the amount of diarrhoea infection, is also instantaneous upon the amount of fly movement, it would hardly be asserted that it instantaneously produces a corresponding number of deaths or births of flies. In the case of either of the latter, there is probably a period of several days before the effect in those ways is evident, and so the variations in the actual number of flies may easily lag a week behind the variations in diarrhoea incidence if the latter follows immediately upon the changes in temperature. It is a matter of common observation that with a fall of temperature on a summer's day to 54° F. or below, the flies which have been busily travelling between the kitchens of neighbouring houses suddenly disappear from the open, many, no doubt, seeking the warmth of the dwellings. For the time being travelling gives place to crawling, and the possible power for scattering infection is thus enormously reduced. They may still, notwithstanding this, keep up the death returns for some days upon the fly-papers. It is evident, then, that this possibility also needs further investigation. Finally, however, it may be stated, with the example of plague before us, that the extraordinary correspondence of diarrhoea with temperature is so suggestive of an insect carrier, and the temperatures 50° F. to 54° F., which limit firstly the movements and secondly the increase and decrease of flies, so closely correspond with those temperatures which affect firstly the variations and secondly the epidemic existence of diarrhoea, that the whole question merits the most thorough and laborious investigation.

As regards the possibility of the effect of temperature being produced in any other way than through its influence on fly prevalence, there is a good deal of evidence that direct personal infection frequently takes place without the necessary assistance of flies. But it has been shown that the temperature keeps a tight hold of all the variations in prevalence, and finally decides exactly when the epidemic shall be depressed to the winter level. If, then, personal infection takes places to any extent, it must also be under the direct influence of temperature, and we must

either conclude that in practically every case occurring during the seasonal epidemic the infection was fly-borne, or allow that temperature can directly influence the degree of infectivity of the hypothetical organism in its passage from case to case—a possibility of which at present bacteriologists take no cognizance. That such a theory is not altogether unjustifiable, and that there is plenty of room for it amongst our epidemiological beliefs, is apparent from a comprehensive study of the seasonal variations which are common to most infectious diseases. The relation of these variations to temperature I am at present investigating with regard to cholera, scarlet fever, diphtheria, measles, Malta fever, and others. These observations are not complete, but in general terms it may be stated that all these diseases display the limiting effects of either high or low temperatures, or sometimes both, upon the degree of their prevalence. In some the heat of summer, in others the frosty weather, make a notch in the curve of prevalence. Cholera responds to the effect of temperature in an even more delicate manner than plague, and fits its epidemic into the annual temperature curve in three different positions, corresponding to the three distinct temperature types of Bengal, the North-West Provinces of India, and of Europe. It is possible, then, that the pursuit of insect-carriers is being pushed too far if it is attempted to seek in them alone the explanation of the yearly variations of cholera, scarlet fever, measles, and many other diseases. Allowing, however, a slight but definite influence of temperature upon the infective process, we have an explanation capable of very general application.

No distinction seems yet to be generally recognized between infectivity and lethal virulence, yet evidence is continually being given by scarlet fever, measles, and other diseases that these two properties may vary quite independently of one another. It would be interesting to have a table prepared of the different specific organisms, enumerating their various vital functions of viability, assimilation, multiplication, toxin formation, &c., and showing the temperature limits of each. Each function would have its upper and lower zero points, and the intermediate optimum temperature, which would differ widely in the different cases. It is possible that infectivity is a function dependent on some ferment action apart from the manufacture of lethal toxins, and that it also has its three temperature points; these being, perhaps, much lower than those of other functions, because infectivity is a function which has to do with the organism's extra-corporeal existence. Thus scarlet fever is said to be rare in tropical countries, and gives evidence of being inhibited by summer temperatures of a little over 60° F. with us, yet the hypothetical

organism has no difficulty in thriving in the human tissues at a temperature of 98° F. or more.

Cholera, again, is inhibited by temperatures a little over 80° F. It is more than probable, then, that the climatological distribution of disease has more behind it than the mere question of carriers; some explanation of a general kind is demanded, and none at present is forthcoming but that just described as the direct effect of temperature upon the degree of infectivity. In the recent investigations on plague in India the fact of the chance of infection from fleas increasing or decreasing with the temperature was explained by the observation that at the higher temperatures phagocytosis was stimulated in the stomach of the flea. It would have been interesting, however, to have seen the further experiment carried out, of keeping and inoculating infective matter at a high temperature without the intermediate passage through the stomach of the flea.

In these varying reactions to temperature the organisms of disease behave in a similar manner to the tissue-cells of the vegetable world, to which, after all, they have always been supposed to belong. The higher the grade in the animal world, the more complete is the tendency for all vital functions to be carried out at one fixed temperature. In the vegetable world, on the other hand, the temperature limits of the various functions are fitted in to the requirements of a vegetative existence dependent on the average course of meteorological and other external phenomena. Thus Schimper [15] shows that the various functions of bearing leaf, flower, and fruit are fitted in to the rising temperatures and short season of temperate climates so that the most suitable temperatures for each function are widely different and are arranged on an ascending scale. In the Torrid Zone, however, in accordance with the continuously high temperatures, the optima of the various functions are more on a level. It is, perhaps, going too far to compare this with the characteristic temperature differences of the præ-epidemic periods of typhoid and diarrhoea.

Another necessary perquisite to the vegetative existence of plants is the faculty of being able, within certain limits, to gradually alter the temperature points of the various functions so as to adapt them to accidental changes of abode, where the altered climatic conditions would otherwise bring about their extinction. In other words, they should possess the faculty of acclimatization. This question in relation to plants is a very complex one. Some plants are able to alter the functional temperature points over a much wider range than others. Again, in the same plant some functions can be adapted much more freely than others. Thus trees from the Temperate Zone may bear luxuriant foliage, but no flowers, in the tropics.

A striking example of adaptation to climate is quoted by Schimper [16]: "In the year 1852, maize for poultry (from Hohenheim, near Stuttgart) was harvested on September 22, 120 days after sowing. . . . This maize year after year ripened more and more rapidly, so that in 1857 it was harvested ninety days after sowing. Seed of the same maize from Breslau, sown in the same bed and at the same time as the former, took 122 days to ripen." In view of this fundamental adaptation of the vegetable cell, it is hardly too much to expect that it should also happen in the equally lowly bacterial cell, which indeed has long been notorious for the variability of its vital functions. It is possible, then, that in the phenomenon of acclimatization we might find some explanation of the next fact to be dealt with in the epidemiology of diarrhoea. Examples drawn from the temperate zone of both the northern and southern hemispheres have shown the limiting temperatures of diarrhoea to be very similar. In the tropical climate of Queensland, however, the limits are very much higher, the equivalent of 60° F. with us being about 66° F. or 67° F. in Brisbane (see Chart IX), while the disease appears to all intents and purposes the same. These facts have also received notice from Dr. Stawell [19], who quotes the opinion of Dr. Hardie, of Queensland, to that effect. The latter also suggests that in still more tropical parts of the country the temperature limit is 5° higher again.

In considering the extent to which acclimatization might be an explanation of the above, the possible introduction of still another factor must be considered, and again an illustration will be drawn from the phenomena of vegetable life. It is shown by Schimper [17] that, although temperature acts powerfully in deciding the date when the various functions of putting forth leaf, flower, and fruit shall be in evidence, yet it is not all-powerful, and forcing by temperature can only be carried on within certain limits and at certain times of the year, for a period of rest is always demanded by the plant tissues, presumably for the renewal of its exhausted energies. He also points out that the plant tissues display an inherent tendency to functional periodicity, whose correspondence with the external temperature conditions of the four seasons is quite a secondary feature—a mere adaptation. It will be seen that the temperature conditions in Queensland, even in the winter time, are still well above the lower limits of diarrhoea prevalence in more temperate climates, so that infection newly introduced from the latter countries would tend to be in season the whole year round. But there seems a constant tendency in these hotter countries for the diarrhoea to contrive, notwithstanding, to have a period of rest extending

over about half the year, while still depending on the stimulus of the upward-moving temperature to recommence its annual active phase. All this tends to support the conclusion that the exhaustion of epidemic potential, noticed also in England, may be largely due to exhaustion of the infective property of the organism itself. A curiously shaped curve is met with again in the subtropical State of Western Australia. The diarrhoea is true to the temperate type in the matter of rising after about one month at 60° F., and only falling when the temperature is well between 50° F. and 60° F. This, however, only leaves it a period of about four months' rest. The summer curve is, however, indented in the middle, or perhaps it should be expressed that the decline which occurs through midsummer is arrested as soon as the heat becomes more moderate, showing a second marked rise in the early autumn. Whether this fall and second rise have anything to do with carriers or with the fact of the short winter rest it is impossible to say. Attention might be incidentally called to the great value of systematic observations upon fly prevalence in these countries would have in deciding the question of carriers. Again, it is questionable as to whether, with the wholesale immigration of recent years, the prevailing infection has not been introduced from the more temperate eastern States and has not yet thoroughly adapted itself. The detailed investigation of the wide field of possibilities here opened must be left for another time.

In conclusion, as suggested in the title, this is only a preliminary study of the subject of season and disease. It has been only possible to open up certain aspects of the question, and whole departments have remained untouched, such as, for example, the direct effect of temperature, as seen in the connexion of cold with bronchitis, as opposed to the indirect effect, through its influence upon the bacterial cause of disease. Every appearance of finality has been avoided throughout. An effort has been made simply to review the facts and set forth, without attaching allegiance to any, every explanation or theory which could be conceived at all likely to explain those facts. It will remain for subsequent work to show which of these theories are false and which are true.

As regards the source of the data in this paper, I am indebted to Dr. Boobyer for the use of the Nottingham statistics of diarrhoea, which are obtained from the Annual Reports; also to Mr. Brown and Dr. Boobyer for the use of their very complete meteorological records, which have been so necessary to the success of the inquiry. The Nottingham enteric fever notifications are from office records. I am also indebted to Sir Shirley Murphy for full permission to make use of

London statistics, to Dr. Greenwood for the use of his interesting diarrhoea records, to the Health and Statistical Departments of Western Australia for information kindly supplied, to Mr. Shacklock, of Mansfield, for his indispensable temperature data, for the assistance of Mr. Marriott, of the Royal Meteorological Society, and for the facilities offered me in the preparation of this paper by Dr. Wills. Where the source of the data of the various charts is not indicated, with the exception of the above, they are all obtained from the weekly returns of the Registrar-General. Finally, I would like to specially acknowledge the kindness of Dr. Hamer, the Secretary of the Section, for help in obtaining statistics, and for the trouble he has taken to make the production of the paper and charts as successful as possible, and to express my thanks for the courtesy extended to me by the President and Members of the Section.

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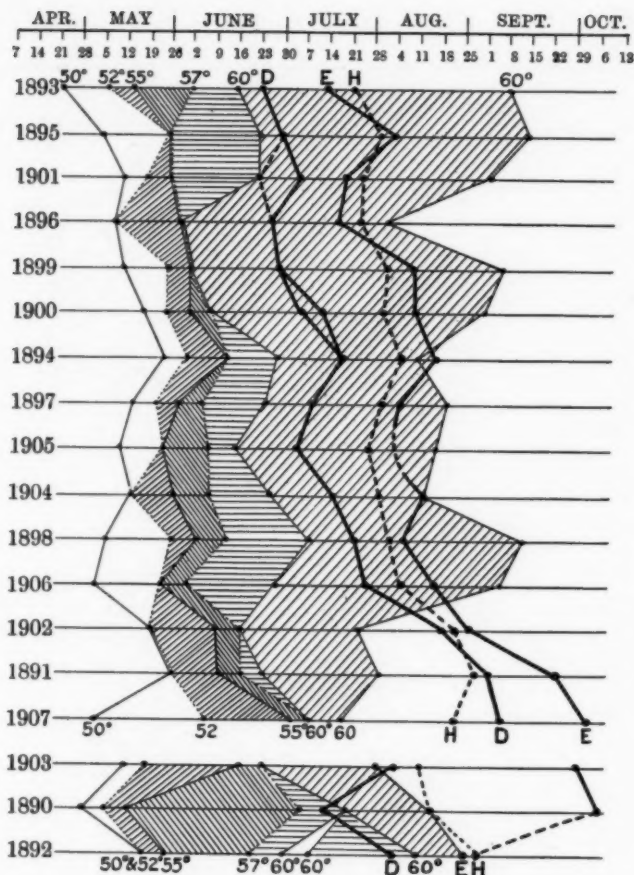


CHART I.

The relation of temperature to the seasonal rise of diarrhoea and enteric fever, and also to the date of harvest, at Nottingham. One-foot earth temperatures are used. Fine lines passing down the chart join the points at which various temperatures were first reached in the different years. Similarly the lines DD, EE, and HH join points representing the main rise of the diarrhoeal and typhoid epidemics and the commencement of harvest respectively. The limit of the plateau above 60° F. is shown on the right. The H points and the week-end dates which the D and E points represent are referred for simplicity to the end of the nearest half week in the standard calendar at the top of the chart. The D and E points are identical for each year in this and the two following Nottingham charts.

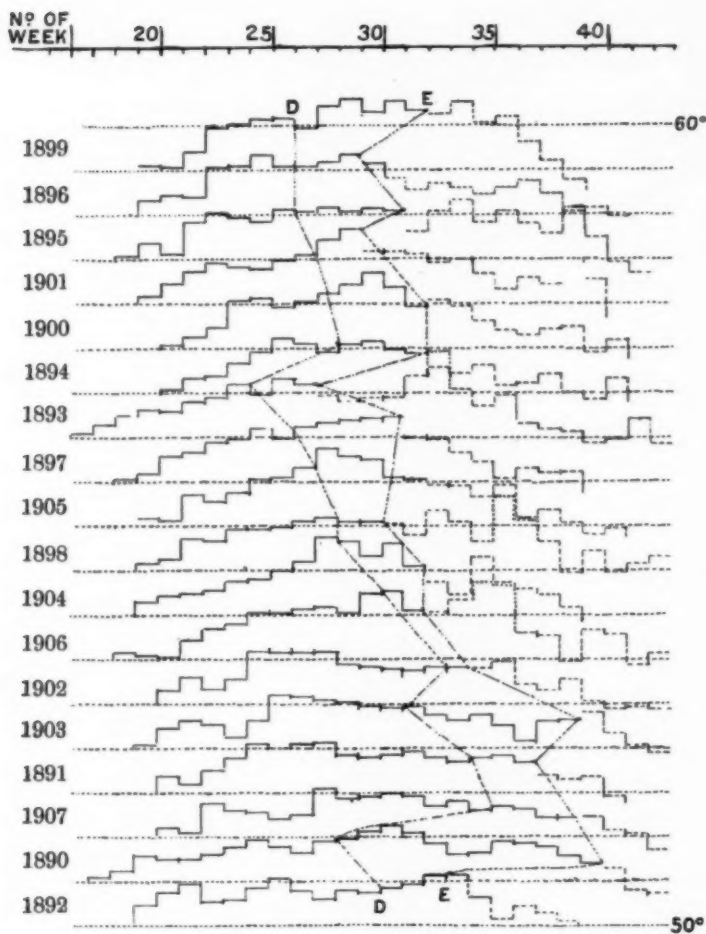


CHART II.

Weekly mean temperatures at Nottingham.

One-foot temperatures thus —
 Air temperatures thus - - -

Each dotted line, except the top and bottom ones, indicates 60° F. for the year below and 50° F. for the year above.

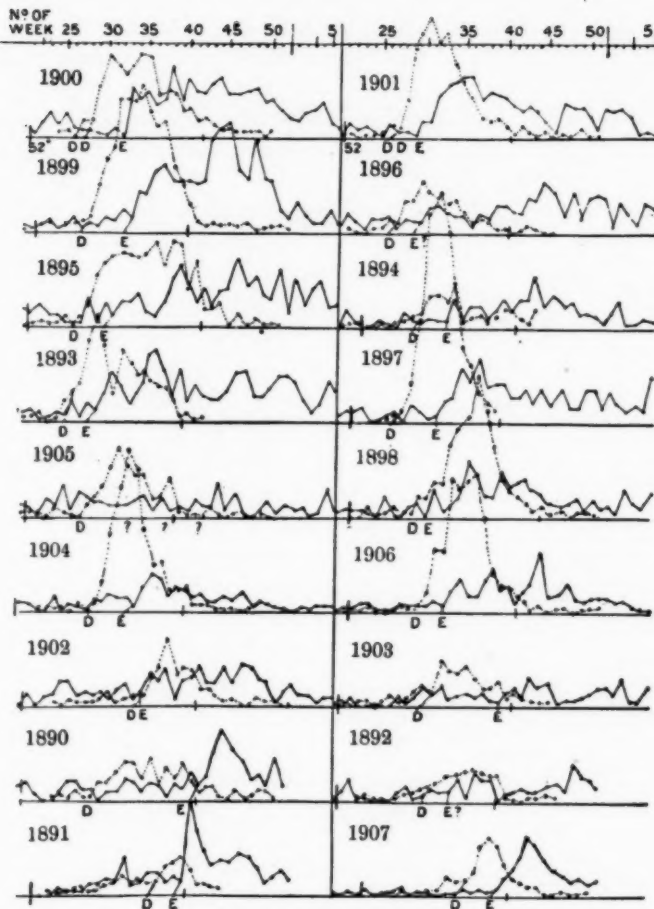


CHART III.

Mortality of diarrhoea and notifications of enteric fever in Nottingham. The tracings are marked across where the rising one-foot earth thermometer reached 52° F., and where the falling air temperature passed 50° F. The top of the space assigned to each year represents thirty-six D deaths or E cases. In this chart the manner in which the D and E points are obtained is correctly indicated, but, owing to errors in drawing which have been overlooked, the weekly records of the two diseases do not always correspond exactly with one another or with the number of the week at the top of the chart. The number of the week can be obtained, however, by counting along from the D and E points, whose position with respect to one another and to the table at the top is accurately indicated in Chart II.

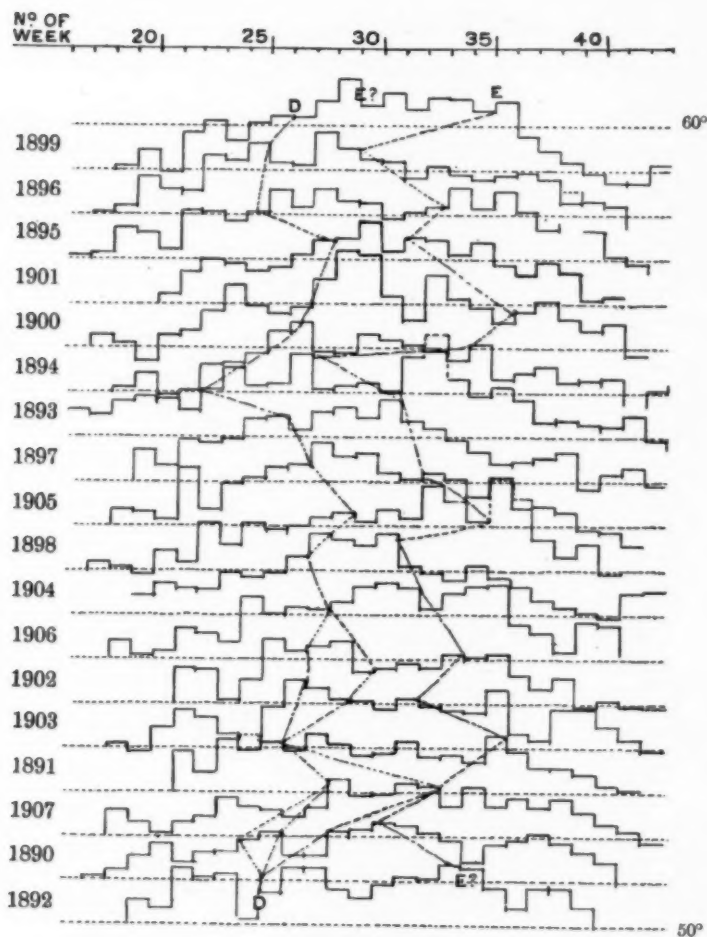


CHART IV.

The weekly mean air temperatures and the date of the seasonal rise of diarrhoea and enteric fever in London. Each dotted line, except the top and bottom ones, indicates 60° F. for the year below and 50° F. for the year above.

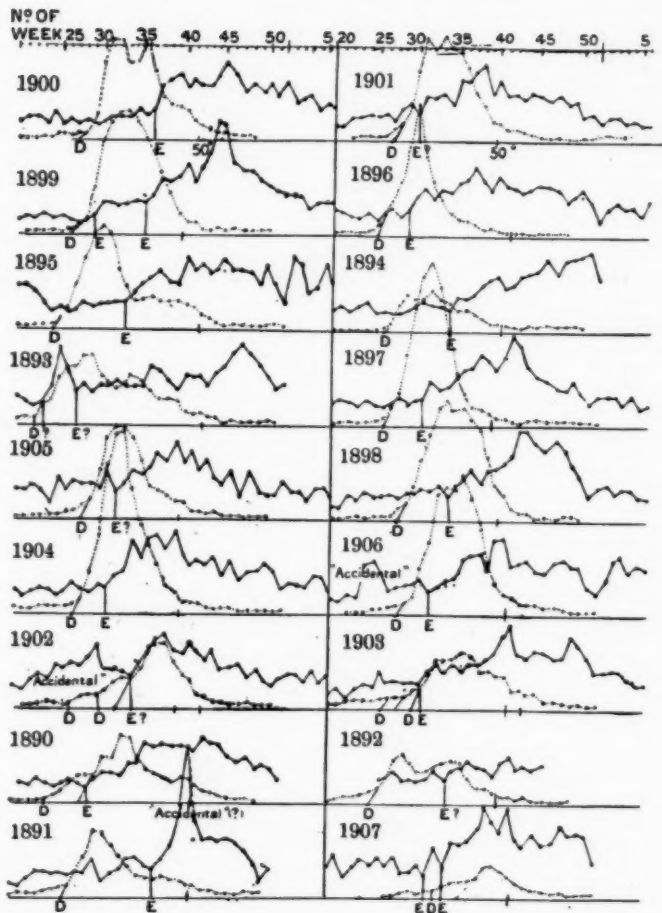


CHART V.

The weekly diarrhoeal mortality and notifications of enteric fever in London. The curves of enteric fever are not comparable from year to year, but simply indicate the relative numbers per week in any given year. The base-line is marked at the point about which the air temperature has fallen to 50° F.

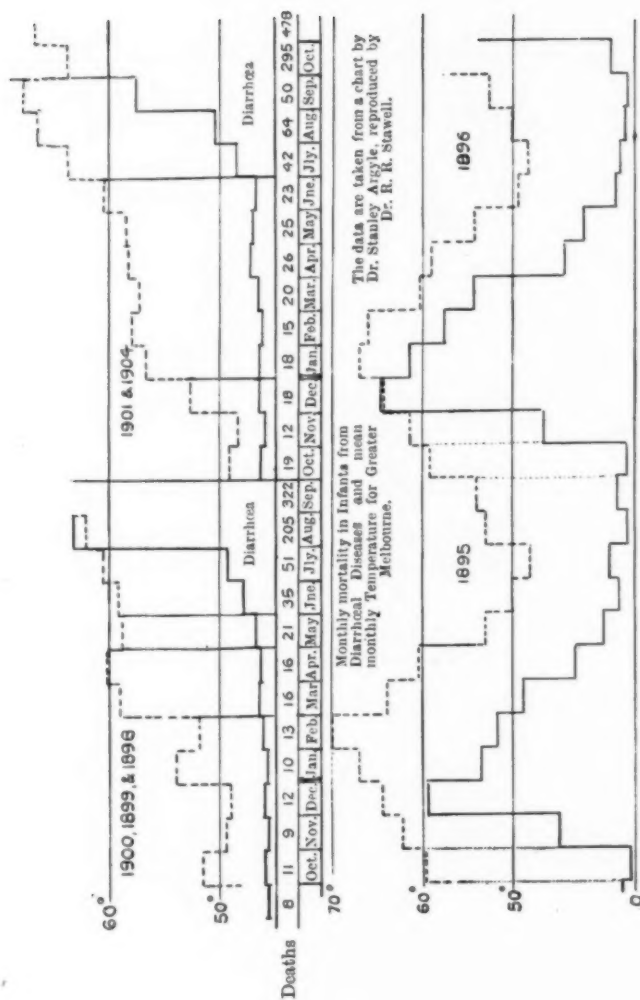


CHART VI.

Composite charts of weekly mean air temperatures and diarrheal mortality—a further analysis of the eight years London curve given in Chart VII. The years are differently arranged to show the influence of different temperatures, maintained at a constant level, upon the first gradual rise of diarrheas above the winter level.

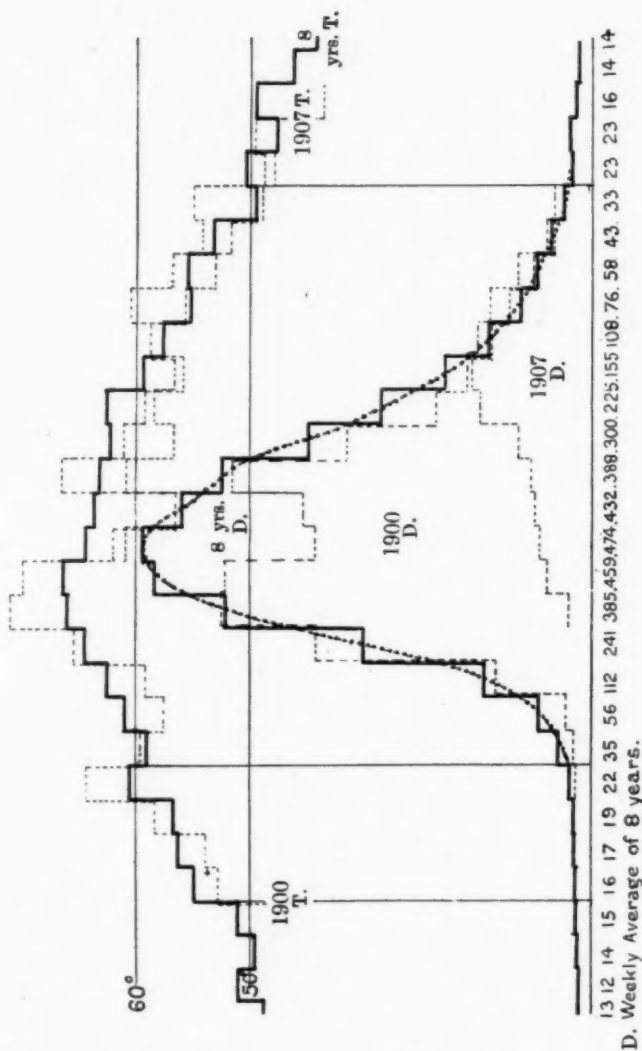
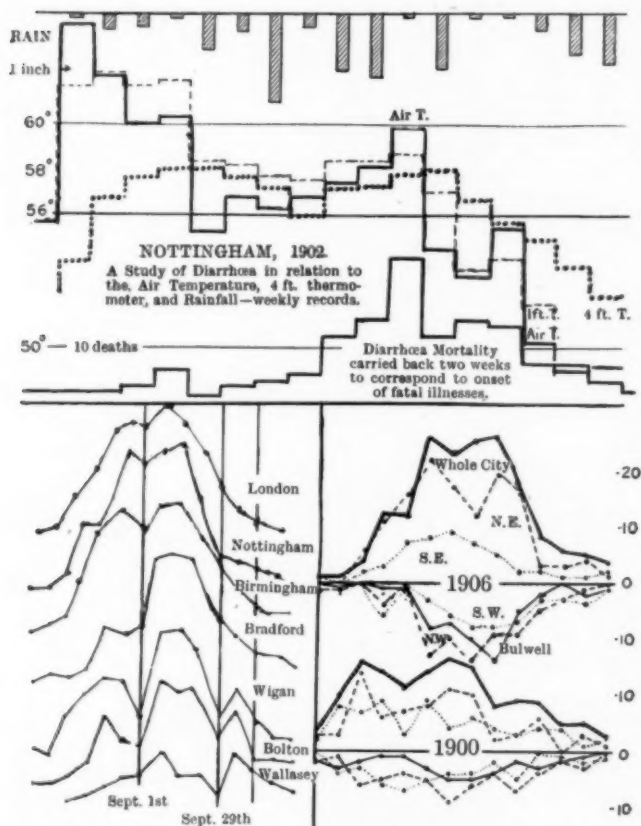


CHART VII.

The average weekly diarrhoea mortality and mean air temperatures of London for eight years, 1897-1901 and 1904-6.

N.B.—The mortality data are brought a fortnight earlier to show correspondence between the temperature variations and the number of fatal attacks. The weekly averages of temperatures and diarrhoea mortality are obtained from the weekly temperature and mortality data by opposing the weeks in which the main rise of diarrhoea mortality occurred, as indicated in Charts IV and V. 1900 and 1907, with their corresponding data, are inserted for comparison.



The Effect of Temperature on Prevalence of Diarrhoea. Weekly Diarrhoeal mortality for 1906 in widely scattered towns, all of which exhibit notches in the seasonal curve, the result of sudden falls of Temperature, which were experienced throughout the country a fortnight before the weeks ending September 1 and September 29.

Seasonal evolution of Diarrhoea in the different districts of Nottingham, showing the manner in which the outline of the City's curve is determined. The curves of weekly mortality for each of the five districts and for the whole City are given.

CHART VIII.

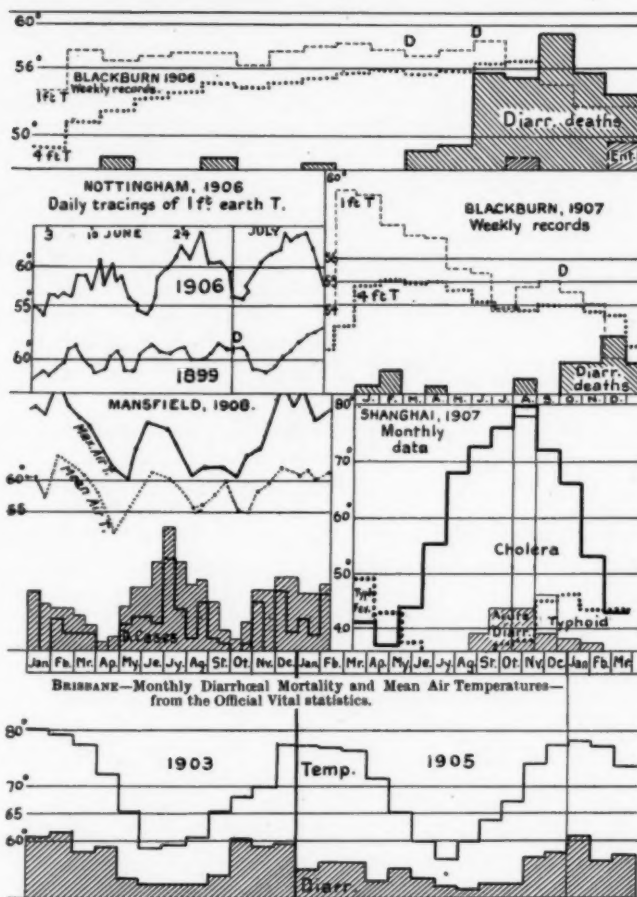


CHART IX.

Notes on above charts:—

BLACKBURN, 1906: Note unusually level and low temperature, illustrating præ-epidemic pause, also apparent sufficiency of four-foot temperature theory. Cf., however, Blackburn, 1907, given below. Conditions for temperature observations unaltered.

NOTTINGHAM, 1906: Showing the extraordinary temperature remissions in 1906. The unusual lateness of the diarrhoea in that year is perhaps a result of this.

BLACKBURN, 1907: Showing fallacy of the 56° F. four-foot earth temperature theory. These data are from Dr. Greenwood's charts.

MANSFIELD, 1908: Non-fatal cases of diarrhoea, occurring daily in adjoining streets, with daily temperature readings. The actual cases are shown and an average curve superimposed. The chart is subject to various corrections, for local influence, &c.

SHANGHAI, 1907: Showing the relative positions of diarrhoea, cholera, and typhoid fever with respect to the temperature curve. Note enormous range of latter, and the onset and apparent inhibition of typhoid in the hottest months. Data from the Health Report (Dr. Stanley).

BRISBANE: The curve of "gastro-enteritis" is given. The curve of all diarrhoeal diseases is practically identical in form.

TABLE I. DIARRHOEA.

YEAR	66°	65°	64°	63°	62°	61°	60°	59°	58°	57°	56°	55°	54°	53°	52°	51°	50°	49°	48°	47°	46°	45°
1899						II	4	4	4	4	4	4	I	5	5	7	8	8	8	10	10	10
1896				I			III	4	4	4	4	4	6	7	7	7	7	7	7	7	7	7
1900					1/2			III	I	4	4	4	I	5	5	6	6	6	6	6	6	6
1894					I	1	2	4	4	5	5	5	6	7	7	7	7	7	7	7	7	7
1895						I	1	2	4	5	5	5	5	6	6	6	6	6	6	6	6	6
1901						I	1	2	3	5	6	6	7	7	7	7	7	7	7	7	7	7
1893					I		I	1	1	3	3	4	6	6	7	7	7	7	7	7	7	7

TABLE II. ENTERIC FEVER.

YEAR	66°	65°	64°	63°	62°	61°	60°	59°	58°	57°	56°	55°	54°	53°	52°	51°	50°	49°	48°	47°	46°	45°
1899		II	I	I	I	II	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10	10
1896				II	I	IV	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
1900	67 1/2			I	II	I	IV	9	10	10	10	10	10	10	10	10	10	10	10	10	10	10
1894					I	III	I	III	8	9	9	9	10	10	10	10	10	10	10	10	10	10
1895						II	IV	II	8	9	9	9	9	10	10	10	10	10	10	10	10	10
1901	I		I			I	I	4	5	6	7	9	9	9	10	10	10	10	10	10	10	10
1893				I	II	I	I	4	5	6	7	9	9	10	10	10	10	10	10	10	10	10

TABLES I AND II.

Number of weeks, at and above various temperatures, preceding the main seasonal rise of diarrhoea and enteric fever. Weekly records of the one-foot earth thermometer are used.

DISCUSSION.

The PRESIDENT (Dr. Newsholme), in the name of the Section, thanked the author for his valuable contribution to the proceedings. It was unfortunate that, owing to the deficiencies of light, the diagrams could not be seen very plainly, but members could look forward to the excellent reproductions which would appear in the journal of the Society. If there was one thing which the members of the Section had to congratulate themselves upon more than another, it was that their papers were admirably printed, and the illustrations reproduced in a manner which was even more excellent. It was clear that the paper represented a large amount of work on the subject, which had always been a fascinating one to the members of that Section.

Dr. HAMER said the paper opened up many interesting questions, and led one to hope that there would later be another contribution from Dr. Peters. The author had discussed, in the light of his thesis of cumulative heat effect, what he described as various provisional theories, and he had taken first of all the hypothesis of continuous case-to-case infection with resulting ultimate exhaustion of susceptible material. He had given to that theory perhaps more credit than it deserved. For example, he alluded to the "gradual nature of the rise itself, as indicated in the returns of a very large city," and then later he commented on the fact that epidemics of diarrhoea began in different parts of a town at different times. He (Dr. Hamer) did not believe these phenomena necessarily showed that the whole story was one of case-to-case infection and exhaustion of susceptible material, but he thought the facts might very well be explained on a hypothesis of food infection. Dr. Peters had further said that evidence of epidemic exhaustion was wanting in Nottingham, whereas in London evidence of such exhaustion existed, and he ascribed this to the fact that the temperature was lower in Nottingham than in London. It struck him (Dr. Hamer), however, that there was more diarrhoea in Nottingham than in London, and therefore, if evidence of epidemic exhaustion were looked for, one would have thought it would have been forthcoming in Nottingham rather than in London. But the point made in this connexion by the author which was especially important was the following: Dr. Peters said: "The question is, Can changes in infectivity definitely related to a long period of meteorological influences take place within the body?" It seemed to him (Dr. Hamer) that here was a very formidable difficulty for the upholder of the thesis of continuous case-to-case infection. The author went on to say: "Until this difficulty is cleared up there is still plenty of room for a theory of infection from external sources." Those who thought there were more things in heaven and earth than were dreamt of in the "bacillus carrier" philosophy might therefore find some crumbs of comfort in this paper. Dr. Peters, however, said he had some difficulty in accepting food infection as playing a large part in all instances in producing enteric fever, because that disease occurred far inland, in new countries, where oysters were not obtainable. But other species in addition to

oysters had been incriminated, and it might be that inland inhabitants of water could also serve as media for spreading infection. Finally, there was a very interesting point with regard to flies and their relation to enteric fever. He referred to the important suggestion contained in the following passage in Dr. Peters's paper: "Owing to the relative conditions that determine their prevalence, the diarrhoea curve will always fall within the period of fly prevalence. Thus, in accordance with the natural history of the two, it will be impossible, by observations and charts of this kind, to show that epidemic diarrhoea can be propagated without the help of flies." It was clearly necessary, therefore, not to lay too much insistence upon the general correspondence between the two curves, however close this might appear, after the prevalence of both flies and diarrhoea had become established.

Mr. LEMPERT said that as one whose work was largely connected with the collection and publication of meteorological statistics, it had been gratifying to him to hear the paper and the use to which such statistics could be put. What the author said about cumulative temperature reminded him of the tables of "Accumulated Temperature," published in the Weekly Weather Report of the Meteorological Office giving such information. In those tables an attempt was made to combine the two elements of duration and temperature by estimating approximately the area bounded by the temperature curve and a base-line of 42° F. In actual practice the published figures were arrived at, not from thermograph records, but from readings of maximum and minimum thermometers. Forty-two degrees Fahrenheit had been selected from the agricultural point of view, as above that temperature the growth of plants in our latitudes was promoted. It would not be difficult to calculate similar tables, referred to a base-line of 55° F. or 60° F., if that limit were found more suitable in relation to disease. The accumulated temperature statistics were summarized most completely not by stations, but by districts. He did not know how far working by districts would commend itself to the medical man, but from the meteorological point of view it had much to recommend it, as it served to obliterate small irregularities and to bring out more prominently the general variations of climate. The author had referred incidentally to the question of exposure, and in making a comparison between Blackburn and Nottingham had suggested that a temperature of 60° F. as measured at one of those places was equivalent to 58° F. measured at the other in bringing on an epidemic of diarrhoea. The statistics for districts gave an effective representation of the climatic variations from time to time. It might be possible to get for the different places factors which might correlate them with the district values, and so get over the difficulty that the district was too large to deal effectively with the problem.

Dr. NEILD COOK said that if a theory held good it should be of almost universal application. In India there were very definite seasons of prevalence in connexion with most of the diseases, but he thought they depended less on the actual meteorological conditions than on the concomitants of those conditions. In Calcutta the season of prevalence for enteric was in the hot

weather, whereas cholera and diarrhoea mostly occurred in the cold weather. Enteric was most prevalent after a long dry season, and the river was low and contained a large proportion of coli organisms from the septic tanks on the banks and other sources of pollution. But he was inclined to attribute the prevalence of the disease more to the house-fly and a number of Service privies which were still unconnected with sewers. Cholera even more than diarrhoea seemed to follow the large religious festivals, when tens of thousands flocked to the places of pilgrimage, where they were badly housed, and bathed in and drank the sacred waters. The sacred quality seemed to be in direct proportion to the number of harmful organisms it contained. Another disease which had a regular seasonal occurrence was plague, namely, at the end of the cold weather and the beginning of the hot, and at that time of the year dog-fanciers would say there was always an epidemic of fleas; probably the conditions which favoured the dog-flea also favoured the rat-flea. Another probable reason of plague occurring then was, that after a long spell of dry weather there were a large number of young rats which had not yet become protected by an attack of the disease, and so were specially susceptible to it. The synchronous occurrence of it among fleas would account for an epidemic amongst human beings. He was aware that some observers would not accept those conclusions. In Calcutta also smallpox had a definite season, namely, at the beginning of the year, lasting all through the dry weather. So that the disease cycles did not seem to depend on the temperature reading or on the rainfall, but on the extraneous causes which were operative at those seasons of the year.

Dr. C. J. MARTIN, F.R.S., said he had listened with great interest to the thoughtful and highly suggestive paper. The author spent most of his time in showing that diarrhoea was influenced by temperature in a cumulative way, and also directly. Before one could interpret the effects of seasonal prevalence upon any epidemic disease, it was necessary to understand not merely the cause, but the exact means of spread of that disease. It might be assumed from the epidemiological evidence which had been accumulated that in infantile diarrhoea some infection, presumably of bacterial nature, got into the milk or milk foods. It was easy to see how temperature could directly influence the spread of infection under those circumstances, because the rate at which an organism multiplied was the function of temperature; multiplication occurred two or three times as rapidly with every 10° rise of temperature, precisely what obtained in an ordinary chemical reaction. Therefore in a given time the child would get a bigger dose. The cumulative effect of temperature was well established by Ballard. Ballard never imagined that the cause of infantile diarrhoea had to be discovered 4 ft. from the surface. He remembered the President humorously touching on that point, and suggesting that if the cause was to be found at that depth the task of searching for the etiological factor would be arduous. Bacteriological methods could be advantageously combined with a little healthy exercise. An interesting point which Dr. Peters had brought out was the difference in the influence of temperature

in warmer climates. On the assumed etiology of the disease it was not very difficult to explain why, in Melbourne, the favourable temperature for diarrhoea should be 5° higher than here. In Melbourne, owing to the higher average temperature, milk was not kept so long as in England. There was approximately the same degree of multiplication of organisms in twelve hours at 65° F. as in twenty-four at 60° F. In the case of infantile diarrhoea he did not see how the cumulative effect of temperature could increase the facilities for infection except by acting on some natural biological process, such as increasing the amount of insect life. One thought first of flies, and one remembered the observations which were carried out by Dr. Hamer and his colleagues on the County Council. The prevalence of flies at a particular date was a function of the cumulative temperature for months before. The investigations were not as extensive as Dr. Hamer would have wished, and more work required to be done before one could see how far the prevalence of flies and of summer diarrhoea fitted. In regard to the charts of temperature and diarrhoea in Melbourne which had been exhibited, very much depended on what scale was chosen for the ordinate in plotting the curve. If this was done with the percentage rise above and below mean temperature and percentage of cases, he might obtain differently shaped curves, and possibly much better correspondence. The paper contained a slight reference to the observations of the Commission for Investigating Plague in India, but he (Dr. Martin) pointed out that the Commission had not yet dealt with the interpretation of seasonal prevalence, except in a casual way now and then. Observations on that had still to come, and the Commission was still busy collecting information from various parts of India where the incidence of plague differed, and when the conclusions were published he could promise Dr. Peters that he would have very little to complain of.

Dr. PETERS, in reply, regretted that the paper was really too long to deal with thoroughly; there had only been time to illustrate the leading points from the charts. With reference to the remarks about epidemic exhaustion not being well exhibited in the Nottingham curves, only a comparative statement was intended. The temperature aggregates compiled by the Meteorological Society were very interesting. When following out the parallel between the harvest and disease he came across the phenological tables of that Society, and felt very grateful for the collateral evidence they yielded. He had no experience of India, but his remarks were based on the statistics of Dr. Cunningham and other observers. As regards the multiplication of organisms in milk, whatever the real value of this factor might be, in recent observations upon the spread of diarrhoea he was not impressed with the all-importance of milk, but he did see a great deal of evidence as to the importance of direct personal infection. There seemed to be little doubt that the charts exhibited evidence of epidemic exhaustion. He had very carefully read the reports of the Plague Commission, and his personal admiration for their work must forbid him from venturing anything in the way of a criticism. He must, however, adhere to the suggestion that epidemiological questions might very profitably be attacked from the somewhat

unorthodox standpoint he had taken. He promulgated no theories in his paper, but simply put forward suggestions as to possible explanations. It appeared that the prevalence of most diseases was determined between various temperature limits. While there was a very general feeling that all these questions would be eventually referred to animal carriers or other ordinary causes, yet until this time arrived the question of the possible existence of more general and unrecognized factors must not be entirely ignored.

The PRESIDENT, referring to the allusions to cumulative heat, thought it should be placed on record that many years ago, in connexion with the well-known epidemic of typhoid fever in Worthing, Dr. Charles Kelly, then Medical Officer of Health of that town, wrote a very interesting report, which was published in *Public Health*, and in this respect showed how excess of cumulative heat, using the words in the agricultural sense, was related to excess of prevalence of typhoid fever.

Fleet-Surgeon W. E. HOME, R.N., wrote that he thought the Society much indebted to Dr. Peters for this paper, which he had prepared with so great trouble, to bring before the Section facts suspected but not previously recorded in detail, elucidating the natural history of these diseases. It was useful to have it brought before them that the etiological factor in these diseases being a plant, albeit a microscopic plant, it had probably some of the vital needs of other plants and a similar life-history—a period of free and eager development, and therewith correlated a period of latency and quiescence; it was well they should be shown that the period of its development might be accelerated or retarded by the meteorological conditions which constituted the climate to which it was exposed. It was impossible to state fully these conditions, with their relative importance and effect on the growth even of wheat, which had been so long and widely studied. How much more difficult to state it for those micro-organisms which they did not observe directly, of which they could only judge, as did Adams and Leverrier of the *Neptune*, by the perturbations they produced. Therefore they were grateful to Dr. Peters for the trouble he had taken to analyse the factors of climate and to decide on their several importance. At the same time he would submit that a result was not always due to the associated factor which seemed the most prominent, the most directly associated—it might be some other less obviously connected. Twenty years ago, in an ironclad at Malta, with 650 men on board, he found that in the winter, whenever they found in any day at one of the six daily observations a relative humidity above 80 per cent., they always had a case of sore throat next day. He therefore associated sore throat with humidity directly. He now believed that that was a mistake—that the cases of sore throat were due to the lessening of the ventilation of the ship, the result of calm, still weather, which allowed air to stagnate, not only in the mess-decks, but also in the Stevenson screen, in which the dry and wet bulbs were exposed. It might interest them to know that for some years he sought in vain other statistical observations on the relation of disease and humidity; at last he found some quoted. They were made in 1743, at Bois-le-Duc, in Flanders, on hygrometer readings on one day, and cases

of what were called intermittent fever occurring the next, and they were made by his own great-grandfather, Francis Home, then surgeon of Colonel Gardiner's Regiment of Dragoons. Similarly the cause of the fluctuations in the D and E lines might be neither the air temperature nor the earth temperature, but some other connected variable to be hereafter identified. He would like to draw charts for each of these years with temperatures vertical and weeks horizontal, to try whether the area between each curve and some low temperature (55° F. or 50° F.) might not be in some recognizable relation to the earlier or later incidence of these diseases. The relations of the H line were interesting, and he understood Dr. Peters to intend that the sudden virulence of the diarrhœa organism varied in onset in certain towns just as the dates of flowering of plants in their gardens depended on the climate—a little more sun here, a warmer wall there, thanks to the stove's chimney, and so on.

Dr. G. B. LONGSTAFF wrote expressing his regret at not being able to attend. For many years he had taken a special interest in the subject, having read his first paper on summer diarrhœa before the Society of Medical Officers of Health in February, 1880, and another in the year following, both of which were included in his "Studies in Statistics." In those early years it was necessary to insist on diarrhœa being due to an organism and not to heat *per se*, but now we had got much beyond that position. He would like to see the methods outlined in the paper applied to two groups of English towns in which the external conditions of soil, climate, and human occupation were quite different. One point occurred to him touching the latter part of the paper. Might not the temperature affect the susceptibility of the population? Might there not be something—apart from all germs—in the ancient belief that the lungs are susceptible in cold weather and the intestines in hot? Such a change of susceptibility conceivably might come about (1) through the nervous system, or (2) indirectly through the alternating activity of skin and kidneys under changes of temperature.

Epidemiological Section.

November 27, 1908.

Dr. A. NEWSHOLME, President of the Section, in the Chair.

The Intermittent Infectiousness of Scarlet Fever.

By W. BUTLER, M.B.

SCARLET fever loses nothing of its interest because of the purely inferential character of its hypothetical contagium. Doubtless, difficulty of interpretation is increased by reason of our ignorance concerning the *materies morbi*, but the phenomena which have eluded the promise of bacteriological research remain none the less a proper object of epidemiological interest. The nature of its infectiousness must, for the time being, be described in terms of its epidemical behaviour rather than be accounted for in the results of experimental investigation.

It is a feature of this behaviour—its intermittency—to which in particular I desire to call attention. Intermittent infectiousness is exhibited whenever from time to time a person who, between-times, is innocuous becomes capable of infecting others with whom during the whole period he may have been in contact. Perhaps the condition is best illustrated in the example of a common cold. Catarrhal affections of the naso-pharynx have at one time been ascribed to chill, at another to infection, and both accounts of the cause of "colds" are probably correct. Whether a "cold" be a specific zymotic disease or not, it is undeniably at times contracted by contact-infection, but the instances in which it is caught as a result of exposure to chill are too well authenticated, both by common and skilled observation, to admit of doubt as to this method of origin. However contracted, colds are, without doubt, infectious when set up. But colds contracted as a result of exposure to chill are examples of disease induced by a critical lowering of resistance in the presence of an infective agent to which, in the absence

of such a crisis, the body was immune. The innocuous presence of the infective agent is the predisposing but essential condition of the irruption of infectivity, and a person who habitually lives in co-partnership with such infectious material as the condition implies may be said, with each recurrence of the exciting cause, to be intermittently infectious.

The flora of the naso-pharynx may be assumed normally to include organisms capable, in those states of body which follow upon chill, of becoming toxic. Having, for the nonce, acquired this property in respect of their habitual host, they preserve the same virulent character when planted out among contacts, with whom association hitherto had been followed by no such untoward consequences. By grouping kindred phenomena we may the better understand the identical process which they exemplify, and in the example of catarrhal fever just outlined we have a type to which, as I hope to show, scarlet fever conforms in many respects. Other examples naturally present themselves.

Erysipelas admittedly exhibits a selective incidence upon those previously attacked. A chill, a prick, a scratch, even a surgical wound made under aseptic precautions, may be followed in those who already have previously reacted to the erysipelatous streptococcus by another attack of the disease. When this happens such infected persons become possible foci of infection to others rendered susceptible by surgical, puerperal, or accidental injury. Yet another example is presented in a definitely specific infection—gonorrhœa. It may be years after the urethritis or other evidence of infectiousness has subsided that an indiscretion of the table, a severe chill, or other depressing circumstance lights afresh the old trouble, with an accompanying recurrence of the infectious state. Such cases are instances of intermittent infectiousness. Between the attacks there is extended a period of latency during which the tissues must be assumed to have harboured a quiescent infective material, innocuous to host or others, until injury lowered resistance and set free the restrained virulence of the lurking organism.

It may be urged that in the case of scarlet fever it is unnecessary to assume an intracorporeal habitat for the infective material, that infected fomites afford a sufficient reservoir on which to draw in explanation of almost all conceivable instances of apparent intermittent infectivity. It would be futile to attempt to prove the negative of this proposition. The view that infected fomites are a means by which scarlet fever is diffused is a theory which stands or falls accordingly as it offers an harmonious explanation of the facts. Without denying the occasional correctness of this explanation, I shall submit a view which it will be my

endeavour to show is more generally consistent with the phenomena observed.

But first I should like to record a case which exemplifies the ineffectiveness of exposure to infected fomites of a person demonstrably susceptible to the disease. A parlourmaid, who for six months had been in daily attendance upon the nurses from the scarlet fever and diphtheria wards, left the Willesden Hospital and two days afterwards was admitted to one of the Metropolitan Asylums Board's hospitals suffering from scarlet fever. The day on which she left Willesden, a laundry-maid in the hospital was found to be suffering from scarlet fever, the rash having developed on that day. The previous day, while suffering from sore throat, she had been in intimate contact with the parlourmaid who had left and there could be little doubt that this was the origin of the parlourmaid's attack. Six months' intimate personal contact with nurses fresh from the wards in their infected garments had failed to convey the disease to the parlourmaid, yet she succumbed immediately she came in contact with the actual disease. There is a growing scepticism as to the part played in the spread of scarlet fever by infected fomites, and it must be admitted that while the possibility of this method of spread is incapable of disproof the evidence on which it rests is far from convincing, and many of the instances cited in proof of its occurrence are better accounted for as examples of personal infection. But so to account for them it is necessary to expand the notion of the conditions under which personally conveyed infection may occur. The prolonged duration of personal infectiousness in those attacked by scarlet fever was for long obscured by the explanation that the infection clung to surfaces and fomites and retained its activity for indefinite and enduring periods. It may be that such is the case, but return cases of scarlet fever are not now explained as being due to the rehabilitation of some long-disused article of play or clothing which had escaped disinfection at the time of removal of the patient. The hypothesis that the infection is retained in and emanates from the persons of those who have recently recovered from the disease is now generally accepted. Whether the infectiousness of such persons is prolonged continuously in anomalous particular cases or is only intermittently persistent under circumstances for the most part undefinable are alternative though not necessarily antagonistic views. Without attempting a comprehensive statement, I believe the following to be true of the infectiousness of scarlet fever :—

(1) Under varying but occasionally recognizable conditions persons recovered from scarlet fever are capable of conveying the infection to

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others after intervals frequently of prolonged duration, when apparently they had ceased to be infectious.

(2) There are persons who have not suffered from the disease who appear to harbour the infection in their tissues in such manner that while there is no ground for considering them during this passive stage a danger to others may yet, by a critical lowering of their resistance, become the source of their own infection.

(3) There are others, again, of whom, while it would be incorrect to say they had suffered an attack of scarlet fever, have yet in some degree reacted to an invasion of the poison and are capable of communicating to those with whom they come in contact the disease to which as such they are themselves immune.

To take the last case first. The frequency with which sore throat in other members of a family precedes a fully-developed attack in one of them leaves little room for doubt that such sore throat stands causally related to the attack in which the classical symptoms of the disease are unfolded.

In the following table is shown the frequency with which scarlet fever is associated with a history of sore throat in other inmates of houses invaded by the disease, together with control results of observations made in respect of diseases not naturally associated with sore throat.

TABLE SHOWING NUMBER OF HOUSES IN WHICH PERSONS HAD SUFFERED FROM SORE THROAT AT, OR WITHIN A MONTH PRIOR TO, THE TIME OF INQUIRY.

	SCARLET FEVER		OTHER DISEASES	
	No. of infected houses	No. of houses where sore throat occurred	No. of houses where inquiry was made	No. of houses where sore throat occurred
Totals	1,266	433 38 ¹	1,644	47
Nett	1,266	395	1,644	47
Percentage ...		31.2		2.8

¹ In these thirty-eight instances the sore throat complained of at time of inquiry subsequently proved to be scarlet fever.

The disparity in frequency in the two series leaves little room for doubt that the prevalence of sore throat in scarlet fever-invaded houses is due in some way to the scarlet fever outbreak itself. Such sore throats are probably identical with those from which nurses and

attendants in scarlet fever wards have frequently been observed to suffer, and are to be looked upon as abortive attacks of the disease. This view is supported by consideration of the difference in age-incidence of scarlet fever and of the related sore throat. Although about 85 per cent. of the cases of scarlet fever occur in children aged under 15, the related sore throat occurs under 15 years of age in about 50 per cent. only of the total number affected. The greater resistance in those over 15 years of age exposed to attack presumably results in only the initial symptoms developing, the attack being aborted at this stage. Diphtheria presents a demonstrable analogue to this condition. A transient sore throat in persons exposed to infection is followed by none of the classical symptoms of the disease, but bacteriological examination reveals the specific character of the faucial inflammation. During influenza epidemics many persons not otherwise ill suffer from a passing lumbago or other muscular pains, which must be regarded as definite reactions to the influenzal infection to which they have been subjected.

In epidemics of other diseases, such as typhoid fever or smallpox, mild cases of illness presenting one or other feature of the prevailing disease are to be found which, while not recognizable as instances of the disease in question, are yet in all probability capable of communicating it to others in its clinical completeness. Apart from the statistical evidence just given it is frequently difficult to account for the origin of certain attacks of scarlet fever, except upon the supposition that out-breaks of sore throat in a family preceding recognizable attacks of scarlet fever render the sufferers from sore throat capable of conveying the disease when all traces of their indisposition have disappeared. The following is an instance—one among many I have noted—which serves to illustrate what I believe to be a far from uncommon occurrence :—

Roger N., removed to isolation hospital September 26, 1908, suffering from scarlet fever ; onset September 25, rash September 26. Shortly before the onset of Roger's illness his brother Albert suffered from sickness and malaise, with redness of fauces, and was considered by his medical attendant to be possibly sickening with scarlet fever. However, as the symptoms passed off without the appearance of any rash, the diagnosis of scarlet fever was negatived. A week later (October 3, 1908) Albert visited his cousin, Ernest A., at a house some distance away, and four days later the cousin failed with scarlet fever and was removed to hospital on the following day. No contact of Ernest A. with Roger N. had occurred subsequent to the onset of Roger's attack of scarlet fever, and Ernest A.'s attack could only be accounted for on the supposition that Albert had acted as a carrier case, not having himself suffered from a recognizable attack of the disease. It is to be noted that Albert did not desquamate.

Even a more striking instance of the infectivity of a transient sore throat unaccompanied by other symptoms is furnished in the following group of cases. Towards the end of September of this year a sharp outbreak of scarlet fever occurred in one of the public elementary schools in my district.

A number of children suffering from suspicious throats were excluded from school. Among these was a child, Kitty L. During her period of quarantine Kitty L. showed no other symptoms than the sore throat referred to, and she was readmitted to school fourteen days after the date of her exclusion, apparently in good health. Eight days after her readmission, however—on October 12—her brother was notified as suffering from what proved to be a fatal attack of scarlet fever, and on the same day a boy in the same class as Kitty L., George H., was also notified as suffering from the same disease. Both George H. and Kitty L.'s brother had an onset occurring on the same day, namely, October 9. There had been no other cases of scarlet fever in the school within three weeks of George H.'s attack.

If the evidence be accepted that persons indiscernibly affected with scarlet fever are yet capable of communicating the disease to others, it will be seen how much more difficult and complicated is the problem of tracing the origin of attacks to infected fomites and surfaces. But more important than this widening of the epidemiological problem is the fact itself that persons presenting no evidence of suffering or of having ever suffered from scarlet fever may, notwithstanding, be harbouring the infectious material of the disease and be the means of infecting others with whom they come in contact. The recognition that apparently normal persons may in their own bodies be carriers of scarlatinal infection very probably accounts for the occurrence of many cases of surgical scarlatina and diphtheritic attacks. It has frequently been noted, and my own observations confirm the fact, that the operation for the removal of adenoids and enlarged tonsils is apt to be followed by an attack of diphtheria or scarlet fever. The interval between the operation and the onset of symptoms so commonly corresponds with the normal incubative period of these diseases that it is difficult to believe that the patient has become infected subsequent to the performance of the operation, and what one is forced to think has happened is that the surgical interference has lowered resistance and auto-infection has been the consequence.

Post-operative scarlet fever is probably strictly analogous to post-operative diphtheria, and each condition presents the phenomenon of an aroused activity of infection due to an induced lowering of resistance. In such instances we have, perhaps, the simplest examples of an

intermittent infectiousness, the intermittency depending upon some crisis in the normal resistance to infection. Continuous exposure in the absence of such a crisis is insufficient to provoke infection, but after an interval of such quiescence traumatism is the occasion of exhibiting a sudden accession of infectivity. Modifications in the degree of susceptibility to specific contagia are responsible for quite as profound changes in the epidemiological history of diseases as are the alterations in virulence of the *materies morbi*, and the familiar example of smallpox stayed in its historic natural epidemicity by the induced immunity of vaccination is a striking instance of this significant fact.

In scarlet fever many of the epidemical features of the disease demand for their explanation less the finding of an actual preceding case than the recognition of variation in the degree of immunity in presence of a tissue infection which has long antedated the actual onset of the disease. It has frequently been observed that children admitted to the scarlet fever wards of a hospital, erroneously supposed to be suffering from the disease, have failed to contract it during possibly weeks of most definite exposure and then have succumbed to the infection upon their discharge and return to the environment of home. It is also probably more than a coincidence that a considerable number of children discharged from hospital after recovery from either scarlet fever or diphtheria should have to be readmitted a few days afterwards from the alternate disease. In all such cases it is probable that the change from the hygienic conditions of the hospital to the less healthy circumstances of the home lowers resistance, and in children capable of auto-infection this variation in degree of immunity determines the attack. It is, on the other hand, no uncommon occurrence to find children attending school desquamating from scarlet fever, who have been in regular attendance during the whole course of their illness and yet have failed during what might be supposed to have been the most active infectious stage to communicate the disease to any of the inmates of the school, although later on they have shown themselves capable of spreading the disease. We are probably in such instances presented with exacerbations of infectivity in the primary case or cases, such exacerbations being disproportionate to the variations in resistance of those exposed. The following series of cases is selected, not merely as illustrating the operation of both these correlative conditions, but as combining many other features in the epidemicity of scarlet fever:—

Florrie C. was taken ill with sore throat on November 5, 1907. She remained at home until November 11, when she returned to school and

continued attendance until November 19, when she was reported as desquamating, and upon being examined was diagnosed as suffering from scarlet fever. No cases of scarlet fever were known to have occurred among the scholars attending the same public elementary school as Florrie C., and no cases of scarlet fever were directly traceable to contact with her prior to her admission to hospital on November 21. On the same date, however, as the commencement of Florrie C.'s illness, viz., November 5, 1907, a boy living next door, Albert E., also suffered from sore throat. He did not desquamate or present other symptoms of scarlet fever, but a month after Florrie C. had been removed to hospital, namely, on December 23, a sister of Albert E., Hilda E., failed with scarlet fever. Her attack could only be accounted for on the supposition that the brother Albert had acted as a carrier case, careful inquiry failing to discover other possible source of infection. During her stay in hospital Florrie C. was found not only to be suffering from scarlet fever, but to be harbouring Klebs-Löffler bacilli. She was discharged from hospital on February 23, 1908, and about a fortnight later, on March 8, her brother, Arthur C., failed with scarlet fever. Arthur's illness was looked upon as being in the nature of a return case and the first to derive from Florrie C.'s attack, notwithstanding her attendance at school during the early part of her illness. On March 12 (four days after the onset of Arthur's illness) Harriet C., a sister of Florrie's, also failed of scarlet fever, and on the same day John K., a lodger in the same house, failed with diphtheria. John K. was discharged from hospital on April 6, Harriet C. on April 25, and Arthur C. on May 2. On May 6 John K. was again admitted to hospital, having failed with scarlet fever the previous day. On May 9 Nellie J., living in the same street, failed with scarlet fever, and it was ascertained she had been in intimate contact with Harriet C. On May 11 Mrs. E., the mother of Albert and Hilda E., already referred to, failed with scarlet fever. She had been looking after Harriet C. since the latter's return from hospital. On May 12 Hilda E., whose previous attack on December 23 has been mentioned, again failed with scarlet fever, and on May 19 Albert E., the supposed carrier in Hilda's attack, also failed of the disease. Nellie J., whose onset on May 9 was believed to be due to contact with Harriet C., was discharged from hospital on July 4, and on July 21 her cousin, Harry J., living in the same house, failed with scarlet fever.

I do not propose to comment in detail upon these cases. Nothing in the course of the inquiries was ascertained which could modify the natural inference as to the origin of the respective cases which the bare record suggests. I will, however, refer to another series of cases which illustrates in lesser degree the intermittent character of scarlatinal infectiousness:—

In January of the present year two cases of scarlet fever occurred at a house in Harrow, and the medical officer of health wrote me that two children, Gladys and Florence Y., living in Willesden, had attended a party on January 6, one of them being stated recently to have suffered from sore throat. Suspicion

attached to the party as the probable origin of the outbreak. Upon visiting the address in Willesden where Florence and Gladys Y. lived, I found that both children were at school—Gladys at one school, Florence at another. When seen, both children were found to be desquamating, and inquiry elicited an onset of the disease on December 26 and 27 respectively. They attended large public elementary schools up to January 16. No cases of scarlet fever occurred at the school attended by Gladys, but one case occurred at the school attended by Florence the day after she ceased attending. In addition to this case, however, it was ascertained that three young adults, aged respectively 18, 27, and 24, who visited at the house of Florence and Gladys, and were frequently visited by them, were each attacked with scarlet fever, the onset of the first case being January 13 and of the other two January 25 and 27.

I will quote only one other instance illustrating the infectivity of persons not discernibly suffering from scarlet fever:—

Lily J. was excluded from attendance during a school outbreak of scarlet fever on account of suspicious illness. There was malaise on September 19, and vomiting, sore throat, and fever on September 20. No rash was observed and no desquamation followed, although she was kept under observation for over a month. Four days after the onset of Lily's illness, her brother Arthur was taken ill with headache, lassitude, and feverishness. No rash was observed, but three weeks afterwards he commenced a typical scarlatinal desquamation. His initial symptoms were much less characteristic and severe than were those of his sister, yet he alone presented the diagnostic features of the disease.

It is a very common experience to find that after cases have been removed to hospital the disease breaks out afresh in the household long after the recognized incubative period has elapsed. Many such cases are doubtless due to the retention at home of overlooked or aborted cases, but it so frequently happens, where the most careful search fails to discover such possible infecting cases, that one is impelled to fall back upon the possibility of auto-infection of the patient at a late period due to some critical fall in his resistance or exacerbation of the latent infection he is harbouring. The following cases are selected as illustrating this point:—

Ethel W., removed to isolation hospital suffering from scarlet fever, September 21, 1908; rash, September 20, 1908. Arthur W., in contact with Ethel up to the date of her removal, remained well until October 11, 1908, when he developed a "cold," and on October 16 he failed with scarlet fever, the rash appearing the following day. When he was removed to hospital on October 18 his brother John also failed with scarlet fever, the rash appearing on October 19. No other persons in the house had suffered from sore throat or other suspicious illness before or after the removal of Ethel to hospital, and it is probable that the superadded infection of a common cold so reduced Arthur's

resistance to the infection, which presumably he had harboured since Ethel's removal, that he contracted the disease by auto-infection and then communicated it to his brother.

A history of superadded infection, such as common cold or influenza, occurring in cases of this character a few days prior to the onset of scarlet fever is so frequent as to suggest the probability of its being an element in the causation. Such added infection conceivably may act by lowering the specific resistance to an infection already present in the tissues, or by symbiotic process raising such latent infection to a pitch of critical virulence. Whichever be the *modus operandi* of infection delayed beyond the commonly ascribed incubative period, there can be no question as to the frequency of its occurrence.

In the following table is shown an analysis of 459 consecutive cases occurring in houses previously infected. The cases are respectively distributed according to the intervals which elapsed between their dates of onset and the dates of removal to hospital of the last preceding case in the house. The table is thus constructed from records of cases where the latest date of possible exposure is fixed by the isolation of the patient.

TABLE SHOWING INTERVALS BETWEEN REMOVALS AND OCCURRENCE OF SECONDARY CASES AND NUMBER OF CASES OCCURRING AT RESPECTIVE INTERVALS.

Interval	No. of Cases
1 to 4 days	210
4 " 7 "	70
7 " 10 "	31
10 " 14 "	28
14 " 28 "	39
28 days to 2 months	53
2 to 3 months	28

The figures permit of many inferences, but it can hardly be questioned that they mean the failure to remove, with the isolation of the patient and disinfection of the premises, all sources of infection in houses invaded by scarlet fever. The prolonged duration of this residual infection and the gradual diminution of its activity with the lapse of time are strongly suggestive that this infection is stored in the tissues of the contacts and recrudesces with a lessening frequency as its origin becomes remote. Whether such contacts are dangerous to others or only to themselves, during the prolonged quiescence of the infection, would seem to be answered mainly by the fact that when the patient fails after such a period of prolonged quiescence he usually infects others who have with impunity been in contact with him during the latent phase of the infection. The sharp drop in the temporal range of

infectivity shown in the first three items of the table, it will be observed, bears no relation to its endurance at a lower but very slowly declining level. For a month at least after the removal of the infected cases from an invaded household the centrifugal diffusion of the infection can be traced. Beyond that period the influence of the primary case or cases in the household is obscured in the period to which the figures relate by the return of the primary cases after a period of isolation. With their return is reintroduced a possible centre of further diffusion, and it is probable that the recurrence of scarlet fever in houses after this period is due largely to such reinforcement.

RETURN CASES.

If it be possible for the infection, diffused by the patient prior to his removal to hospital, to recrudescence at intervals during at least six weeks after his removal and give rise in those who, apparently in their own persons, have harboured the infection, to attacks of scarlet fever, it is not remarkable that the patients who have actually suffered from the disease should, after their recovery and release from isolation, be found frequently to continue its spread.

Probably no one will now dispute this explanation of the occurrence of return cases. What has not so generally been recognized, however, is that the infecting cases, when discharged from hospital, may apparently be non-infectious, while at a later date they will present unmistakable evidence of an aroused infectious activity. Perhaps the most striking evidence of the truth of this view is to be found in the intervals which elapse between the return home of the infecting case and the occurrence of derivative return cases. I have analysed over ninety cases, accounted for as return cases, occurring in Willesden, in relation to the interval elapsing between the return home of the infecting case and the occurrence of the return case, with the following tabulated results:—

ANALYSIS OF NINETY-THREE RETURN CASES, SHOWING INTERVAL BETWEEN RELEASE FROM ISOLATION OF INFECTING CASE AND ONSET OF RETURN CASE, NUMBER OF CASES, AND PERCENTAGE.

Interval	No. of Cases	Percentage of Total
7 days ...	32	34.4
14 „ ...	21	22.5
21 „ ...	9	9.6
28 „ ...	20	21.5
Over 1 month ...	11	11.8

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It is to be observed that only one-third of the cases are infected within the recognized maximum incubative period, dating from the home-coming of the infecting case, while in nearly 12 per cent. of the cases the onset of illness in the derivative cases is deferred for over a month. It is found upon inquiry that practically in all cases there had been intimate contact between the infecting and return case immediately upon release from isolation of the former and throughout the whole subsequent period. Either the infectivity of the patients released from isolation varies in the interval between their return home and the occurrence of derivative cases, or the resistance of the derivative cases changes critically during the period. Many of the infecting cases at the time of occurrence of the derivative cases are found to be subject to mucous or purulent discharges, and when, as is so frequently the case, it is found that the discharge is of recent origin it is difficult to escape the conclusion that with the advent of the catarrh there has been a recrudescence of infectivity.

An analysis of the ninety-three return cases already referred to in relationship to the presence of discharges yields the following results:—

ANALYSIS OF NINETY-THREE RETURN CASES SHOWING NUMBER OF INFECTING CASES GIVING A HISTORY OF "COLD" OR DISCHARGE.

Number of infecting cases with discharge	...	35.0
Percentage of total	37.6

Of the above, in twenty-six of the cases found with discharge or cold upon their return home, all left the hospital free from discharge, but fifteen had had during their stay in hospital either otorrhœa or rhinorrhœa, while ten were free from discharges during their stay in hospital, but were found to be suffering at the time of inquiry from discharge either from nose or ear. One case was a home-isolated case and was found at time of inquiry to be suffering from discharge. Thus 60 per cent. of those found with discharges at the time of the occurrence of the return case had already suffered in hospital from discharge, although apparently cured at the time of release from isolation. That 40 per cent. of those suffering from discharge should have been free during their stay in hospital is very remarkable. It is probable that the return to the comparatively unhygienic conditions of home, after the hospital regime, renders the patient liable to infections of the mucous membrane, and any previous discharge is more likely to be set up afresh under these conditions.

Obviously, however, the presence of discharges is not accountable in itself for the recurrence of return cases. Many of the infecting cases at the time of communicating the disease present a perfectly normal appearance and are quite free from pathological discharges. But so, in fact, are many of the cases which during the clinical course of their attack develop one or other of the complications of the disease. Apart from the desquamation, which is a sequela of the initial dermatitis rather than evidence of an actively existent infective process, many patients convalescent from scarlet fever are perfectly normal to appearance and physical examination, when an adenitis, a nephritis, or an otorrhœa reveals a lurking infectivity, of the existence of which prior to the onset of the complications there was no evidence. These exacerbations of infective processes in scarlet fever patients are characteristic, and the clinical course of the disease has its parallel in the epidemiological history in which most typically scarlet fever is revealed. The patient, after perhaps months of segregation, recovers an appearance of normality and is assumed free from infection, but on commingling with those susceptible to attack he communicates the infection either at once or at an indeterminate period subsequent to his apparent recovery.

We have seen how the infection, diffused prior to the isolation of the patient, may reverberate as it were in infected households for weeks at least after the removal of the discernible cases. For how long may the infection be diffused after the return to the home of the patients who were the starting-points of such outbreaks? It has been customary to take the somewhat arbitrary period of a month or thereabouts after the date of release from isolation as that during which recurrent cases in the household might be considered "return cases," when no evidence to the contrary was forthcoming. There is probably no more ground for ascribing the limit of infectiousness after release from isolation to this arbitrary period than there was for supposing that the period of six weeks was the natural limitation of infectiousness in ordinary uncomplicated scarlet fever. Yet, as may justly be inferred from consideration of the tables given, both of these periods in relation to their respective cases coincide with the interval during which the greatest volume of infectious spread occurs.

It is now some years ago that I was struck by the frequency with which scarlet fever invaded a house for a second time a year or more after the occurrence of a first case in the household. Such cases have not infrequently been recorded, and explanations have been offered that they were to be accounted for by persistence of the contagium in some

article which had escaped disinfection. I have not found that the greatest attention to the details of disinfection by the improved methods of recent years has in any way prevented the recurrence of such cases after prolonged intervals. Accidental coincidence, however, may lend to the occurrence of such cases a striking phenomenalism which may readily be misinterpreted in terms of causal sequence and which a wider survey of the facts would possibly disprove.

Does scarlet fever recur in houses invaded by the disease with greater frequency than in houses previously free from its occurrence? and, if so, for what period is the increased frequency to be observed, and to what is it to be ascribed? With the view of throwing some light on these questions I have analysed the distribution of scarlet fever in Willesden in relation to previously infected houses over a period of several years, namely, from 1900 to 1907 inclusive. During this time there have been notified 3,857 cases of scarlet fever, occurring in 2,882 houses. Recurrent infections in houses have been distributed according to the intervals shown in the respective columns. These intervals represent the period between the receipt of one notification and the next in respect of cases occurring at the same address. All instances in which notifications received within three weeks of previous notifications of cases occurring at the same house have been ignored. For comparison with the results so obtained I have calculated the number of cases, in previously infected houses, which during identical comparative periods would have occurred had the incidence upon previously infected houses been the same in the respective periods as upon all the houses in Willesden. This is to say, that the houses which yielded the recurrent cases would, in the periods in which these cases occurred, have furnished the number of cases set out in the comparative figures had these houses to an equal extent with all the houses in Willesden shared in the distribution of scarlet fever. The figures calculated for comparison are thus corrected for the varying incidence of scarlet fever during the eight years it has been necessary to include in order that a completed five years of recurrences might be observed.

It will be noted that the actual incidence upon previously infected houses is higher throughout the whole period than the comparative figure, which represents what may be called the normal indifferent incidence. As the interval between primary and secondary invasion of the house lengthens, however, the difference becomes less. Had it remained constant throughout, the invariable maximum would probably have been wholly accounted for by increased risks which already

invaded houses may be supposed to incur owing to age-distribution of their inmates. This must be assumed to be a selective influence in the formation of a class of already infected houses. But the fact that the difference is so much less marked in the groups with recurrences exceeding three years than in the groups with recurrences within this period cannot be explained on this ground. The data are not of sufficient magnitude to warrant anything in the nature of definite conclusions, but they are very suggestive. There is nothing inherently impossible in a recrudescence of infectivity of scarlet fever between two and three years after its onset. It has been observed that return cases are even more frequently associated with patients who have undergone prolonged detention in hospital than with those simpler cases which permit of early discharge.

TABLE SHOWING RECURRENT CASES (IN BLACK FIGURES) AND COMPARATIVE FIGURES (IN ITALIC FIGURES) IN INTERVAL GROUPINGS IN RELATION TO WHICH THE RESPECTIVE HOUSES WERE PRIMARILY INVADIED DURING AN IDENTICAL PERIOD.

Year	3 weeks to 3 months		3 to 6 months		6 to 12 months		1 to 2 years		2 to 3 years		3 to 4 years		4 to 5 years	
1900	8	<i>0.7</i>	6	<i>0.9</i>	5	<i>1.8</i>	—	—	—	—	—	—	—	—
1901	28	<i>1.9</i>	5	<i>2.5</i>	7	<i>5.0</i>	9	<i>6.0</i>	—	—	—	—	—	—
1902	14	<i>1.3</i>	5	<i>1.7</i>	4	<i>3.4</i>	6	<i>8.0</i>	6	<i>4.9</i>	—	—	—	—
1903	16	<i>1.4</i>	9	<i>1.8</i>	8	<i>3.6</i>	10	<i>6.8</i>	4	<i>8.0</i>	3	<i>4.8</i>	—	—
1904	22	<i>0.5</i>	4	<i>0.7</i>	1	<i>1.4</i>	6	<i>4.4</i>	5	<i>4.1</i>	3	<i>4.9</i>	1	<i>3.0</i>
1905	15	<i>0.9</i>	8	<i>1.1</i>	6	<i>2.2</i>	7	<i>3.4</i>	11	<i>5.5</i>	12	<i>5.1</i>	8	<i>6.0</i>
1906	23	<i>2.2</i>	11	<i>2.7</i>	7	<i>5.9</i>	14	<i>7.1</i>	14	<i>5.5</i>	9	<i>8.9</i>	12	<i>8.2</i>
1907	36	<i>2.0</i>	6	<i>2.5</i>	11	<i>5.0</i>	18	<i>10.8</i>	8	<i>6.5</i>	6	<i>5.1</i>	7	<i>8.3</i>
	162	<i>10.9</i>	54	<i>13.9</i>	49	<i>28.3</i>	70	<i>46.5</i>	48	<i>34.5</i>	33	<i>28.8</i>	28	<i>25.5</i>
Mean	20.25	<i>1.4</i>	6.75	<i>1.7</i>	6.1	<i>3.5</i>	10.0	<i>6.6</i>	8.0	<i>5.7</i>	6.6	<i>5.72</i>	7.0	<i>6.4</i>

So soon as it is recognized that return cases do not depend for their infectivity upon hospitalism—a fact which is disproved by their occurrence among home-isolated cases—there is no difficulty in conceiving that the case which is admittedly infectious after nine months' isolation in hospital may be found not to have ceased to be infectious twelve months, or even two years, later. Certainly the grosser lesions may persist for longer periods, and such occasional persistence of prolonged infectivity would account for the statistical results with which we are here dealing. It does not follow that such cases are continuously infectious during this time. All the facts seem to indicate that continuous infectiousness is not a feature of scarlet fever. Many cases

seem from the first to be little, if at all, infectious, and the temporal distribution of the derivatives from known infecting cases indicates a rapid decline in infectious activity. Yet, after intervals of widely varying range, there is frequently to be observed an irruption of active infectiousness which in these intervals appears to have been quiescent. Such intermittency in the diffusion of the disease does not appear to be wholly confined to those discoverably suffering from it. It would seem necessary, in order to account for many of the facts in the spread of scarlet fever, to postulate carrier cases analogous to the demonstrable passive bearers of the diphtheria bacillus and the more recently discovered typhoid carriers. The voiding of typhoid bacilli has been shown to be an intermittent phenomenon in one detected carrier of the disease,¹ and an analogous process may be inferred in scarlet fever. There appears to be little ground for seeking an extracorporeal habitat for the infectious material of scarlet fever if we except the one cultural medium—milk; and a view more consonant with all the facts would seem to be that there is continuously diffused throughout the community the flora—if it be a flora—of scarlatinal infectivity which under suitable seasonal and other conditions is communicated from person to person in toxic quantity and degree, from time to time appearing phenomenally as sporadic or epidemic scarlet fever. The conditions under which such infectivity is aroused are little known, but are probably of correlative importance, as a means of limiting the disease, with those whose aim may be said to be to immobilize in discovered cases the infection of which they are obviously the intermittent bearers.

There appears good ground for saying that the change from a hygienic to a relatively insanitary atmospheric environment in those who are the hosts of the infection arouses its kinetic energy, and it is in this way that many hospital return cases are to be accounted for. In the multiple infections of the home, many of them probably non-pathogenic and operative only because of the symbiotic processes they induce, are to be found conditions which offer a promising field of inquiry. The vagaries of scarlet fever will not fit the somewhat stereotyped views with which it has been extensively identified. They are so elusive of formulation that, of almost every statement which may be made concerning it, the opposite is also true: it is a very slightly infectious disease, it is a highly infectious condition; it is a very mild affection,

¹ See paper on "Typhoid Carriers, with an Account of Two Institution Outbreaks traced to the same 'Carrier'" by D. S. Davies, M.D., and I. Walker Hall, M.D., *Proc. Roy. Soc. Med.*, 1908, i, Epid. Sec., p. 175.

it is a most dangerous disorder; it breeds true, but scarlet fever convalescents frequently communicate diphtheria; its infectiousness is short-lived, but it endures for x months or years. Such paradoxes might be indefinitely multiplied. They merely show the difficulty of formulating a comprehensive statement of the natural features of the disease and of presenting any harmonious concept acceptable as a theoretical account of its varied phenomena. The hypothesis of its intermittent infectiousness resolves many of the difficulties with which the epidemiology of scarlet fever is beset, and if we are obliged to recognize that the seeds of the disease are disseminated far beyond the zone of controllable limitation, there to germinate under complicated and obscure conditions, our disappointment will not be unalloyed. For we shall know that in the truer appreciation of the natural epidemicity of the disease preventive measures will be designed more accurately adjusted to the intricate complex of conditions which has intruded upon attention and has refused, with all the insistent compulsion of fact, to be thrust from our calculations or to be ignored in our measure of the forces we desire to control.

DISCUSSION.

The PRESIDENT (Dr. Newsholme) said the Section had listened with great interest to Dr. Butler's paper, and he was voicing the wishes of the Section in thanking him for it. The contribution was most interesting from an epidemiological point of view and presented many points suitable for discussion.

Dr. E. W. GOODALL agreed with the President's expression of high appreciation of the paper, and said the subject was a very wide one, touching many points—etiological, clinical, and pathological. The idea of the prolonged infection of infectious diseases was not entirely new. Twenty years ago a paper was read before the Epidemiological Society by the late Dr. Gresswell, in which instances were narrated of patients who had suffered from diphtheria and at long periods afterwards had presented affections of the throat which, though sometimes clinically diphtheria, in many cases were not; and yet those cases appeared to have given rise to fresh outbreaks in places remote from those in which the patient had originally suffered. There was no doubt that those patients had recrudescences of diphtheria in a mild form a long time after the original attack, and were still infectious. He did not remember that Dr. Gresswell touched upon the question which Dr. Butler had raised, namely, the intermittency of infection; that author had published his observations to show that diphtheria might remain infectious for a long time. Dr. Butler had

adduced evidence to show that, though patients were usually non-infectious, yet every now and then they might become infectious. Four years ago the President had read a paper before the Royal Medical and Chirurgical Society, in which he put forward a large number of facts to prove that not only diphtheria but also scarlet fever might be chronically infectious, and he believed the President used, in his paper, the words "recrudescence of infection." Allusion was made by the President to Dr. Greenhow's book, published in 1860, but he did not think that Dr. Greenhow had any idea that diphtheria was a chronic disease in the sense meant by Dr. Newsholme. Dr. Greenhow alluded only to cases of diphtheria in which the disease lasted several months—a condition noticed by French writers, who spoke of a prolonged form of diphtheria, a form rarely seen since the introduction of the antitoxin treatment. Another work dealing with the subject was the Report issued by the Metropolitan Asylums Board, which was drawn up by Dr. A. G. R. Cameron, a report which had not received the attention at the hands of medical officers of health and epidemiologists which it deserved. Though he did not agree with all the deductions drawn in it, it contained a large mass of facts. Several matters dealt with in the present paper would be found covered even more fully in that report. Dr. Butler had drawn attention more particularly in his paper to the intermittency of infection. During the past year he (Dr. Goodall) had had brought to his notice a curious case of the sort. Possibly it consisted of a mere series of coincidences, but he thought it was more than that. R. C., a boy aged about 13, who was a boarder at a first-class private school, was taken ill on December 14. On December 17 he was sent home and the next day scarlet fever was diagnosed. A few days later another boy in the school developed scarlet fever. R. C. passed through a not very severe attack and on January 30 went to Hampshire, where he remained until February 11, when he returned home. On February 24 he went back to school. On the day after his return to school his mother developed scarlet fever and was isolated. His two sisters were sent away to another house; they had been associated with their brother after he had returned from the country. On March 2 the master of the school sent the boy home again because two other boys had developed scarlet fever four days after R. C. returned to school. The family were then more or less separated: the boy slept in one house, the two sisters in another, and the mother was isolated in a third, but the boy was allowed to be with his sisters during the day. On March 21 one of the sisters failed with scarlet fever and was sent to another house. Nothing more happened until April 20, when one of the servants, a kitchenmaid, developed scarlet fever and was sent to hospital. He saw the boy in April, very soon after the kitchenmaid was taken ill, and there was very little amiss to be found with him; there was only slight hypertrophy of the mucous membrane of the posterior pharyngeal wall. The boy gave a history of having a small sore on one nostril at about the time the attack of scarlet fever commenced, and that it had not healed until just before he returned to school. It was advised that the boy should have his nose douched and that he should not go back to

school in the summer-time. This advice was carried out. He went to a private tutor during the summer daily; the private tutor had two (presumably susceptible) children and they did not contract scarlet fever. R. C. paid a visit of six days' duration to a house in which were several boys; during that time he felt well and nobody else got scarlet fever. He went back to school on September 24. On September 29 a boy sleeping in the next bed developed scarlet fever, and another boy, not in the same dormitory, was attacked with the same disease a day or two later. R. C. was sent home, and he (Dr. Goodall) had an opportunity of seeing him. R. C. was isolated on October 1 and went home on October 3. When he saw him he was thick in his speech, as if the posterior nares were blocked, and there was hypertrophy of the mucous membrane of the pharynx. The boy said that on October 4 he noticed that his nose was stopped up and that later there was a good deal of coryza. He was taken to an eminent throat specialist to see whether there was any chronic condition present, such as adenoids, which could be removed, and that gentleman reported that the pharyngeal mucous membrane was not quite normal, but that there was nothing which was removable. Now that attention was being drawn to cases of this kind more would be heard of them. Probably the intermittency of the infectiousness might be due to more than one condition; it might depend upon the condition of the person who had had scarlet fever or upon the condition of the organism, whatever it was. The organism might pass through virulent and non-virulent stages. During a virulent stage such a condition as a cold, with frequent sneezing, would disseminate the organism, and if the organism was in the virulent stage the disease would be spread. He thought there could be no doubt that some persons who had scarlet fever became chronically infectious in the same way that diphtheria patients did. As the President pointed out in his paper, it was curious how attention was fixed on diphtheria and yet investigators failed to see the same sequence of events in scarlet fever. Dr. Butler was almost persuaded that fomites had nothing to do with the spread of infection in scarlet fever. He (Dr. Goodall) was not inclined to go as far as that. In most places disinfection was well carried out by medical officers of health, so there was not now the opportunity of seeing the spread by fomites as in old days. It was to be remembered that the reception of infection was a question of dosage; if the body was invaded by only a few micro-organisms, the disease would not develop. A class of cases which seemed to show that infection might be conveyed by fomites was that in which a patient was ordered to be discharged from a fever hospital, but for some reason he was detained, and the next day, or within a day or two, another case developed in the house. Such cases would have to be looked into more closely in future. To him they appeared to be cases of infection derived from clothes and similar articles, but in the light of Dr. Butler's paper the continuance of the disease in the house in the form of mild sore throat would have to be considered. The author had referred to the important question of infected houses. That was almost a new point, but Dr. Berry, the medical officer of health of Grantham, had contributed a paper on the subject to the *Lancet*

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a year or two ago. In that paper he stated that he had noticed that in a certain epidemic scarlet fever cases kept cropping up in houses for several months after the first case; but Dr. Berry offered no explanation of these cases, which he had never observed in previous epidemics, so it was clear that they would not be finding those carrier cases, or cases of recrudescence, in every epidemic. After the present paper he hoped more investigation would be carried out on that point. On the whole, his views agreed with those of Dr. Butler; in fact, during the last two or three years he had been teaching, as an hypothesis, that the virus of scarlet fever, when definitely recognized, would probably be found in a large number of healthy people, and that certain circumstances, such as the removal of tonsils and adenoids, or an attack of diphtheria, produced a condition of the mucous membrane which allowed it to become active and so set up an attack of the disease. That hypothesis would explain cases which were otherwise obscure. There was evidence that the scarlet fever virus was very widely distributed throughout the community, and that fact did not encourage hopes of being able to stamp out the disease. Scarlet fever was not now so virulent as it used to be, but he feared it was equally prevalent. If those views were correct, they could hope to limit the incidence of the disease, but hardly to do away with it altogether. He thanked Dr. Butler for his contribution, which would lead them to carry out their inquiries in new directions.

Dr. PARSONS expressed his agreement with the commendation of the paper which had been made, but he thought they must also agree with Dr. Goodall that if the hypothesis of the intermittency of infectivity of scarlet fever removed many of the difficulties in the way of its epidemiology, it also added many difficulties to the problem of its prevention. If the infection was so widely prevalent it seemed almost hopeless to attempt to fight it by isolation and disinfection, and therefore the question arose whether it might not be well to let it burn itself out, and allow the people who harboured it to acquire immunity. We might thus hope to obtain a race that would be less affected by it. Possibly something of the kind might have been acting, thus accounting for the disease now being less virulent than in times past. With regard to the development of infectivity after it had apparently ceased, in reading accounts of return cases he had often been struck by the remark that infection from the convalescent took place after there had been some discharge, frequently following catarrh. There were cases of scarlet fever which occurred after operations about the throat, and in them the cause might have been the lowering of the patient's resistance. But it seemed to him that an explanation might possibly be that there was a discharge in such cases which afforded a medium for the multiplication of the organism upon which the disease depended. In a healthy mucous membrane there was scarcely any discharge, and therefore little upon which the organism could thrive. But if there were a mucous, or muco-purulent, or sanguineous discharge, the organism could easily be multiplied. It was known that the contracting of infectious diseases was very largely a matter of dosage; that a person who had a certain degree of immunity might come into slight contact

with the infection without contracting it, but that if the infection were presented in a concentrated form he would contract it. With regard to fomites, he knew one case in which a child was taken ill with scarlet fever without any ascertained cause, and the case was a mystery until it was remembered that a few days previously she was playing with a doll which had been given to her by a lady. Inquiry showed that the lady had a child who had died of scarlet fever a year before, and that in the interval the doll had been shut up in a box. When he was inquiring about the spread of infectious disease by rags, he found plenty of cases of smallpox in which rag-workers had contracted the disease, but there were very few even suspicious cases in which scarlet fever had been so contracted. But against that it must be remembered that rag-workers were people who were less susceptible to scarlet fever, because most of them had probably had it in childhood or early youth. With regard to scarlet fever houses, he asked whether the explanation might not be one of age, *i.e.*, in one house there was a large number of children, whereas in another the inhabitants were mostly adults.

Sir SHIRLEY MURPHY desired to ask a question concerning the construction of the table on page 73. He was not sure whether they were to understand that the inmates of those houses throughout the whole period were the same, or whether the author was dealing with tenement houses in which people stayed only a short time, and in which there were many changes. Perhaps, if he had not the exact figures, he might be able to say generally whether, in his opinion, it would materially affect the result. With regard to the cases of scarlet fever occurring after injury, they were all familiar with septic eruptions following injury, which eruptions sometimes closely simulated that of scarlet fever, and he did not know how, clinically, it would be easy to distinguish those cases from actual scarlet fever; they had the red rash and the sore throat, but he could not say whether there was desquamation afterwards. But the test would be as to whether they infected other people. Such cases were referred to in Dr. Butler's paper, and he therefore asked him if he had evidence of the spread of infection from such cases. With regard to scarlet fever following diphtheria, in diphtheria cases one often found eruptions which must be regarded as septic eruptions. Those, again, he did not always know how to distinguish from scarlet fever. One heard of prolonged infections in cases of scarlet fever and diphtheria, and one was told about them in connexion with enteric fever. He had never heard them mentioned in connexion with measles, but he had no opportunity of knowing whether they did occur. Neither had he heard of them in connexion with smallpox, as he probably would have done if they actually existed. He wished to join with other members in expressing his very high appreciation of the paper; the facts were not only admirably told, but the evidence collected showed a most painstaking and exhaustive inquiry.

Dr. BEDDOES said he had tried to read reports of the Metropolitan Asylums Board, to which Dr. Butler had called attention, but the difficulty of obtaining them and knowing when they were coming was so great that he did not know any Government publication so hard to study. Perhaps Dr. Goodall would

consider that in relation to those he was capable of influencing. With regard to the distribution of scarlet fever, it was very extraordinary how in some countries it was almost absent, according to the statistics issued by them. For instance, in South America it was a comparatively rare disease. At Manchester, in a limited population with a limited area, it had struck him at a hospital there that there were differences in the virulence of the local infection. He meant that from one part of the district nearly all the cases came with throat affections, while from another there was a high percentage of cases of albuminuria; and from still another a batch with more joint affections. If that was generally so, he was not aware that the fact had been recorded. If it were true, did the return cases have the same characters? With regard to surgical scarlet fever and its outbreak where there was local temporary lowered resistance, he remembered a lad coming for the operation of cleft palate. He was apparently in good health when he came in, but on the second day his nose became very red, and on the third or fourth day he had a typical scarlet fever rash. Shortly afterwards a case of burn of the foot was admitted, which was not treated by iodoform, and on him there was a rash similar to that of scarlet fever. He read the matter up in "Holmes's System of Surgery," and that author said there were often rashes spreading from burns. He would like to know whether anything had been done as to tracing how late a scarlet fever case showed the reaction of Wassermann. He thought that might well be worked out.

Dr. CORBIN said he agreed with Dr. Butler that in scarlet fever infection by fomites was very rare, but he thought that in regard to smallpox it certainly did take place. In Stockport a case of undoubted discrete smallpox arose in one of the mills. There were no cases at that time in the country, so it was impossible to connect the case with any contact. He investigated the matter and found that the patient was a piecer, whose duty it was to piece together broken strands by wetting the finger and thumb—which was usually done by the mouth—and rubbing the broken ends together until they united. He found that the cotton came from New Orleans, where he discovered that smallpox was endemic, and at that particular time was epidemic. Though this case appeared at a time when the resistance of the community against smallpox was particularly great, the girl was infected apparently by fomites. It was interesting that the girl was working in a room with twenty operatives, and of those only three were vaccinated, and of the unvaccinated he could only persuade a few to get vaccinated. Yet not one case of smallpox arose. Thus it was evident that the resistance of the community, as well as the resistance of a particular individual, had to be taken into account. No doubt the latency of those diseases was somewhat determined by climatic influences, and the return cases of scarlet fever were inclined to arise at one period of the year more than at another. With regard to injury influencing scarlet fever, he had come across cases in which injury, without causing superficial abrasion, had caused sufficient shock to lower the resistance, and he thought that this had been the predisposing cause of the scarlet fever. He knew of another case in which exposure to coal-gas caused scarlet fever, and he did not doubt that the lowering of resistance was a very definite factor.

Dr. MEREDITH YOUNG said that when ten years ago he was medical officer of health for Crewe he knew an incident which excelled anything he had heard in the history of this disease. In one of the public elementary schools he found a child who had scarlet fever and who had been attending school, with the exception of one day when the rash was out, during the whole attack. At the time of his visit she was sitting in the back row, there being thirty or thirty-two other girls in the class. She was picking off little pieces of desquamating skin from her fingers and passing them along the bench to her school-mates. Those girls were not merely examining the pieces, but several of them, in his presence, put them into their mouths and chewed them. The child suffering from scarlet fever had some excoriation about the anterior nares and evidence of dried discharge about one of the ears, and she had both otorrhœa and rhinorrhœa. He followed the history of each member of that class with the greatest interest, but none of the girls had an attack of the disease. In the face of facts like that, one must regard any measure for prevention as mere finesses against the unknown.

[Endeavours have been made since this speech was delivered to find the records of the number of protected and unprotected children in this class, in accordance with the suggestion of the President of the Society. As the incident is about nine years old, this effort has so far been unsuccessful.]

Dr. TURNER asked what was the explanation of the table on page 68. The author said a rapid drop of cases occurred from the first week to the third month. But in the middle of the chart there was an extra infection by the discharge of the primary case. He asked whether he was to understand that the table as given included all the return cases. If so, it seemed peculiar that there was no recognizable hump on the curve. He also asked whether the return cases were included in the last table.

Dr. HAMER said one point particularly interested him in this very suggestive paper, namely, that Dr. Butler conclusively showed that, to use Pettenkofer's terminology, in addition to *x* and *y*, the susceptible person and the germ, there must also be a third something, *z*, before scarlet fever could be produced. It seemed to him (Dr. Hamer) that while the author demonstrated that *z* had an existence, the explanation he offered of the nature of *z* was inadequate. He understood him to say: *first*, that there must be a lowering of resistance in an individual, who, *second*, must be living in co-partnership with "the flora of scarlatinal infectivity," while, *third*, there must be a simultaneous lowering of resistance in a predisposed individual who was to receive the infection. Thus Dr. Butler postulated an exceedingly complex kind of *z*—it was a function of three variables; and inasmuch as one could assign values to all the variables, one would think *z* would fit any given circumstances. But Dr. Hamer considered there were a number of critical objections which this *z* did not meet. There was in the first place, at the foundation of Dr. Butler's argument, the assumption that the flora of the naso-pharynx, "having acquired toxic property in an habitual host, preserved the same virulent character when planted out

among contacts." That was simply an hypothesis; no direct evidence was adduced in support of it. There was no conclusive evidence that a healthy child having diphtheria bacilli in its throat possessed the ability to communicate diphtheria to other children; similarly there was no conclusive evidence that typhoid bacillus carriers could communicate typhoid fever to other persons. Dr. Butler said: "The hypothesis that the infection is retained in and emanates from the persons of those who have recently recovered from the disease (*i.e.*, scarlet fever) is now generally accepted." But how did the infection emanate? It was, however, only fair to say that Dr. Butler's attitude, so far as scarlet fever was concerned, was precisely that assumed by Dr. Savage, who had done so much to illuminate this particular field of inquiry in a recent bacillus carrier discussion. Dr. Savage said of the bacilli of bacillus carriers, in the discussion at Cardiff: "It is a reasonable assumption that they cause disease, and the onus of proof hardly rests with the bacteriologists. But if not with them, with whom did it rest? In any case, when Dr. Butler assumed that the throat flora of his scarlet fever contacts was capable of infecting other persons, was he not begging the question at issue? The second point Dr. Hamer desired to make was with regard to Dr. Butler's evidence of the remarkable tendency of scarlet fever (and diphtheria) to cling to houses. Dr. Butler said: "It is a very common experience to find that, after cases have been removed to hospital, the disease breaks out afresh in the household, long after the incubation period has elapsed." That looked as if *z* was something clinging to the house, and not necessarily something inhering in the apparently healthy human inhabitants. The third point was that there was, as had been observed by many observers, from Sir W. H. Power's Pirbright inquiry onwards, a similar tendency for throat malady to cling to the school, and not merely so, but also to the department and the class-room. That propensity seemed to indicate that it was something in the class-room and in the building, rather than something in the children, which was determining the persistence of the infection. *Fourth*, the views here expressed seemed borne out by the fact that, while the tendencies to cling to the house and to the class-room were so marked, the phenomenon was not exhibited in the case of the isolation hospital ward, unless it were under exceptional conditions of overcrowding. *Fifth*, as Dr. Peters pointed out at the last meeting, there was the question of temperature, and as he said: "It is difficult to see how those temperature influences could get at the organisms within the body, where the temperatures are notably high and constant." If the healthy carrier hypothesis was to be accepted as covering the entire ground, some explanation was necessary in this connexion. *Sixth*, taking a broad view of the matter, it was necessary that the unknown *z* should explain the epidemiology of scarlet fever—its seasonal distribution, its relation to hot years and to years of deficient rainfall, its historical, geographical, and racial distribution, and other things, too, and it seemed to him that the healthy carrier hypothesis hopelessly failed to explain that epidemiology. Dr. Butler had taken a recently formulated fashionable theory and applied it to explain *z*. Dr. Hamer suggested that it was necessary to

consider also some other hypotheses. There was the hypothesis mentioned by Dr. Goodall, that clothes might play some part. Another theory which had had some vogue, as regards one and another disease, was that of an intermediate host. If Dr. Butler were to take, for example, a flea hypothesis and apply it to the facts mentioned in his paper—the laundry-maid and the parlourmaid, Roger N., Kitty L., and the rest—he would find it adequate, and beyond that such hypothesis would meet some of the objections he (Dr. Hamer) had raised to Dr. Butler's hypothesis. Whether a flea hypothesis would meet the last of these objections was a difficult question. More knowledge was required. Before finally deciding as to the nature of *z*, it was clearly necessary carefully to examine all the suggested hypotheses.

The PRESIDENT (Dr. Newsholme) expressed his high appreciation of the paper. Dr. Goodall had referred to a paper by him (Dr. Newsholme) which he read four years ago in that room. As this paper dealt with the same subject as Dr. Butler's, and arrived at somewhat similar conclusions, he thought the most suitable contribution he could make to the discussion would be to have printed in the *Proceedings* an extract from this paper:—¹

"*Summary of Conclusions.*—Firstly, . . . cases of protracted and recrudescent infection are relatively infrequent, representing only a very small percentage of the total cases of these two diseases. . . . The number of 'return' cases is small and not such as seriously to invalidate the enormous benefit which patients and their friends receive from the hospital, even if it be assumed that 'return' cases are special hospital phenomena. Secondly, cases of protracted and recrudescent infection are oftenest associated with nasal and aural discharges, though not always so. The proportion between 'return' cases in which such discharges have been present and those in which they have been absent cannot be stated in the absence of statistics of the most accurate and detailed description on a large scale, the data for which do not at present exist. . . . Thirdly, the absence of this information indicates the necessity for accurate records, both in hospital and home practice, of all cases of protracted and recrudescent infection. . . . Fourthly, the occasional occurrence of such 'return' cases emphasizes the necessity for post-isolation supervision of patients, whether treated at home or in hospital. . . . Fifthly, a study of the preceding cases indicates that in most instances the rhinorrhœa, which is oftener than any other morbid condition associated with protracted infection, was already present when the patient was first isolated, or appeared soon afterwards. This fact has an important, and hitherto neglected, bearing on the origin of 'return' cases. It suggests the belief that a condition determining protracted infectivity may have been similarly present from the first in the infecting patients,² in whom no obvious mucous discharge occurred. . . .

¹ "Protracted and Recrudescent Infection in Diphtheria and Scarlet Fever," by A. Newsholme, M.D.; read June 14, 1904; *Med. Chir. Trans.*, 1904, lxxxvii, p. 549.

² Patients causing "return" cases are "infecting patients." Patients infected by these are "return" cases.

Seventhly, *there is definite evidence that in some instances infection has become dormant, to be again roused into activity by various circumstances*, as by the disturbance caused by cessation of isolation, possibly in connexion with baths at the time of discharge, or at a later period by a catarrh or other means tending to lower personal resistance. Eighthly, the cases bring out the close analogy between the phenomena of protracted and recrudescence of infection in scarlet fever and in diphtheria. (1) In both diseases infection is occasionally exceptionally prolonged. The percentage of 'return' cases in most statistics is stated to be much lower in diphtheria. We have no certain knowledge how long the scarlatinal germ will survive in the naso-pharynx, though probably in exceptional instances quite as long as the diphtheria bacillus. . . . From a clinical standpoint it must, I think, be regarded as still an open question whether protracted infectivity is more common in one of these diseases than in the other. . . . (2) In both diseases infection occasionally recrudesces. This may occur (a) in the form of a definite relapse having all the clinical features of the disease in question; but more commonly (b) the only accompaniment of recrudescence of infection is the recurrence of a mucous discharge, and even this indication of the possibility of recrudescence may be absent.

"Explanation of Protracted Infectivity.—The persistence of active and the occasional recrudescence of dormant infection may be compared with corresponding phenomena in ringworm. Most cases of scarlet fever or diphtheria are like tinea of the face or body—the contagium quickly disappears. The exceptional cases are like tinea capitis. No difficulty arises in destroying the contagium on the scalp surface, but it reappears with the growth of the hairs from their follicles. And this comparison incidentally furnishes what is, I believe, the correct view of the causation of persistence of infection in scarlet fever and diphtheria. The specific micro-organisms of these diseases remain in crypts of the naso-pharynx, which act as active 'incubators' of them. This applies to the cases continuously infectious. In those cases in which infection temporarily disappears, to recur under the influence of a catarrh or some traumatism, &c., it may be supposed that the specific micro-organisms have been lodged and at least partly incarcerated in the follicles, and possibly in deeper lymphatic tissues or glands. The occasional protracted persistence of the Klebs-Löffler bacillus in the throats of certain healthy individuals who have been in contact with diphtheria and in the throats of convalescents from this disease is well known. . . . The facts as to protracted scarlet fever are not so well recognized, but exactly the same phenomena occur in this disease as in diphtheria. . . .

"Hospital Treatment in Relation to 'Return' Cases.—Instances have been given in this paper of protracted infection in scarlet fever in which there had been no recent contact with acute cases of that disease, and of cases in which there had been no contact during the patient's illness with any except the patient's personal infection; and it has been shown that in some of these cases infection recrudesces after an interval of apparent freedom from infection. It has also been shown that such instances of protracted and recrudescence of infection occur

in diphtheria when patients are treated at home: and in this disease, no suggestion, so far as I am aware, has been made, or could be supported, of any special hospital influence favouring 'return' cases. The known close relationship and analogy between the two diseases suggests that the explanation of the above occurrences for one disease will apply equally for the other. How do these facts and considerations bear on the suggestion that the occasional persistence of infection in scarlet fever is due to hospital infection? The aggregation of cases in hospital wards has been widely taught to be the cause of 'return' cases in scarlet fever. . . .

"In view of the preceding facts and considerations, I maintain that in scarlet fever, as in diphtheria, *the occasional persistence of infection is a phenomenon in the natural history of the disease, irrespective of, and in the main uninfluenced by, the external conditions to which patients are ordinarily subjected.* This view (a) is not contradicted by the statistical evidence as to the excess of 'return' cases in hospital experience; and (b) it is strongly confirmed by the facts and considerations already advanced; (c) by the facts as to relapses in scarlet fever; and (d) by the analogous facts as to protracted infection in other infectious diseases besides scarlet fever and diphtheria.

"*Statistical Evidence.*—Statistical evidence supports the view that 'return' cases occur in disproportionate numbers in connexion with hospital-treated cases of scarlet fever. But it needs to be remembered that the number of 'return' cases in any experience is relatively small, and that home-treated cases have not hitherto been rigidly investigated in regard to 'return' infection. It is known that from 1 to 4 per cent. of 'return' cases ordinarily occur after hospital treatment of scarlet fever. When home-treated 'return' cases have occurred, they have commonly been ascribed to failure of disinfection rather than, as in hospital cases, to persistent or recrudescient personal infection. I am at present totally unable to accept as satisfactory the statistical evidence that 'return' cases are a special hospital phenomenon. It has to be remembered that home-treated cases are picked cases, allowed to be treated at home because there were no susceptible children in the same house or because the home conditions as to isolation and cleanliness were good. Before any accurate conclusions can be drawn, it will be necessary to know both for hospital and for home-treated cases of scarlet fever, the number, age, and sex of all the persons in the same household not previously having had scarlet fever, and to state the incidence of recurrent infection with due regard to these facts. One statistical reason for the under-statement of home-treated 'return' cases is illustrated by cases given in this paper. A considerable proportion of the total home-treated cases is furnished by families in easy circumstances, with whom change of air during convalescence is a matter of routine. Even if it be ultimately found that there is a real excess of 'return' cases in connexion with the hospital treatment of scarlet fever, the facts already recorded, showing (a) that 'return' cases occur when single cases are treated at home, and (b) that they occur in hospital-treated patients for whom either the conditions of home treatment have been secured or for whom separate treatment in

convalescent wards has been secured, indicate that 'return' cases are not essentially hospital phenomena, *i.e.*, that treatment alongside of other acute scarlatinal patients is not the unconditional invariable antecedent of protracted or recrudescent infection. Consequently hospital treatment is degraded to the position of a non-essential concomitant, at the most an auxiliary, of such infection.

"*Protracted Infection in Scarlet Fever.*—Here, again, much light is thrown on the problem by comparison with other infective diseases. Diphtheria frequently manifests protracted infectivity in home-treated cases. We know also that the Eberth bacillus may persist for many months in the gall-bladder, in periosteal abscesses (ten months after an attack of enteric fever), &c. There is no suggestion in this disease of renewed doses of external infection. Why, then, should the view that the occasional protracted infectivity of scarlatinal patients is caused by repeated doses of infection from without be entertained? In influenza the possibility that relapses and recrudescent infection may be due to external infection from other patients will often be tenable, in view of the fact that this disease usually runs rapidly through a family. But the influence of hospitalism cannot be urged; furthermore, all the available evidence tends to show that the contagium of this disease, like that of measles, is very short lived when exposed to desiccation and the influence of light and air. In this disease, therefore, as in the others, the most reasonable hypothesis is that relapse and recrudescent infection are due to the revived activity of specific micro-organisms stored in the patient himself, and derived from his own initial attack; relapse or recrudescent infection being brought about by some factor which temporarily removes or reduces the slight degree of immunity already established.

"It is unnecessary further to summarize the converging lines of evidence . . . which suggest the inference that protracted and recrudescent infection, when they occur in scarlet fever, are due, as in diphtheria, in enteric fever, and in other infectious diseases, to the prevalence of a particular type of disease and to conditions inherent in the patient himself, and not to conditions, whether hospitalism or other, external to the patient."

Dr. BUTLER, in reply, said he could not complain of lack of criticism, and he was particularly indebted to Dr. Goodall for reference to the cases connected with the schoolboy. He had not himself come across a series of cases which so indicated and illustrated the main thesis of the paper as did that. Dr. Goodall misunderstood him if he supposed that his mind was so fully made up on the question of the possibility of the spread of infection by fomites that he would be prepared to advocate that disinfection should no longer be carried out. He still thought there was not sufficient evidence to justify the forgoing of disinfection after the occurrence of scarlet fever in a house, and until there was sufficient evidence to justify the experiment, he was far from thinking that fomites as a source of infection should be put out of court. In nothing which he had said that evening did he suppose he was original, if by that was meant saying something new; but everything he had said was original in the sense

that it was what he thought. Moreover, the claim to originality presupposed familiarity with the work of every preceding investigator, which he did not claim. He had been often struck with the way in which men worked along parallel lines unknown to each other. He was almost horrified at a possible charge of plagiarism when, a week ago, he turned up a paper, after his own was in the printers' hands, by Dr. Berry, showing cases which illustrated better than his own the very points which he had been contending for that night. Dr. Parsons and Sir Shirley Murphy had questioned whether the difference in the columns in the diagram was not largely to be accounted for by the difference in the age-distribution. He could not tell whether that was due to the different ages of the inmates of those houses, but as he had pointed out in the paper, differences in age-distribution of the inmates of houses previously infected as compared with all other houses in the district should show themselves in uniform differences in the lengths of the compared columns, whereas this was only apparent in the later years of the period. During the first three years after the occurrence of the primary cases in infected houses the differences in the lengths of the compared columns progressively diminished. In reply to Dr. Turner he had to say that the curve shown on the diagram was constructed so as to include return cases, and he agreed with him that it was surprising the curve showed no hump in consequence. Dr. Hamer complained that the statement as to the flora of the naso-pharynx retaining their acquired toxic properties when planted out among contacts was a mere *ipse dixit* unsupported by evidence. It was true that until the toxic species of this flora was identified the truth of this statement would not be bacteriologically demonstrable, but that was not to say that there was no evidence of its truth. It was a matter of common observation which he should have thought would not have been questioned that colds spread rapidly by contact-infection when introduced into a household, and he himself had repeatedly observed this to occur when the initial cold had been contracted as a result of exposure to chill. Actual demonstration of infectious spread was scarcely possible, but the inference was irresistible when we found the mere commingling of the sick with the healthy was invariably followed by the spread of an ailment the infectiousness of which was only known by this observed sequence of events. He had not set out to explain every feature of scarlet fever, and what he had submitted was a view which he found more generally in harmony with the facts than other views which attempted such explanation. He had to thank Dr. Newsholme for his criticisms of the paper. In drawing an analogy between variations in individual susceptibility and community, susceptibility to specific infections due to the immunity constitution of its members, as modified by their zymotic history, he did not think he could properly be charged with dogmatism, nor again did he think he was dogmatic in saying that "return cases do not depend for their infectivity upon hospitalism—a fact disproved by their occurrence among home-isolated cases." Such a statement, particularly when taken with its context, might be made by one who accepted hospitalism as one of the factors in the causation of some return cases. The statement did not reduce all

the difficulties in the epidemiology of scarlet fever to a simple proposition ; it merely insisted upon the fact of the prolonged infectivity of the disease irrespective of its method of isolation and without prejudice to the part played by conditions arising out of the manner of isolation, which might or might not modify such persistence. The fallacies in the table which had been referred to were fallacies of inference which the table might not warrant. The table was a mere statistical presentment, which would have to be interpreted very carefully. He gave it for what it was worth. It was impossible with the data at his command to give more than a crude statistical result. The concrete facts on which it was based perforce had been ignored. There were changes in the population living in the houses to which the figures related, anomalies of age-distribution, errors of diagnosis, and many other circumstances which detracted from its value. They must give pause to the inference which the table seemed to suggest. But such as it was, he did not think it altogether useless. It was possible only to record such facts as were available. The fallacies to be avoided were erroneous inferences.

Epidemiological Section.

January 22, 1909.

Dr. A. NEWSHOLME, President of the Section, in the Chair.

Some Bacteriological Problems Considered from an Epidemiological Point of View.

By W. H. HAMER, M.D.

THE paper which is herewith presented for the consideration of the Epidemiological Section must be regarded as a preliminary exposition of some of the difficulties which beset the student of epidemiology in studying some of the later phases of bacteriological development. It is, in fact, an attempt to raise a discussion which may prove helpful, not only to the author himself, but also, possibly, to other students who may have experienced similar perplexities. It is proposed in the first instance to discuss the nature of the difficulties to be inquired into; then to consider some possible methods of dealing with these difficulties; and finally to make some preliminary observations with regard to the application of these methods.

THE NATURE OF THE DIFFICULTIES TO BE INQUIRED INTO.

As knowledge has, step by step, been acquired with regard to pathogenic organisms it has become more and more apparent that, if a secure foundation upon which to build up a complete explanation of the phenomena of epidemics is to be laid down, a critical attitude must be maintained with regard to acceptance of germs commonly found in association with particular diseases as the "causal organisms" of those diseases. The student of epidemics, concerned as he is with long periods of time and with the inhabited globe, has found it increasingly difficult to reconcile his conceptions with some of those which have been held by

investigators who have been dealing for the most part with outbreaks having very limited distribution in time and space. But putting considerations of this kind on one side, there are exhibited, by one and another school of bacteriologists, differences in mental attitude with regard to the question of the relation of germs to the production of outbreaks of disease not a little disturbing to those interested in this subject. Thus there are doubtless many bacteriologists who hold, in accordance with the description of the orthodox position given by Hueppe, "that the pathogenic bacteria are specific entities, that they are the true and sufficient cause of disease, as Pasteur and Koch have affirmed;"¹ and there are not a few who further entertain belief in "unvarying specific character and physiological effect" (*loc. cit.*). These positions, be it noted, are, of course, far from being those assumed by Hueppe himself, for he writes: "Upon sifting all the available material I cannot find a fact which is in real harmony with Koch's conception of 'specific disease germs.'"²

In view of the striking divergencies in method of approach exhibited, it is not to be wondered at if difficulty comes to be experienced in reconciling differences which arise. The following instances may be given:—

(1) While, on the one hand, on passing in review the epidemic prevalences of successive epochs, striking evidence of persistency of disease types throughout the historical period is discoverable, differences are again and again detected, on the other hand, in the germs found in association, at one and another time, with individual outbreaks, and there is a tendency to consider that such differences have specific value. Thus, if regard be paid to the claims originally made for the

¹ "The Principles of Bacteriology," Jordan's Translation, 1899.

² I am indebted to Dr. F. W. Andrewes for reference to Hueppe, and to his four conditions, which must be fulfilled if pathogenic bacteria are to be regarded as "specific entities" . . . as "true and sufficient causes of disease:" "First, the disease-producing bacteria should exert no other effect than that of producing disease; second, their ability to produce disease should remain constant; third, they should affect all animals in the same way without reference to particular species; and fourth, they should produce only a single sharply defined typical 'specific' infectious disease." "In such a way as this," says Hueppe, "Koch has really pictured things to himself." According to Hueppe, "micro-organisms change with the changes in their surroundings, and the placing of this fact on a sure footing constitutes the great advance that modern bacteriology has made beyond the standpoint reached by Koch." Hueppe declares that "the disease germ is only apparently the 'essence' of an infectious disease, that in reality, here as elsewhere, a true internal cause is to be found, inherent in the internal organization of men;" . . . the germs merely "liberate impulses" and set free "predispositions towards disease." It will be seen in the sequel that, whether stress be laid upon the "specific germ" or upon the "predisposition towards disease," there are a number of outstanding facts which neither bacteria nor predispositions sufficiently explain.

agglutination test, it becomes necessary to assume a multitude of special creations, so to speak, of "causal organisms" of "meat poisoning"—one, in fact, it may almost be said, for every German town in which an outbreak which is bacteriologically investigated occurs. Again, recent work on cerebrospinal meningitis seems to show, if regard be paid to all the distinctions drawn between Gram-negative and Gram-variable cocci, and to all the slight differences observed on application of the agglutination or fermentation tests, that many distinct "strains" of organism are operating to produce the disease in different parts of the world; if, indeed, it be not found necessary to lay down the rule—so many outbreaks, so many meningococci.¹ The most striking example of the kind of difficulty here in question is, however, afforded by the case of influenza, for the prevalences of this disease studied in bacteriological times have been sometimes clearly bound up with, and sometimes apparently entirely dissociated from, the supposed "causal organism"—the bacillus of Pfeiffer.

(2) A second instance in point is the difficulty presented by the notably profuse distribution of "causal organisms" without corresponding production of disease. For example, in phthisis there is a possible daily discharge of some billions of bacilli from a single patient; and yet, in face of this fact, it has to be realized that authorities differ as to the existence of finally conclusive evidence of direct case-to-case infection. It remains to be added that the *Bacillus tuberculosis* is widely distributed in milk and in butter, to say nothing of meat, and is therefore, of course, consumed almost daily by everybody. Similarly, in typhoid fever, the "bacillus-carrier" harbours germs for as long a period as fifty-two years² with, apparently, no evil result, until, on the occurrence of typhoid fever among persons with whom he (or more often she) has been more or less associated, suspicion is aroused, bacteriological inquiry is instituted, and then at length the "carrier" is declared to have become "effective." In diphtheria, typhoid fever, and cholera it becomes necessary to claim an exceedingly wide distribution for the assumed (potential) "causal organisms" among healthy persons; while, to take one more instance, in leprosy also the bacilli are, of course, produced in enormous numbers, though in this case the difficulty of explaining why the disease is not highly infectious from person

¹ Bochall (Zeitschr. f. Hyg. 1908, lxi, p. 3) speaks of the agglutination test as the "key-stone" of the diagnostic arch. Ghon, Mucha and Wiesner (Centralbl. f. Bakt. Leipz., 1908, xlii, No. 19-21, Referate) find the test valueless. In their experience meningococcus-sera agglutinate gonococci and gonococcus-sera meningococci.

² Lancet, 1908, ii, p. 492.

to person is met by assuming that the organisms contained in cells in the skin have lost their infective power.

(3) The germ theory does not, it must be admitted, afford a wholly satisfactory explanation of the close relationship between certain diseases—for example, scarlet fever and diphtheria, or typhus and relapsing fevers; or, again, between different forms of one and the same disease—for example, influenza in its cerebrospinal and in its respiratory or other forms, septicæmic and bubonic plague, malignant pustule and wool-sorter's disease; and other instances could, of course, be cited. In a possibly related way the difficulties of symbiosis and metabiosis have as yet failed to find satisfactory solution. Beyond this, there are the facts which are ordinarily explained (for example by Hueppe, loc. cit.) as resulting from "immunity" or from "predisposition." "How comes it," asks Gottstein (*Allgemeine Epidemiologie*, 1897), "that one man who is attacked by the bacillus becomes tuberculous, while another man remains healthy? Why in cholera epidemics is only so small a proportion of the populations affected attacked by the disease?" Questions of this sort open up difficulties which it is by no means easy to deal with in an altogether satisfactory manner.

At this stage it may be useful to consider how far it can be said that the requirements, laid down by Koch as being those which a "causal organism" must fulfil, have been rigorously adhered to. The four original conditions have been elaborated in the light of later knowledge, and they are set out in the left-hand column below in their most recent form (as given by Ritchie); while for purposes of comparison there are placed in juxtaposition (in the right-hand column) certain well-recognized facts which appear deserving of study in relation with these conditions.

(a) The bacterium must be invariably found in the tissues of an animal dead from or affected with the disease in question.

This condition is not fulfilled by "causal organisms" in all instances. When it is not complied with, it is not unnaturally assumed that a sufficiently prolonged search was not made, or that such search was not carried out at the proper time. If fulfilled, the condition does not, of course, serve to finally decide the question: Causal organism or secondary invader?

- (b) It must never occur in other diseases or in normal tissues.

- (c) The organism transmitted from the diseased or dead animal to an unaffected animal of the same species must produce lesions similar to those present in the animal from which it was derived; and in this second diseased animal the original organism must be found.

- (d) If the organism can be cultivated outside the animal body, then an artificial cultivation, inoculated experimentally into an animal, must again produce the disease, and this animal must again contain the organism in its tissues or blood.

The latter, like the former, may, *ex hypothesi*, be expected, generally speaking, to comply with condition (a).

The organisms of cholera, typhoid fever, and diphtheria, and other "causal organisms" are frequently met with in healthy persons. Hence the necessity for recognition of healthy "carrier cases."

There is difficulty here in interpreting the words "lesions similar."

In the cases of cholera and typhoid fever, for example, are the lesions produced on inoculation of animals to be regarded as "similar" to those developed in the course of cholera or typhoid fever in man? The organism admittedly causes "disease" in the experimental animal, just as it is admittedly concerned in the production of certain symptoms in man. The question at issue, however, is: Does the similarity of lesions *when the whole course of the diseases is passed in review* compel belief in the supposed causal relationship?

There is the same difficulty here as that referred to under (c). The tubercle bacillus, for example, produces in experimental animals "lesions similar" to those found in the natural tuberculosis of man and other animals. The question, however, arises: Does the tubercle bacillus *originate* tuberculosis in the animals last named?

It is noteworthy that, even in the case of the eminently susceptible guinea-pig, the tubercle bacillus is said never to *originate* tuberculosis, as long as the animal is living amid natural surroundings. It is only under the quite peculiar conditions of the laboratory that tuberculosis of guinea-pigs has been observed.¹

(e) It is an additional link in the chain of evidence when toxins having a specific action can be isolated from cultures of the bacterium, and when these reproduce in animals the characteristic features of infection with the living organisms.

This condition has been added to Koch's four original conditions. It will be realized, on consideration, that it affords little, if any, help in differentiating between "causal organisms" and "secondary or terminal invaders." Thus, despite all the detailed knowledge acquired concerning diphtheria toxin, there is no nearer approach to settlement of the exact relation to the disease, whether as "causal organism" or "secondary invader," of the Klebs-Löffler bacillus.

(f) These processes must occur in invariable succession under identical conditions.

This requirement appears to stand or fall with the other conditions. It would seem that it has added little, if anything, to the strength of the case as determined by them.

Dr. Ritchie, after formulating the above-named conditions, proceeds by way of summary: "In human diseases it is, of course, rarely possible to establish the causal relationship between a bacterium and a disease by strictly following these canons." "But," he says, "a practically certain proof is attainable in such cases by establishing canons (a) and (b), by then reproducing the disease in animals, and satisfying the remaining canons in the experimental inoculations." He adds: "This

¹ See Sidney Martin, "Appendix to Report of Royal Commission on Tuberculosis," par. 12. Also Gottstein, "Allgem. Epidem.," p. 166.

is what has happened in the case of tuberculosis." He is careful to point out, however, that "the more the effects produced in animals differ from those obtaining in man, the greater care must be exercised in drawing conclusions, and the difficulty of the situation may be further increased by the *non-specific nature of the lesions of the original disease*" (the italics are not in the original article).

The question becomes yet more complicated when account comes to be taken of the recent discoveries of "terminal invaders," on the one hand, and "healthy carriers" on the other. Obviously mere compliance with canons (a) and (b) does not now afford "practically certain proof"; reliance must of necessity more than ever be placed upon the extent to which "the remaining canons in the experimental inoculations" are satisfied. But here there stands up in bold relief the very formidable question as to the specific or "non-specific nature of the lesions of the original disease." A good example is that given by Dr. Ritchie, viz., "Pneumonia": "In man there is usually a lesion strictly confined to the lung . . .; from the great majority of cases the pneumococcus can be isolated. When this organism is injected into an animal, such as the rabbit, it will produce, even if inoculation into the lung be practised, not a pneumonia but a general septicæmia." Similar instances are forthcoming in the case of the ordinary pyogenic organisms, or again in that of species allied to *Bacillus coli*. Study of the last-named group of organisms, in particular, has quite recently shed new light on this very obscure field of inquiry.

ON THE POSSIBILITY OF OBTAINING A CLUE TO METHODS OF DEALING WITH THE AFORE-MENTIONED DIFFICULTIES.

The idea that importance must be attached to "secondary invaders" is by no means new. Already, in 1894, Gottstein (loc. cit.) had suggested that pathogenic germs might be merely playing the part of "associated organisms" (jeweiligen Begleiter) in established conditions of disease. More recently much has been written on "symbiosis." O. Liebreich, again, has developed the notion of "Nosoparasitismus," claiming that the pathogenic germ is "nicht ein Parasit an sich, sondern ein Parasit der Krankheit, ein Nosoparasit." According to this view, disease, or *nosos*, is that condition of the cell in which the external agent is able to accomplish the cell's destruction; and specific organisms are not able to set up disease processes in the body by their own unaided activity, but can only do so when they chance to co-operate with other

injurious influences impairing bodily resistance. The subjects of "associated organisms" and of "nosoparasitismus" have in the light of recent discoveries come to assume added importance.

Thus the modification in point of view necessitated with regard to one of the organisms allied to *Bacillus coli*—the hog-cholera bacillus (or swine fever bacillus, as it is generally called in this country)—is a striking modern instance which may be first considered in this connexion. This bacillus is now known to be, generally speaking, a normal inhabitant of the pig's intestine which assumes importance in conditions of disease. It was the discovery, in the first place, of the fact that the actual virus of hog-cholera is a "filter-passer" which raised suspicion; the further result was then later elicited that while both the naturally produced disease and that set up by inoculation of infected blood are infectious from animal to animal, the disease induced by inoculation of cultures of the "terminal invader," though it resembles hog-cholera in most respects, is not thus infectious. Hottinger¹ lays stress upon this last-named difference, and urges that the four conditions laid down by Koch as determinatory of the "specificity" of an organism should be enlarged, in the case of diseases such as hog-cholera, by the addition of a requirement that the infectious character be exhibited by the inoculated animals. Or, speaking more generally, he adds: "The condition should be insisted upon that the artificially produced disease must fulfil the requirements of epidemiology." On application of such a condition the hog-cholera bacillus fails to pass muster.

Further than this, inquiry has been made in Germany concerning *Schweineseuche* (or "swine plague," as it is called in England, in contradistinction to "swine fever"), a disease (with lung complications) affecting pigs, often confused with hog-cholera (swine fever), and supposed hitherto to be due to a distinct causal organism. The outbreaks of *schweineseuche* are, it is now agreed, really outbreaks of hog-cholera in which a second organism, once regarded as "causal," but now believed to be merely a "terminal invader," appears upon the scene and plays its part. It is stated, moreover, that the weakened resistance of the intestinal wall enables yet other common intestinal bacteria to obtain access to the blood and tissues of affected animals (see p. 98).

Sir John M'Fadyean, in discussing the swine fever bacillus in a series of papers, to be later more particularly referred to, says: "Some authors have recently belittled the role of the swine fever bacillus and

¹ *Centralbl. f. Bakt.*, July 15, 1908.

have denied that it is in any degree responsible for the lesions which are characteristic of the disease. Such a view is, however, far from being established by ascertained facts, and it appears to be much more probable that the bacillus is mainly responsible for the diphtheritic and ulcerative processes which occur with such constancy in the large intestine in cases of swine fever." "Then," he adds, "assuming . . . the lesions . . . are secondary in their nature, and caused by an organism which does not constitute the actual cause and contagium of the disease, the question naturally arises, May not this be true for what are regarded as the essential lesions of some other diseases?" He further says: "When anyone, as part of the evidence proving that an organism is the cause of a particular contagious disease, claims that he has successfully employed pure cultures of that organism to reproduce the disease, he ought to show that the experimentally induced disease, when afforded the opportunity, spreads by contagion like the natural disease." "And," he continues, "when one reflects on the nature of the evidence on which it has been held, and very generally accepted, that particular bacteria are the cause of contagious diseases, it becomes manifest that in some cases the proof is no more complete than it was in the case of swine fever. It is, therefore, not improbable that future investigations, conducted on the lines necessary for the detection of an ultra-visible virus, may bring on certain bacteria, at present accepted as the cause of disease, the discredit which has already fallen on the so-called bacilli of swine fever, swine plague, and canine distemper."

It is but natural that, in the light of these observations with regard to the hog-cholera bacillus, attention should now be turned to the part played by a very nearly allied if not identical organism, *Bacillus paratyphus* (B), in connexion with disease in man. The position of affairs, so far as this organism is concerned, has been thus described by Jürgens: ¹ "Clinical manifestations cannot be relied upon to delimit the range of paratyphoid; pathological anatomy cannot fix its boundaries. The etiological factor, embodied in the paratyphus bacillus, alone is capable of combining these varying symptom-groups and pathological appearances in one unified conception." Again, Uhlenhuth and Hübener, in a recent communication on hog-cholera, ² after reviewing the evidence concerning the bacillus of hog-cholera, and dealing with criticisms raised by von Lourens and others, turn to consider *Bacillus paratyphus* (B). They point out that healthy persons, who have not ascertainably been

¹ "Ueber typhusähnliche Erkrankungen," *Deutsch. med. Wochenschr.*, January 3, 1907.

² *Centralbl. f. Bakt.*, 1908, Beilage zu Abth. 1, xlii, Referate.

exposed to paratyphoid infection, present *Bacillus paratyphus* (B) in their excretions and blood, and they add that Rimpau has isolated this bacillus from food materials (sausage) known to have been eaten without producing ill effects. Uhlenhuth and Hübener thereupon conclude that the discovery of the *Bacillus paratyphus* (B) in the stools of patients does not necessarily prove that they are the subjects of paratyphoid, and further that the discovery of the bacillus in food materials, and in connexion with disease set up by eating such materials, does not, without further evidence, demonstrate causal relationship between the bacillus and the disease.

It is interesting to consider, now, how these authorities deal with the typhoid bacillus itself. Jürgens writes: "Typhoid fever is produced only when, in addition to the exciting organism, certain other etiological factors operate" (loc. cit.); and again: "The more closely we examine the part played by Eberth's bacillus the more necessary becomes the appeal to co-operating causal influences"; while once more he says: "There are clear indications that typhoid diagnosis has to some extent gone astray on a path which sooner or later will be found impassable, and we shall be compelled to retrace our steps."

Uhlenhuth and Hübener (loc. cit.), after pointing out that not only hog-cholera bacilli, but numbers ("einer ganze Reihe") of other bacteria, are able to invade the organs and muscles of swine from the intestine, in the course of development of hog-cholera, go on to say that this is due to loss of resistance induced by the diseased condition; and they adduce as analogous cases, scarlet fever, yellow fever, and brust-seuche of horses. They then ask, Does something of the same kind occur in typhoid fever?—and they refer to the work of Busse bearing on this question. This last-named writer¹ declares that observations made during the past year, in Posen, place the fact beyond doubt that typhoid bacilli occur in diseases other than typhoid fever. He describes in minute detail four cases in which typhoid bacilli were found in the blood of persons having either tubercular ulcers of the intestine or else presenting evidence of intestinal catarrh, but exhibiting no sign of the presence of typhoid fever. He says: "I take it that the intestinal catarrh or the actual ulcers in the intestinal mucosa facilitate passage of the bacilli from the intestine into the blood." He refers to the fact that his experience is confirmed by a case recorded by von Krehl, and that somewhat similar cases have been described by Jürgens. These cases show,

¹ *Milch. med. Wochenschr.*, iv, p. 1113, 1908.

he says, that proof of the existence of typhoid bacilli in a patient's blood does not necessarily indicate that the patient is the subject of typhoid fever.

On the general question, as to whether when typhoid bacilli obtain access to the circulating blood they produce ill effects and influence the course of an established disease, Busse considers, "We can say nothing"; but one thing, he notes, may be safely asserted: "When an intestinal disorder occurs in a bacillus-carrier, with result that typhoid bacilli find their way into the circulation, typhoid fever is not thereupon produced." ("Zum Ileotyphus wird eine Darmerkrankung eines Bazillenträgers auch dann noch nicht, wenn die Bazillen in das Blut gelangen, und hier kreisen.") His final summary is: "Even when there are strong reasons for suspecting the presence of typhoid fever, discovery of typhoid bacilli in a patient's blood affords no certain proof of the existence of the disease."

Recent investigations have in fact led a number of observers to modify the view at one time generally held concerning typhoid fever. Thus Dr. Dudgeon, in his Horace Dobell lecture, states that "most people are agreed, at the present day, that by the term typhoid fever we understand certain clinical phenomena which are due to infection by various micro-organisms belonging to the typhoid group, and not necessarily to the specific micro-organism the typhoid bacillus." Similarly Major Statham¹ writes: "There are quite a number of allied but distinct species of bacteria, any one of which is capable of originating typhoidal disease; in fact, the etiology of this condition is as complex as its symptomatology." Again, Dr. Peabody² expresses a similar view. Question thereupon arises whether the difficulties of those who maintain that the organisms are "causal" are lessened by thus widening the circle and admitting "allied but distinct species." Recent work, such as that of Houston and Savage, on application of the latest approved methods to differentiation of *Bacillus typhosus*—and bacilli of the Gaertner group—from "allied but distinct species," and a paper such as that of Baumann,³ afford some indication as to the nature of the problems which must be faced if the circle be thus enlarged.

In connexion with study of organisms such as those referred to in the last two or three pages, there has been exhibited, as a rule, unwillingness

¹ *Journ. Roy. Army Med. Corps*, xi, No. 4.

² *Journ. Amer. Med. Assoc.*, 1908, li, No. 12.

³ "Beitrag zur Kenntniss der typhusähnlichen Bazillen," *Arb. aus dem Kaiserl. Gesund. Berl.* 1908, xxix, p. 372.

to admit the closeness of approximation of pathogenic to allied non-pathogenic forms. Hueppe, on the other hand, representing the minority, insists that individual bacteria adapt themselves to the "milieu," and he says: "The constancy which Koch's method revealed was not one of the permanence of species, but rather one of permanence of varieties due to station." As an extreme instance of the opposite view, the teaching of a few years ago on the subject of an organism nearly related to the hog-cholera bacillus—Sanarelli's bacillus—may be referred to. The discoveries of the part played by a stegomyia in disseminating yellow fever, and of the fact that the virus itself is a filter-passer, have now quite discredited Sanarelli's bacillus, and it is perhaps worth noting, therefore, that prior to these discoveries being made importance was by many attached to the *Bacillus icteroides* and to the supposed characteristic tests held to differentiate it from allied species. The criteria relied upon have, as time has tried them, proved altogether elusive. Hottinger has, indeed, pointed out that the differences are ultimately narrowed down to this: Sanarelli's bacillus only occurs in yellow fever. A similar bacillus, occurring elsewhere, was not styled Sanarelli's bacillus, but given, says Hottinger, a name corresponding to its habitat ("den zum Habitat passenden Namen").

In France and Italy more especially there has been again and again displayed a tendency to question the views held by the majority of bacteriologists and indeed to speculate as to the possibility that *Bacillus typhosus* may be nothing more than a variant of *Bacillus coli*. Tarchetti,¹ four years ago, in the *Centralblatt für Bakteriologie*, 1904, xxxvi,

¹ Tarchetti directs attention first to the strange fact, as he terms it, that the virulence of the classical *Bacillus typhosus* is less than that customarily exhibited by *Bacillus coli*. The former, it is true, can apparently be exalted by "passage" through animals, but Tarchetti raises question whether in such experiments opportunity may not be afforded to the normally present *Bacillus coli* to penetrate into the peritoneal cavity, and even to outgrow the *Bacillus typhosus* there, so that, in the final result, it is the virulence of *Bacillus coli*, and not that of *Bacillus typhosus*, which has been augmented. Secondly, he says, there is no single characteristic which can be relied upon for differentiating between the two kinds of bacilli. Thirdly, they approximate closely to one another when grown on the ordinary artificial media. Fourthly, when cultivated under unfavourable conditions the agglutinability and rate of growth show a sort of inverse relationship in both instances—the more free the growth the less the agglutinability, and vice versa. Fifthly, the alterations which can be produced in *Bacillus coli* by "passage" seem to show that from widely separate varieties intermediate forms far less clearly differentiated from one another may be produced. Further, Tarchetti has criticized the results obtained by observers who have attempted to isolate *Bacillus typhosus* from artificial mixtures of *Bacillus typhosus* and *Bacillus coli*. He argues that it must be borne in mind, when special methods designed to repress the growth of one or another variety are adopted, that these methods may have an influence upon the characters exhibited by the varieties which are not suppressed. He thinks that when, by special methods, typhoid bacilli are isolated from the stools of typhoid patients, it may be that the two combined influences—that exerted upon the flora of the intestine by the presence of the diseased condition, plus that

Referate, p. 307, pleaded on behalf of this view, and von Benczur (*Centralblatt für Bakteriologie*, 1908, xlviii [originale], p. 276), while affirming that he cannot obtain laboratory evidence confirmatory of Tarchetti's thesis, frankly admits that, under "certain special circumstances within the body," it is quite possible that the transformation may be effected.

That a bacillus may lose its pathogenicity has long been a commonplace; that it might acquire pathogenicity was, until comparatively recently, to some at any rate, unthinkable. Even Hueppe, when noting that the ability of a bacterium to produce disease is variable, writes: "It can diminish; but it can also," he guardedly adds, "under some conditions, increase." With ever-accumulating evidence of increase, if not of acquirement, of pathogenicity by "passage," the point of view has been materially changed. The work of the English Tuberculosis Commission, in particular, has greatly modified the general attitude with regard to such questions. Quite recently Twort has opened up new ground by showing that the power of fermenting sugars can, under certain conditions of growth, be taken up or cast off by some organisms. In a still later paper² Reiner Müller (after referring to previous work by M. Neisser on *Bacterium coli mutabile*) gives account of similar organisms found in the Kiel laboratory by A. Burk and H. Gräf; cultivations of these germs on lactose-containing media were found to present daughter colonies (Knöpfe), yielding a differing ferment reaction from that of the original organism. Reiner Müller noted that, in a similar way, typhoid bacilli formed knöpfe when grown on isodulcit, and in some instances these daughter colonies exhibited changed ferment property. This Reiner Müller regards as evidence of

exerted upon it by the culture medium used—may result in production of a weakened and less luxuriant variety of *Bacillus coli*, one possessing, in fact, the negative characteristics of *Bacillus typhosus*. He believes that *Bacillus typhosus* under particular conditions, observed clinically, may revert towards the original type of *Bacillus coli*. He concludes, therefore, that this protean bacillus, which is normally a harmless intestinal parasite, may under appropriate circumstances assume pathogenicity; and that when it thus invades the tissues it shows loss of virulence (as exhibited in animal inoculations), less luxuriant growth on culture media, and enfeeblement of certain of the activities ordinarily possessed by it. Tarchetti is, however, convinced that the isolation in a case of typhoid fever of the classical *Bacillus typhosus*, exhibiting none of the reactions of *Bacillus coli*, is by no means so common an event as is generally supposed, and that, when it does occur, the result may be in part due to the influence exerted by the culture media employed. He says attention has too largely been concentrated upon the attempt to demonstrate the existence, outside the body, of an ideal form of *Bacillus typhosus*, possessing a number of characteristic differentiating marks, and but little regard has been paid to the really important question, which is: How are we to distinguish active and virulent from inactive and harmless forms of *Bacillus coli*?

² *Centralbl. f. Bakt.*, 1908, Beilage zu Abth. 1, xlii, Referate.

occurrence of a "mutation," to use de Vries's term. It is, indeed, remarkable that so much importance has been attached in the past to the fact that, under laboratory conditions, these "mutations" are difficult to produce, and that, arguing from this difficulty, it has so insistently been maintained that they never occur. Dr. Pavy, in his recent lectures on "Diabetes," has reminded us that "in our laboratory procedures we lack the power of forging the implements that Nature in her building up and breaking down operations works with. When we get hold of and employ these implements—as, for instance, when we operate with 'enzymes'—we can achieve in our laboratory experiments the same that is effected within the living organism." Or again: "Nature with her 'enzyme' may be able to provide the requisite adaptability for union, whilst the laboratory worker, unsupplied with the 'enzyme,' may be unable to do so."

From all these researches the fact clearly emerges that "strains" which are closely related, if not identical from most points of view, may stand widely apart as regards promotion of a particular ferment change. It has been, until quite lately, generally held that specific differences are here in question, but it must now be realized that consideration of the hypothesis, elaborated by Ehrlich in a somewhat similar connexion, suggests it is possible that the altered behaviour results from transference of something not essential to the species; it may be of an "activator," or an "amboceptor," or an "enzyme," which must be looked upon as attached to the bacillus after the manner, so to speak, of a "side-chain" (to use Ehrlich's terminology). On such a view of the matter the difficulties of Jürgens, who has to appeal to an "etiological factor embodied in the bacillus," would be met. There is, in fact, ready to hand an hypothesis which not only affords a simple explanation of the otherwise bewildering relationships (*qua* fermentation processes) of strains of bacteria (*e.g.*, *Bacillus coli* and various cocci), but which may also be applied to certain difficulties with regard to the phenomena of agglutination, &c.; and which last, but not least, may be extended in the field of disease to elucidation of the gain or loss of pathogenicity.

There is thus reached a possible conception of the symptoms making up a disease picture as being due, not to highly specialized "causal organisms," but rather to acquirement on the part of non-pathogenic (it may be of well-known ordinary saprophytic) organisms of pathogenicity, by virtue of their becoming linked to an "activator" or "enzyme"; the latter may be looked upon as an attached "side-chain," or it may alternatively be regarded as a parasite of what is now itself, thanks to its ally, a

pathogenic form. The harmless parasites thus transformed into disease germs may, on such a view of the matter, be, say, in the case of pneumonia, the *Diplococcus lanceolatus*, one of the streptococci, or Friedländer's or other bacillus; in the case of throat malady, the *Streptococcus scarlatinae* or the Klebs-Löffler bacillus; in the case of influenza, Pfeiffer's bacillus, the *Micrococcus catarrhalis*, possibly Weichselbaum's meningococcus, or some other coccus; in the case of typhoid fever, one or other bacillus of the *Bacillus coli* group; in that of cholera, a saprophytic comma bacillus; and there may even arise question as to adding, in that of tubercle, a bacillus capable of taking upon itself the acid-fast and other appropriate characters.

On the hypothesis here outlined, the view now so frequently expressed, that healthy persons are apt to carry about with them germs which may prove capable of playing a part in the production of the symptoms of disease, appears to find justification. Dr. L. S. Dudgeon, in his recently delivered Horace Dobell Lecture, has discussed the "latent persistence and the reactivation of pathogenic bacteria in the body." He describes cases illustrating "the persistence of cocci in the tissues, for indefinite periods, without giving rise to ill effects, except during intervals of reactivation." Again he says: "The streptococci met with in many of our common diseases may, as regards cultural and certain other properties, be indistinguishable from the streptococci normally present in the human body." The pneumococcus and the gonococcus, he notes, "may remain in the tissues for indefinite periods without apparently producing ill effects"; and so also the Klebs-Löffler bacillus, *Bacillus proteus*, *Bacillus coli*, and *Bacillus typhosus* may similarly persist, "without giving rise to any symptoms, but occasionally acting as the cause of some acute illness." Dr. Dudgeon believes that "auto-infection" by these various organisms may be the cause of disease being set up. But the question thereupon arises: Why does the auto-infection occur? Why, on such an hypothesis, in a milk outbreak of diphtheria, scarlet fever, or typhoid fever, are large numbers of persons simultaneously auto-infected? Why in a school outbreak of diphtheria is the disease limited to particular departments (or even class-rooms), the children in these class-rooms infecting themselves, while those in other class-rooms escape, despite the now well-recognized fact that they also may be harbouring the Klebs-Löffler bacillus in their throats? It seems necessary to postulate "something," not pre-existent in the child or in the bacilli, which helps to overcome the resistance of the former or to arouse the slumbering energies of the latter in these instances, and that

activating "something" must be ingested with the milk in the former case, and communicated to the scholars in some way or other from without in the latter. This tertium quid it seems reasonable to regard as the cause of the disease; the predisposition to attack is not in itself sufficient, while the slumbering germs for their part are mere "secondary invaders," capable, when they are activated, of producing some of the symptoms sooner or later to be associated with the disease picture.

It may, however, be objected—even granting that there is need for some third thing—How can a ferment play the part indicated? It is interesting to note how greatly opinion has changed on this question during the last half century. Fifty years ago the doctrine of a *contagium vivum* was struggling for recognition; now this doctrine holds the field. Yet, as Farr¹ has pointed out, zymosis and the verb from which it is derived occur in Hippocrates, and Sydenham, Morton and Willis "all announced more or less clearly the zymotic hypothesis." Dr. B. W. Richardson dismissed the claims of the *contagium vivum*, in 1859, in a paper "On the Theory of Zymosis,"² but within quite recent times the whole question has been dealt with anew by Professor Moore, of Liverpool University, in a paper entitled "A Chemical Theory of Certain Infectious Diseases."³ Dr. Moore argues that examination of the "observed facts regarding infection" yields no proof "that the infective agent is necessarily a living cell," and he says "the alternative view may at once be suggested that the infection is carried by a non-living chemical substance." He then enters upon a detailed inquiry as to the ability of a chemical substance, an enzyme, to comply with all the observed requirements, and he describes the action, which he says "is spoken of as autocatalysis," an action which "may proceed with infinite slowness until a sufficient amount of the product of reaction accumulates to give speed to the reaction, which then, accelerating as it goes, may finish with explosive violence." He adds, further, considerations as to incubation period, resistance of enzymes to heat and chemical disinfectants, and as to relative degrees of infectivity, for which reference must be made to his paper.

It becomes, in fact, necessary, if a line of reasoning such as that which has been outlined be acquiesced in, to take a step forward as regards the conception of disease. Originally diseases were classified according to their symptomatology, as agues, fluxes, sweats, and the like; then came

¹ Fourth Annual Report of the Registrar-General (1840-1), 1842, pp. 202-204.

² Trans. Epidem. Soc., 1863, i, p. 20.

³ Journ. State. Med., 1904, xii, p. 193.

pathological anatomy, with its false membrane, giant-cells, and the rest; pathological anatomy in its turn gave way to the germ theory and the bacillus. But it is impossible to regard this as the final goal in all instances. No one is content to do so, for example, in smallpox; everybody looks further than to the pus-cells and the pyogenic organisms for the cause. In other diseases organisms once regarded as causal have now no longer such significance—for example, in the case of malaria, Klebs and Tommasi Crudeli's bacillus; in that of syphilis, Lustgarten's bacillus; in that of cholera, Emmerich's bacillus; in that of yellow fever, Sanarelli's bacillus; in that of hog-cholera, Salmon and Smith's bacillus and the bacillus of schweineseuche and many other organisms all have been relegated to the position of "secondary invaders." It may be urged that, in the light of these considerations, it is necessary to take stock of the position anew with regard to diseases such as scarlet fever, diphtheria, typhoid fever, cholera, &c.

POSSIBLE RESULTS OF APPLICATION OF THE FOREGOING PRINCIPLES.

From one point of view—that of the investigator who seeks explanation of the facts of epidemiology—there is a great temptation to speculate as to whether ferments may not play an important part in connexion, at any rate, with some of the diseases which have just been mentioned, or, again, as to the possibility of the existence of non-bacterial causal organisms having phases in their life-history passed alternatively in the human body and in some intermediate host. With regard to such speculations it has been often deemed sufficient to apply the maxim: "As to things which do not appear, the conclusion is the same as to things which do not exist." But to this the rejoinder might, of course, be made that although an organism has not been cultivated on artificial media it may still have an existence, that the epidemiological phenomena demand explanation, and that while the germ theory explains why bacteria occur in a particular association, this bare fact would in itself be accounted for just as well by regarding the bacteria as "secondary" as it is by looking upon them as "causal." It seems, at any rate, therefore worth while critically to examine the position in the case of the chief epidemic diseases.

Diphtheria and Scarlet Fever.—These diseases are taken together because of their well-known epidemiological relationships. Their distribution in time and space, their apparent interchangeability in one and the same outbreak, their relation to season, to rainfall, to milk, and to

school attendance—all these afford strong epidemiological reasons for claiming some common cause or closely related causes. Beyond this, there are the facts noted, in 1886, with regard to diphtheria, by Gresswell,¹ and commented upon by several observers since that time, in connexion more particularly with examination of the "return-case" phenomenon. Thus Butler, in a recent paper,² has given detailed evidence of recurring incidence of throat malady in children occupying the same class-rooms or living in the same houses; while such recurring incidence is not, as a rule, observed in well-regulated isolation hospital wards. These phenomena in themselves suggest the possibility that some intermediate host—for example, a biting insect—may be concerned in producing the observed effects.

Again, study of recent bacteriological work appears to favour a belief that the difficulties attendant upon acceptance of the Klebs-Löffler bacillus as a mere "secondary invader" are by no means overwhelming, and there is then room for the streptococcus to enact a similar rôle in scarlet fever. The very fact that a streptococcus theory of scarlet fever—the streptococcus playing the part of "causal organism"—has been broached and has by many been rejected may perhaps raise a doubt in some minds with regard to the precise relation of the Klebs-Löffler bacillus to diphtheria; and, again, it must not be forgotten that no clear proof has hitherto been forthcoming that risk is incurred by susceptible persons who are compelled to associate with healthy carriers of diphtheria bacilli. If, on the other hand, the bacillus be regarded as a "secondary invader," the difficulties (so apparent to the student of work such as that of Graham Smith) experienced in differentiating between the Klebs-Löffler bacillus and closely related non-pathogenic or feebly pathogenic forms having a wide distribution and very commonly met with in healthy mucous membranes cease to have any serious significance. It is easy to understand how it happens that, provided the soil is once prepared, whether by some unknown specific organism or by a specific "ferment," the widely distributed "secondary invader" finds scope for its development, and the well-known bacteriological phenomena associated with an attack of diphtheria supervene. Under other conditions, the same cause of disease, operating in conjunction for the most part not with the Klebs-Löffler bacillus, but with streptococci or with other bacteria, brings about an outbreak of scarlet fever. On such

¹ "Diphtheria as a Chronic Malady," by D. Astley Gresswell, *Trans. Epid. Soc. Lond.*, 1885-6, v, p. 57.

² "The Recurrent Infectiousness of Scarlet Fever," by W. Butler, *Proc. Roy. Soc. Med., Epid. Sec.*, 1908-9, p. 59.

an hypothesis the well-known tendency to interchangeability of the two diseases may perhaps find explanation; it would apparently be necessary to assume that the nature of the malady set up in an individual becoming the subject of throat affection would be largely determined by the character of the organisms already present in his throat and capable of taking up the role of "secondary invaders."

Finally, it should be observed that "surgical scarlet fever" and "puerperal scarlet fever," perhaps even "milk-borne scarlet fever," fall into line on the theory now propounded. In all these instances, it may be claimed that there results from the disturbances (operation, childbirth, consumption of infected milk) a sequence of events similar to that set up in true scarlet fever; and opportunity is afforded for the "secondary invaders," which commonly operate in scarlet fever (streptococci, &c.), to come into action, just as in true scarlet fever their activity is stimulated or let loose. The fact may be recalled that *hog-cholera spreads from case to case*; not so the "symptom-complex" produced in the pig by inoculation of the hog-cholera bacillus. So here, in throat malady, it may now be noted that surgical scarlet fever, puerperal scarlet fever, and milk-borne scarlet fever, have all been found to exhibit only rarely, if at all, a capacity for spreading from case to case. In these affections, therefore, it may be conjectured that "secondary invaders" alone are at work, the original causal organism of scarlet fever possibly not coming into operation at all in such instances.

Typhoid Fever and Cholera.—These two diseases may be taken together, not, of course, from any suggested association, such as that just discussed in the case of scarlet fever and diphtheria, but because problems of one and the same sort are opened up on consideration of the relation of the two supposed "causal organisms" to the two diseases now in question. In both instances the pathogenic germs are closely allied to very widely distributed non-pathogenic forms; in both instances the question of "healthy carriers" arises; in both there is difficulty in explaining why, if the organisms cause the disease, their distribution in such enormous numbers in discharges does not lead to its wholesale dissemination by the individuals attacked; in both there is conspicuous rarity of the number of instances in which the germs have been found in food materials (water, milk, shell-fish, &c.) known to possess infective property.¹ The typhoid fever bacillus is already, it is true, detectable

¹ The upholder of the "alternation of generations" hypothesis will have in mind the possibility that a phase in the life-history of the causal organisms of both typhoid fever and cholera is passed in molluscs and in fish.

in the blood quite early in the disease (first week); but the "causal organisms" of typhoid fever must have already commenced to grow in the body some ten to fourteen days before this. Inasmuch, however, as the question of the relation of *Bacillus typhosus* to typhoid fever was discussed at some length at a meeting of the Epidemiological Section of the Royal Society of Medicine in March, 1908, it is unnecessary further to refer to it here.¹

Plague.—Here, again, it has been urged that the facts at present known concerning the plague bacillus do not furnish material for a full account of the epidemiology of plague. The question is, of course, much complicated by uncertainty as to the relative importance of the parts played by fleas, and by rats and other animals. Recent bacteriological work (*Journal of Hygiene*, May, 1908) shows that the *Bacillus pestis* is exceedingly closely related to common and well-known inhabitants of the bodies of rats, guinea-pigs, &c., and it might conceivably be argued that the difficulties attaching to the acceptance of the *Bacillus pestis* as a "secondary invader" are in a measure lessened in the light of such knowledge.

Tuberculosis.—In this disease (and a similar remark may be made as regards leprosy, commonly considered to be due to a closely allied organism) acceptance of a causal role for the *Bacillus tuberculosis* (or for the *Bacillus lepræ*) involves difficulties such as the following:—

(1) The absence, on the one hand, of clear evidence of "droplet infection," or of infection from inhalation of dust derived from dried sputum; and, on the other hand, the uncertainty as to the extent to which infection can be attributed to tuberculous milk, butter, and meat.

(2) The time of appearance upon the scene of the organism—its presence is of necessity only demonstrated, speaking generally, at a time when the breaking down of tissue has commenced. The early events, in commencing pulmonary phthisis, are deduced from what is known of spreading tuberculous mischief in the lungs or elsewhere.

¹ In that discussion most of the cases of supposed infection by bacillus-carriers, then reported, were referred to. More recent instances are summarized by Mr. Dudgeon in his Horace Dobell lecture. The remarkable experience of Scheiller (*Centralbl. f. Bakt.*, 1908, xlv [originale], p. 395, is particularly deserving of note, as this observer incidentally found that of forty healthy persons associated, in one way or another, with an infected milk supply, seventeen, or upwards of 40 per cent., were bacillus-carriers. Neither this instance, nor the still more recent one related by Huggenburger, nor the others quoted, before or since March, 1908, throw much, if any, light on the question, Are the bacilli of bacillus-carriers capable of conveying infection? Within the last year there have appeared several papers dealing with the occurrence of typhoid bacilli in cholecystitis, and under other conditions, apart from obvious association with typhoid fever.

Furthermore, in the phthisis of dusty occupations it has been claimed that the bacillus is a secondary invader.¹

(3) The discrepancies in the purely bacteriological evidence—for example, it is well known that *emulsions* of organs are as a rule far more virulent than *cultures* containing approximately equal numbers of bacilli (see pp. 8 and 13 of the English Tuberculosis Commission's Second Interim Report, part I, where it is stated, for example, that the smallest fatal dose of culture which led to fatal progressive tuberculosis in the bovine animal was some 20,000,000,000 bacilli, "while the smallest determined effective dose of emulsion contained only 5,500 bacilli.") Again, appearances closely simulating those of tubercle are produced in animals by inoculation of cultures of "Timothy grass bacilli," "mist bacilli," "butter bacilli"—nay, by inoculation of dead bacilli; while, on the other hand, in rats, even though bacilli are abundant, they are "unable to give rise to the tissue changes which constitute tuberculous lesions."

To the above-mentioned difficulties varying degrees of importance will be attached. There are, however, certain further facts which claim closer examination. Thus there is the wide range, with well-nigh innumerable intermediate forms, exhibited on study of the behaviour of tubercle bacilli as disease-producers. On the one hand, the more or less marked differences between these intermediate forms may be observed on inoculation of the various bacilli (*typus humanus* or *typus bovinus*, for example) in guinea-pig, rabbit, bovine animal, pig, dog, cat, rat, ape, and man; and yet it has to be recognized that even so constant a character as equality of virulence, in the rabbit and the bovine animal, sometimes fails, as when the English Tuberculosis Commission² discover *one strain*, "the only case we have hitherto met with, in which a bacillus shows a marked difference in its virulence towards calves from that towards rabbits." On the other hand, even more

¹ Dr. Chas. Creighton ("Contributions to the Physiological Theory of Tuberculosis," 1908) contends that "The principle that only the bacillus of Koch produces true tubercle is a formula . . . which admits of a dozen qualifications." He cites, as instances of like structural changes induced by other agents, certain "placental analogies" and the effects of injection of blood-discs and of "tissue fibrinogens," and he says we must "drop all idea of conformity or non-conformity to an absolute structural type as if there were any such due to any bacillus." Again he says: "I maintain that the things common to the various agents are dissolution of the blood, multiple thrombi, miliary and other infarcts, red and white, and, if the process can be followed long enough, certain productions of tissue or neoplasm from the matters of reduced blood, amongst which the tubercle plasmodium in the rodents is by no means unique or of a specific character."

² Loc. cit., p. 30.

subtle differences are to be noted when one and the same type of bacillus operates in one and the same animal—(man)—producing, say, strumous cervical glands, lupus, hip disease, and so on. The question naturally arises in this connexion: Why is it that the subjects of enlarged glands, of hip disease,¹ and of lupus do not develop phthisis with far greater frequency than other persons? The generally accepted explanations are, either that different "strains" of bacilli are in question, or that the vitality of the particular tissues concerned is lowered—it may be by injury, it may be in some other way—while other tissues remain intact. In either case the conclusion seems unavoidable that the tubercle bacilli found in connexion with hip-joint disease or with lupus do not comport themselves, in the human body, in the same way as do those met with in certain other forms of human tuberculosis. Similarly, while it is agreed that, in the bovine animal and in the rabbit, for example, the bacillus of *typus humanus* reacts differently from the bacillus of *typus bovinus*, it is stated that "the pathological changes which constitute the disease called tuberculosis can be set up to a certain extent in the bovine tissues by the bacillus of Group II" (*i.e.*, by the bacillus of *typus humanus*); and, further, that "the changes brought about by the bacillus of Group II and by the bacillus of Group I are, at the outset of their respective actions, one and the same" (English Tuberculosis Commission's Report, p. 29).

Then, again, there are the close resemblances exhibited between butter bacilli, grass bacilli and innumerable other acid-fast bacilli (and possibly even non-acid-fast bacilli, for in young cultures of the tubercle bacillus itself the acid-fast property is not exhibited) and tubercle bacilli. Indeed, apart from consideration of the "habitat" from which the specimen is derived, it appears to be admitted that it is impossible to draw the line between the tubercle bacillus and allied forms. The difficulty which has been felt in the case of diphtheria and allied diphtheroid organisms, in the case of cholera and allied vibrios, and in that of *Bacillus typhosus* and its near associates, is now seen once more to be conspicuous in the case of tubercle. So far as cultural characters are concerned, there are all gradations, from a non-pathogenic acid-fast bacillus to the most active specimen of *typus bovinus*. Between the pathogenic forms, as Dr. Eastwood says, "the series is an unbroken one, the difference in growth between any two specimens, adjoining each other in the series, being almost an imperceptible one." In fact, one passes, as the

¹ See Bowlby, *Brit. Med. Journ.*, 1908, i, p. 1465.

Commissioners observe, "without any marked breach, from the least to the most luxuriant, from the most to the least dysgonic."¹

On reaching this point it is necessary to revert to the hypothesis that a bacillus may acquire pathogenicity in virtue of its becoming attached to a ferment or "enzyme." It has been suggested elsewhere that the difficulty of accounting for a large number of closely allied forms of meat-poisoning bacilli may be met, if it be assumed that one or more species of bacilli act in association, at one time and another, with a limited number of "enzymes." Some such hypothesis might be deemed applicable in the case of tuberculosis also. Indeed, the work of the English Tuberculosis Commission suggests that the change from strain to strain actually occurs. The Commissioners say (*loc. cit.*, p. 29): "Bacteriological experience furnishes us with many examples of a pathogenic organism being lowered or raised in virulence and at the same time modified in its other characters by being cultivated in the living tissues of this or that animal—that is to say, being 'passed' through the body of this or that animal." In such cases they say "*the organism remains 'one and the same,' but it is changed in virulence and often in other characters.*" They then describe three "passage experiments" in which increase of virulence was brought about, and they observe: "We may safely assume that, in the three experiments in question, certain special conditions were present, and these special conditions determined that, during the passage, the initial eugonic bacillus of low virulence was in some way or other replaced by the final dysgonic bacillus of high virulence."

As a possible explanation of this replacement the suggestion, of course, might be adduced that, in the transformation, one or more "enzymes" have disappeared or have been added, or that there has been actual replacement of "enzyme" by "enzyme." It might be urged on such a view of the matter that the tubercle bacillus was not necessarily a pathogenic organism; that the thing of chief import was the "enzyme" (or "enzymes" if there are more than one), which attaches itself to and multiplies with the bacillus and renders it virulent. On such an hypothesis the "enzyme," or the agent which introduces the "enzyme" into the body, is the cause of tuberculosis; the widely distributed saprophyte,

¹ If further evidence of this statement be required, reference may be made to a paper on "The Susceptibility of Tubercle Bacilli to Modification," by Mohler and Washburn, Twenty-third Annual Report of the Bureau of Animal Industry. These observers write: "In fact, there seems to be a point where the two types meet, and this strengthens the supposition that whatever differences there may be between them are mainly due to differences in environment during the course of their development."

the tubercle bacillus, becomes then merely a "secondary invader," and it is endowed with pathogenicity by virtue of the attachment of the "enzyme" to it.

An explanation of this sort would enable account to be given of the differing behaviour of the bacillus in one and another tissue; its assumption, for example, of lupus-producing characters in the skin, and of joint-disease-producing characters in synovial membranes, and so on. These differing behaviours would then have, as correlated phenomena, the association with one and another bacillus of one and another "enzyme," that is to say, attachment of something which goes with the bacillus; whereas, on the rival hypothesis, that it is the varying natures of the tissues which are alone responsible for differences of behaviour observed, there is a logical necessity for assuming that the bacillus must remain essentially always one and the same; and, as has been seen, this view is hard to reconcile with observed facts.

Furthermore, hypothetical, or, indeed, fanciful, as such an explanation may at first sight seem, it is perhaps deserving of consideration, if only for the reason that it would solve outright some of the chief difficulties as regards spread of infection. The observations concerning the correlation of phthisis with overcrowding in barrack rooms, common lodging-houses, and the rest, those of Dr. Ransome with regard to "tubercular infective areas"¹ and the house-incidence studies of Dr. Hermann Biggs and others, have all been held to suggest that phthisis, if not an infectious, is at least a "sub-infectious" disease. Yet it is recognized that there is an excessive incidence of phthisis in the prison, in the convent cell, and in the case of caged animals; and in these instances direct case-to-case infection can hardly play a large part. On turning, on the other hand, to sickness returns of the staffs of consumption hospitals and to the statistics of double incidence of disease upon husband and wife, the results are certainly not of a strikingly positive character. This fact is, indeed, fully borne out by Pope and Pearson's latest contribution to the subject of "marital tuberculosis." Furthermore, it has been recently stated by Dr. Mott that while phthisis is exceedingly rife in lunatic asylums among the patients, it is a comparatively rare occurrence for an attendant to be attacked.

If it were assumed that the bacillus is only secondary in appearance, and that the origin of pulmonary tuberculosis is to be sought in an infection which clings to houses and localities, but is not directly

¹ *Trans. Epidem. Soc. Lond.*, N.S., vi, p. 124.

transmitted from human being to human being,¹ some seeming difficulties would be smoothed away. At least it would be possible to explain the differences between the apparent infectiousness manifested in the dirty, gloomy, and overcrowded tenement, or in the dark and neglected cell, and the apparent absence of infectiousness in the well-regulated ward of the consumption hospital or lunatic asylum, or between man and wife. It is manifest that on a direct droplet or dust-transmission hypothesis there must be, in the last-named cases, such exceptional opportunity for spread of disease that the figures might have been expected to give the most unmistakable indications. The absence of such clear evidence is, to some minds, conclusive on this point.

Finally, if the bacillus were only secondary in appearance, the varying incidence of tuberculosis in animals might become more easy of explanation. The extreme susceptibility of some of the common laboratory animals, despite the rarity of occurrence of tuberculosis (naturally acquired) in them, might appear less mysterious. But, beyond this, in the case of the bovine animal, as also, indeed, in man, it was sought, at one time, to evade difficulties by laying stress upon the predisposition of individuals. Now, however, Sir John M'Fadyean² has stated that experiments in which cattle have been infected with measured quantities of tubercle bacilli have not detected "a single animal possessing powers of resistance much above or much below the average for animals of the same size and weight." So that in the case of bovines there "are absolutely no facts which compel one to admit that the incidence of the disease . . . is determined by varying degrees of resistance or of susceptibility on the part of different breeds or families." The "variation in the incidence of the disease" Sir John M'Fadyean attributes to "dissimilarity of environment"; but the explanation of observed variations in such incidence, in the case of animals as in that of man, might possibly be greatly facilitated if the bacillus ultimately came to be regarded as a secondary invader.

Anthrax.—In this disease the circumstances are quite unique, but they cannot here be fully discussed. It may, however, be noted in passing that the difficulties in separating pathogenic from allied non-pathogenic forms are not perhaps so conspicuous as in previous cases (diphtheria, cholera, tuberculosis). Moreover, the part played by the bacillus in producing disease is a more defined one. It may well be, of

¹ Here again the advocate of an "alternation of generations" hypothesis will find room for belief in an intermediate host, arthropod, insect or other.

² *Proc. Roy. Soc. Med.*, 1909, ii (Discussion on "Heredity and Disease"), pp. 83, 84.

course, that the anthrax bacillus stands on an entirely different footing from other bacteria. It will not, however, escape notice that concerning anthrax among the horses of Siberia, the goats of Asia Minor, or the cattle of China, practically nothing is known. The epidemiology of the disease, as exhibited in its endemic areas, has yet to be written; it has only been studied as it occurs sporadically far away from the lands of its origin; and even in this country, for some obscure reason, anthrax prevails at one time in an "external," and under other conditions in an "internal," form. It may be that biting flies play an important part in disseminating anthrax, and the rôle of the anthrax bacillus may prove, when it comes to be studied on the American prairie or the Asiatic steppe, to be actually that of a "secondary invader."

In the preceding discussion epidemiological difficulties have in the main been referred to, and it has been suggested that, to escape from them, it may perhaps be found necessary to postulate the existence of ferments capable of attaching themselves to bacteria and of endowing their hosts with pathogenic property. It is, it may however be noted, a matter of no little interest to find that, from a laboratory standpoint also, it is now declared to be necessary to review the beliefs generally entertained with regard to the relation of bacteria to disease. In a series of articles on "The Ultravisible Viruses," contributed to the *Journal of Comparative Pathology and Therapeutics*, by Sir John M'Fadyean (March, June, and September, 1908), the facts are concisely summarized. The virus of disease after disease has during the last decade been demonstrated to be capable of passing a porcelain filter. To Iwanowski, says Sir John M'Fadyean, really belongs the credit of providing (in 1892) the first experimental evidence, in connexion with his study of the mosaic or spotted disease of the tobacco plant. The fact that the juice of diseased plants retains its infective power, after it has been filtered, was rediscovered by Beijerinck some ten years ago, and this observer adduced evidence, to be later again referred to, in favour of the belief that the virus must be, not in a corpuscular, but in a "dissolved or fluid state."

In 1898 foot-and-mouth disease; in 1900 African horse sickness; in 1901 fowl plague; in 1902 yellow fever, cattle plague, sheep-pox and epithelioma contagiosum of birds; in 1903 hog-cholera and rabies; in 1905 cowpox, equine pernicious anæmia, and canine distemper; and still more recently the blue-tongue of sheep and leucocythæmia of fowls have, apparently, been added to the list of diseases caused by a virus which is

filtrable. The group, as Sir John M'Fadyean says, is a remarkably heterogenous one, and the viruses must vary considerably in size, judging by the results obtained with porcelain filters of differing degrees of efficiency.¹ Each of the viruses has hitherto resisted all attempts at cultivation on artificial media, and some of them exhibit extraordinary powers of resistance to heat and to certain chemical substances.

There is thus, as it were, a combination of forces: the bacteriologist who demands, with Jürgens, "an etiological factor embodied in the bacillus"; the student of epidemics, who asks for recognition of the fact that "something," possessing most of the attributes of a ferment, must co-operate with germs before they can be expected to exhibit pathogenicity; and the laboratory worker, who propounds a doctrine of an ultravisible virus, are presumably looking, if not for one and the same thing, for things that have very close relationship one with another. It is especially interesting here to recall the hypothesis of Beijerinck concerning the virus of the tobacco plant disease. "Reasoning from the fact," says Sir John M'Fadyean, "that the virus was able to multiply only when it was combined with the living protoplasm of the plant-host (loc. cit., pp. 64 and 65), he concluded that it must exist in a dissolved or fluid state. . . ." He argued that a corpuscular contagium "should build up visible masses of growth, or alter the appearance of the medium when cultivated, like the ordinary bacteria. . . ." Finally he showed that the virus was able to penetrate, presumably by diffusion, to a certain depth into nutrient materials, and this he held to be proof that the contagium was a fluid or water-soluble one. Sir John M'Fadyean notes that "a similar power of diffusion has been claimed for the virus of rabies."

If Beijerinck's speculations become accepted facts, "ferment" and "ultravisible virus" may, in the case of the tobacco plant disease, have

¹ It does not, of course, follow that because a virus is filtrable it is therefore invisible; the organisms of the pleuro-pneumonia of cattle ($\frac{1}{2} \mu$ to $\frac{1}{10} \mu$ in diameter) and, according to Lipschütz (*Centralt. f. Bakt.* 1908, xlviii [originale], p. 77), those of taubenpocke (epithelioma contagiosum of birds) and of molluscum contagiosum of man are instances to the contrary. Lipschütz takes exception to the terms invisible, submicroscopic and ultramicroscopic, used by Roux and Remlinger, the observers who have so largely contributed to the new knowledge concerning filtrable viruses. Halberstädter and v. Prowazek lay stress upon the fact that the filtrable organisms sometimes occur in cell-inclusions (Einschlüsse), and they group the germs of taubenpocke and molluscum contagiosum with those of vaccinia, scarlet fever, rabies, trachoma, and fowl plague under the name chlamydozoa (*chlamys* = a husk). Lipschütz objects to this term, as he says the organisms may occur independently of Einschlüsse. He therefore prefers the designation strongylosomen or strongyloplasmien (*strongylus* = round), observing that the spherical form is characteristic of organisms capable of passing through the pores of filters.

to be regarded as interchangeable terms. It does not necessarily follow, however, that this will hold good in all instances. It may well happen, for example, that the ultraviolet (or, at any rate, unknown) virus, originally concerned in production of a disease, has yoked with it a ferment, which ferment on becoming detached is apt to be taken up by the "associated organisms" of the disease. The ferment may thus be the missing link between the original virus and the secondary invaders, and on this view of the matter the difficulty in understanding how, for example, the bacteria of swine fever or swine plague stand in such close relationship with particular "symptom-complexes" may perhaps find explanation.

Alike by study of the behaviour of epidemics and by research in the laboratory the question is seen to be pressing for attention—What is the reason why a particular "associated organism," which is not, to use Sir John M'Fadyean's expression, the "actual cause and contagium" of a disease, in so many instances presents itself in connexion with the disease? The ferment hypothesis affords a means of escape from this difficulty.

In considering the question of association of organisms with ferments, attachable to and separable from them, it is of some interest to note, in passing, the suggestion that something of a like kind occurs also in the case of the digestive processes of the alimentary canal. Physiological theory takes account, in the first instance, of some half dozen ferments, though it does not altogether omit mention of micro-organisms. The former are supposed to do the work; the latter are for the most part ignored. In disease, on the other hand, the micro-organisms are carefully classed into species held to be causal each of a particular malady; these organisms are ferment-carriers in many instances, just as are those of the alimentary canal, but the ferment-carrying property, in the case of pathogenic germs, is regarded as being useful mainly for enabling the laboratory worker to differentiate between any particular species and nearly allied parasites. May it not be that, in the theory of digestive processes, the importance of the micro-organisms and, in disease, that of the ferments has been underrated? Again, just as in the case of the various digestive secretions, so in that of the processes of absorption of foodstuffs from the alimentary canal, there may be important lessons to be learnt having application to epidemiological problems. Dr. Pavy, in his recent lectures on "Diabetes,"¹ has explained

¹ *Lancet*, 1908, ii, pp. 1499, 1577, 1727.

how sugar may be transported "in a locked-up state, from the intestinal seat of food absorption and from that of glycogen storage, to where it is required for service"; and, further, how the pancreas "supplies amboceptors" which effect "the attachment of the sugar molecule to the bioplasmic molecule." Such attachment, indeed, suggests that it may be necessary to take account, in theories of disease processes, of "amboceptors" which attach to bacteria the complements endowing the bacteria with pathogenicity.

From study of the relation of organisms to fermentation processes it becomes at least clear that great care is necessary as regards the employment of ferment reactions for formulating criteria used for defining species of bacteria. The idea that any and every cultural peculiarity affords a sufficient means of distinguishing between one species and another is no longer entertained. A bacillus can, it seems to be agreed, be trained to take up certain functions which it is not at the time performing; the task can be accomplished perhaps as easily or, indeed, far more easily than that of teaching a boy, say, to swim or ride a bicycle. The boy who learns these arts does not *ipso facto* cease to be a boy, and the dysgonic bacillus which becomes eugonic is not of necessity converted into a new *species* of organism.

Dean Swift's giants were of a comparatively manageable size, but if it be sought to construct, in imagination, a Brobdingnagian creature having similar proportion to a man that a man has to a bacillus, it becomes necessary, in thought, to magnify a man some 500,000 times (linear dimensions). Such a giant, were he to examine the world with a powerful lens, might be expected to find upon it many "colonies" of inhabitants. He would, it may be assumed, only see with great difficulty objects smaller than a man; thus very many animals would be "ultravisible," and for simplicity's sake men alone may be regarded as coming within his ken. Let him be supposed now to remove, on an immense platinum loop, from the surface of the world (a surface which would, of course, appear to him comparable in extent with an area such as we recognize as being occupied by the walls, floor, and ceiling of an ordinary living room) human specimens from one or other locality, and to examine them under a huge microscope. A sample from some field of battle might yield armed men—"pathogenic organisms"; a second sample, from the street of a great city, would supply men on horses, cars, &c., moving comparatively rapidly across the microscopic field; if an ordinary labourer were by chance picked up, he would probably bear his shovel and pickaxe, or a smith

might carry his hammer. The ultra-Brobdignagian inquirer, critically examining his specimens, might regard the possession of rifle, pickaxe, shovel, or hammer as constituting a well-defined specific character. If the hammer were mislaid, or the shovel and pickaxe abandoned, the inquirer would note the presence of an "involution form"; if the rifle disappeared there would be loss of "pathogenicity," only to be restored by "passage" of the organism through a suitable animal body—*i.e.*, through an arsenal containing magazines of rifles.

To make the parallel closer it must, however, of course be assumed that the giant is able to grow his types on appropriate nutrient media, and that, as the men multiply, the machines do so also; further, it must be imagined that the attachment of the men to the machines they manipulate is not directly determined, but that it is deduced from the *effects produced by the machines*, and that these effects, studied after obtaining "pure cultures," are used to differentiate the specific types one from another. These assumptions are necessary because, in the case of the bacteria and enzymes, the ability of the two associates to multiply *pari passu* and of bacteria to lose and acquire enzymes under certain circumstances are now well-ascertained facts. Under these newly imposed conditions the ultra-Brobdignagian inquirer would be placed at a considerable disadvantage, and, indeed, be likely to experience grave difficulty in realizing precisely what he was dealing with. Let it be supposed now that he is asked to believe a number of his carefully differentiated firing, digging, and hammering forms are all men (one species) using various sorts of machines. Possibly this allegory may help to explain how it is that so much difficulty is experienced at the present time in accounting for the facts of epidemiology.

DISCUSSION.

The PRESIDENT (Dr. Newsholme) said no one could complain of want of lucidity in Dr. Hamer's setting out the subject, though it was not likely that the whole presentment could be appreciated without reading the paper in its entirety. He doubted whether any other member could have presented in such an able way so embarrassingly large a collection of facts and inferences. The paper was full of bacteriological detail, derived from bacteriology in its earliest infancy as well as in its more robust youth. The present might be described as "an evening out." Released from their administrative duties, they were at liberty to undertake an adventurous journey, conducted by an aeroplanist, who had conducted them over pinnacles and precipices, and brought them back

triumphantly to mother earth. Metaphorically, they were gasping for breath after the wondrous journey. Dr. Hamer had brought out the defects of bacteriology in the past, and his statements would act as a valuable tonic to bacteriologists. The paper was essentially speculative, and speculation was a valuable means for the advancement of science when and if it was conjoined with or followed by investigation. The routine administrative duties of those members who held public health appointments consisted, however, in a large measure, in the application of epidemiological and endemiological evidence of the present day, not of speculations as to what might or might not be revealed hereafter. Happily, to-night they were not tied down by administrative considerations, compelling them to make the best possible use of present knowledge, but were speculators and epidemiologists; and, led by such an able thinker as Dr. Hamer, he hoped those speculations would lead to further investigations, and whether those investigations confirmed or refuted Dr. Hamer's ideas, administration would thereby be based on more complete knowledge.

Dr. M. H. GORDON said he had listened to the paper with great interest, because it embodied the views of one whose distinguished position as an administrator entitled them to grave consideration, and while he could not for a moment accept those views, he, nevertheless, could appreciate the courage and skill with which Dr. Hamer had advanced them. Dr. Hamer's main thesis was this. As an epidemiologist—that is to say, as one accustomed to take his stand upon the widest possible base, as one accustomed to view epidemic disease, not so much in its relation to individuals as in its relation to groups and communities—he was unable to accept our present knowledge of the relation of bacteria to disease as sufficient to account for the facts of epidemiology. He took it that Dr. Hamer admitted the established data of bacteriology—for instance, that in epidemics of plague, cholera, typhoid, diphtheria, anthrax, &c., the specialized micro-organisms so well known in relation to these diseases were constantly found present. He also admitted (presumably) the pathogenicity of these bacteria for laboratory animals. His chief difficulty arose when he considered these infecting organisms from the epidemic standpoint; and he had special difficulties with regard to the inception of the epidemic diseases in point amongst others. Now it is absolutely necessary to bear in mind that, from our present knowledge of the subject, an epidemic disease, such as one or another of those he had mentioned, is not so much an *entity* as a *resultant*; it is the result, in fact, of an *interaction* between two opposite forces—the infecting agent and its host—and, therefore, before they could hope to get to the bottom of this matter they must first consider each of these factors separately. Both of them in the diseases in point, being living organisms, are liable to be influenced by their environment, and were subject, therefore, to some variation, the precise degree of which has by no means yet been adequately determined. Now, he considered it as a very grave omission on the part of Dr. Hamer that he had neglected to take due account of what, from the present imperfect state of our knowledge, would seem to be one of the greatest factors

in the inception of a case of an infectious disease of the types mentioned. The factor to which he referred was not the germ, though that, of course, was indispensable. It was not, he submitted, the vague hypothetical enzyme that, in Dr. Hamer's view, debauched the microbe, so to speak, and endowed it with homicidal capacity—but it was *susceptibility*. In other words, the variation of chief import with regard to the spread of an infectious disease, especially of those widely distributed in our midst, is not variation of the microbe so much as variation of its host. He would ask this Society to recall for a moment the enormous importance in regard to epidemic disease of one or another kind of age-susceptibility, of malnutrition, and of fatigue. Of much importance, also, is the physical condition of the environment, especially in regard to temperature and humidity, factors that may be more related to the phenomenon of the seasonal incidence of disease than is yet realized. The influence of polluted atmosphere in relation to disease is also a matter of high importance. The subject of the purity of air is practically, at the present time, ignored administratively, yet it is probably of greater importance, from the point of view of the public health, than purity of food or of water. He hoped he had said enough to remind them of the complexity of the subject, of the wide issues involved, and of the danger of endeavouring to make bacteriology explain more than can reasonably be demanded of it. Coming now to Dr. Hamer's bacteriological difficulties: His first was with regard to the variation shown in the matter of agglutination by bacteria, associated with outbreaks of meat-poisoning. He (Dr. Gordon) would point out that the agglutinins were only one of the factors whereby the blood resists infection, and that their exact differential value had still to be determined. The class of micro-organisms to which he referred was particularly susceptible to agglutination, and while all members of it respond to "group agglutinins," it is only in the higher dilutions that the difference obtains between them. The exact significance of this minor quantitative difference has still to be decided in case of the group of bacteria in point, and we must wait for further evidence. Personally, he saw no difficulty in accepting them as responsible for the outbreaks in question, even if they did differ as regards agglutination, and for that reason were regarded as different bacteria. His second difficulty related to the cocci found associated with epidemic cerebro-spinal meningitis. Now, on this subject, Dr. Gordon could speak from experience, as he had investigated these cocci at some length. He could assure them that there was no difficulty. The micro-organism found again and again in outbreaks of this disease is identical, and is always Gram negative. Those who have found Gram-positive or Gram-variable cocci have either been dealing with contaminations, which are particularly difficult to exclude in this case, or with meningitis cases of other than the epidemic type, or have failed to make control experiments to ensure correct staining. The pneumococcus, streptococcus, and other bacteria, of course, may produce meningitis. He must also deny that the sugar tests had proved disappointing for the purpose of differentiating these and other cocci. They had, in his own experience, proved of the very greatest value for this purpose, and others, also,

had found them so. No doubt some confusion had arisen from impure sugars having been used occasionally. In the matter of influenza—Dr. Hamer's third difficulty—was not it time that we recognized that influenza is only a term? At least four different infections are included under this name, and the careful work of Dr. Allen, recently published, has done much to improve our knowledge of the bacteriology and treatment of catarrhal conditions of the upper respiratory passages, called by some colds, and by others influenza. The four bacteria that have been found most frequently are Pfeiffer's bacillus, *Micrococcus catarrhalis*, *Bacillus coryzæ segmentosus*, and Friedländer's bacillus. His fourth difficulty—namely, that causal organisms are widely distributed without production of disease—was, in Dr. Gordon's opinion, overstated. Dr. Hamer had lost sight of the rapid death of most pathogenic bacteria outside the body under the influence of sunlight, desiccation, and other conditions unfavourable to them. As regards tubercle, he must say that he had searched for this organism in over twenty samples of street dust from various parts of London by injecting that material into guinea-pigs subcutaneously and intraperitoneally. The result was uniformly negative. He considered, therefore, that Dr. Hamer had overstated this point. He must refer to another of Dr. Hamer's difficulties—viz., that whereas in man the pneumococcus produces pneumonia, this organism in the laboratory animal produces septicæmia. Recent observations of early cases of the disease in man have shown that before there are any physical signs of pneumonia in the lung, the pneumococcus can be recovered from the blood of the patient. This difficulty, therefore, does not exist in fact. With regard to Koch's postulates, they represented a useful standard to aim at, and when introduced were the result of conceptions of what was desirable rather than the product of extended experience of what was actually practicable. These conditions have proved too rigid in the case of several diseases, and for the reason that these diseases are not communicable to laboratory animals in the same sense that they obtain in man. For instance, one cannot hope to produce the rash and characteristic desquamation of scarlet fever in laboratory animals, because they do not take this disease.

Dr. LEDINGHAM said that if the paper had the effect of promoting a closer co-operation between the epidemiologist and the bacteriologist than had hitherto existed in this country it would have done a great service. But after reading the paper he thought the author had greatly overstated his case; had, in fact, drawn conclusions from insufficient premisses. His reference to bacteriological papers in a non-critical way was probably due to his lack of working knowledge of bacteriological methods. It would be admitted that the *Bacillus typhosus* was unable to produce the clinical picture of typhoid fever on animals in the laboratory, but there was a large mass of evidence connecting that bacillus with the disease; in 90 per cent. of the cases it was obtained from the blood, and in 70 per cent. from the fæces. Evidence was also derived from the immunity reactions in the blood-serum. Dr. Hamer said the bacillus ought to be present before the first week of the disease. That might be so, and if Dr. Hamer would send him a case in the incubation period he would gladly investigate it. The

author referred to a paper by Tarchetti on the probability that the *Bacillus coli* might change in the intestine into the *Bacillus typhosus*. Tarchetti had no experimental evidence to support that, and the Dark Ages were long past when they could not distinguish between the *Bacillus typhosus* and the *Bacillus coli*. Mutation might possibly occur, but it was probably only temporary, and there was a strong tendency for the micro-organism to revert to its original properties. The *Bacillus typhosus* was remarkably constant in its fermentative reactions. He believed Dr. Hamer had characterized the idea of typhoid-carriers as being a fashionable theory. He (Dr. Ledingham) thought it would be as futile to over-estimate the importance of that question as to under-estimate it, as Dr. Hamer did. It was necessary to wait for some years until a large mass of facts had been accumulated. In Germany, America, and in this country, numerous instances demonstrating the importance of the carrier in typhoid epidemics had been recorded, and the mass of evidence was constantly being added to. He thought it would be found that even the large epidemics of typhoid—water-borne and milk-borne—might be due primarily to the infection of those supplies by typhoid-carriers. With regard to the presence of bacilli in the blood of cases which were suffering from diseases other than typhoid fever, Dr. Hamer referred to Busse's paper, where he stated he found the *Bacillus typhosus* in the blood of certain tuberculous patients. There was no history in those cases of a previous attack of typhoid fever, and the Widal reaction was negative. When a bacteriologist met with such a disconcerting fact he was not down-hearted; he set to work to investigate further. Possibly those cases might have been mixed infections, or possibly they might have been typhoid-carriers, even in the absence of a definite typhoid history. He feared he did not understand the enzyme theory, but he thought it was premature to postulate such a theory at present; they must wait for facts in greater quantity. As the theory of immunity advanced, many of the difficulties raised by Dr. Hamer might find solution.

Dr. E. W. GOODALL said that after reading the paper he did not understand Dr. Hamer's position. Was he a believer in the germ-theory at all? Or was this an insidious attack upon it? It would have been better if he had boldly stated what his position was. The first difficulty the author had was to reconcile the persistence of the disease types with the variety of micro-organisms found in association with the different types, and he instanced cerebro-spinal meningitis, pneumonia, influenza, and so on. Dr. Gordon had touched on the point, but had not sufficiently emphasized it. There was really no difficulty in it. More than one germ caused the same lesion, such as inflammation, and the symptoms would depend on the part affected. Cerebro-spinal meningitis might arise from the meningococcus in one case, and from the pneumococcus in another, and it was the accident of locality in the body which caused particular symptoms. If he were to write a book on medicine and treat of diseases due to micro-organisms, he would not describe the diseases pneumonia and cerebro-spinal meningitis, but the diseases due to the meningococcus and to the pneumococcus, which would at one time give rise to meningitis, at another to

inflammation of the lungs, and at another to peritonitis. There were diseases known to be due to certain inorganic poisons, such as lead, which might cause different lesions—such as neuritis, or arterial degenerations, or renal changes—yet there was no difficulty in accepting the view that lead was the cause in each, even though the same lesions might be produced by other poisons. He could not help feeling that in that paper Dr. Hamer had set up difficulties that he might have the pleasure of knocking them down again. He did not understand the author's enzyme theory. Where did the enzyme come from? He hoped Dr. Hamer would explain. Was it in the micro-organism, or in the body or tissue attached, or diffused through Nature? If it was something which was picked up by the micro-organism and conveyed to the body to be attacked, the whole question of epidemic diseases would be much more difficult to understand.

Dr. BAINBRIDGE said he understood the theory which Dr. Hamer had put forward was due to the difficulty he found in accepting the causal relation of organisms to disease, and therefore any criticism of his paper must be directed largely to those difficulties. One difficulty concerned the paratyphoid bacillus, and Dr. Hamer quoted a paper in which it was stated that healthy persons might have that bacillus in their stools, and that it had been isolated from food which had been eaten without causing ill effects. He thought Dr. Hamer had been misled by the German writers, whose position was a curious one, for they stated in the same paper that the *Bacillus paratyphoid* and the *Bacillus Aertrycke* were indistinguishable from each other. But they went on to give cases of food-poisoning in which they found what they called paratyphoid (B) bacillus. If the two were indistinguishable, it was difficult to see why different names should be given. He had found that the two bacilli could readily be distinguished by the absorption method, but that method was very little used in Germany. He did not know of any definite instance in which the paratyphoid (B) bacillus had been associated with acute symptoms such as those of food-poisoning, and there was no reliable evidence in the writings of German authors that such was the case. Until the organisms concerned had been differentiated by the absorption method it was premature to assert that the paratyphoid (B) bacillus could occur in food and be eaten without causing ill effects. At present there was no evidence to controvert the clinical entity of paratyphoid fever; and the clinical and bacteriological evidence on the subject went hand in hand. Dr. Hamer's difficulty in that respect, therefore, was not a real one.

Dr. BUTLER welcomed the paper as an offset to a tendency to get too crystallized in ideas, and thus fail to keep abreast of recent knowledge. A disease was a symptom-complex, and that appeared to have been Dr. Hamer's idea. The history of medicine showed that often what had been regarded as one disease was shown to be many, in accordance with its sequel of symptoms. The advent of bacteriology had caused a considerable shaking up of ideas. It was said that influenza was not a disease, because it was not the product of a specific infection, but a complex of a variety of infections. Must diseases be classified as infective by their contextual symptoms, or according to the

specific infection? Or were they to go further and, as Dr. Hamer suggested, classify them according to the products of infection? The speculation was interesting, and the results might be useful. Both the human body and the organism were subject to much variation, and that fact accounted for many of the difficulties. Dr. Hamer seemed to be in pursuit of something which was more constant than the organisms, and in that he thought members would be prepared to follow him.

Mr. KENNETH GOADBY said Dr. Hamer had already quoted Hueppe, and he seemed to adopt that writer's point of view and to refer to specific bacteriology in the same terms as the schoolman of the Middle Ages referred to disease generally; but Dr. Hamer adopted the specific view in regard to enzymes, so that if the god of the specific bacteriologist were the god of evil, this was also true of the specific enzymologist. He (the speaker) failed to understand the application of a hypothetical enzyme, particularly when one considered that toxins, endo- and exo-cellular, were associated with bacterial action. Recent work on enzymes led to the idea that an enzyme was a body which owed its action not so much to the setting up of any specific action as being a catalyser or "lubricant" of such action when it had been already set up. And the enzyme itself combined with the substratum in which it was found, it obeyed the laws of mass action, its action gradually diminished according to fairly well-known mathematical laws, it was directly influenced by the quantity of end-product in the substratum, and in many instances underwent changes, gradually becoming inert. If one presupposed that an enzyme must get into the body or be attached to the bacillus, or be obtained from the body in conjunction with the bacillus, there must be a large supply of enzyme present in all the reactions which produced the disease, and there was no experimental evidence forthcoming to warrant such a supposition. Various toxins had been obtained from bacteria, or extracted from the bacterial body in various ways, but these toxins did not seem to be able to go on producing themselves, or to go on producing such profound ultimate change in the substratum to the same extent that we should expect if it were an enzyme capable of acting in conjunction with the bacillus. He had been glad to hear the remark about fermentation in sugar. The point was familiar to him that the given fermentations of any organism depended largely on the quality of the material used for the test, and MacConkey had shown that the reactions of the lactose fermenters were especially constant when pure sugars and proper length of time were made use of in the observations. It was notorious that chemists said it was impossible to procure certain sugars pure, and yet some of those sugars were still used and the results published as a method of differentiating bacteria. He thought Dr. Hamer had been inclined to glean in odd bacteriological corners, and to use material which happened to fit in with his scheme. He should rather have taken those facts upon which bacteriologists agreed, or at any rate referred to their existence. He carefully avoided the problems on which bacteriologists held a strong position, and he glossed over many important experiments in relation to epidemic disease, as, for instance,

the experiments which had been done in connexion with plague infection, especially those where monkeys protected from the bacillus-carrying fleas in native dwellings survived although the animals not so protected developed plague; the fleas caught on the "tangle foot" around the surviving animals being shown to be full of virulent plague bacilli.

Dr. BULSTRODE thought that Dr. Hamer's paper must be regarded as a very valuable contribution relative to the present phase of bacteriology, and it was clear that there was need for reconsideration of the standards set up by Koch and extended by other bacteriologists. It was obviously impossible in the case of certain bacteria to comply with the standards in question, and, as Dr. Hamer had shown, the discovery of "carrier" cases had still further modified the original position. The author had also shown that much recent work was pointing to the conclusion that current views as to the stability of micro-organisms might have to be very materially modified, and it was not altogether unthinkable that pathogenicity might be merely a phase in the life-history of certain micro-organisms. In other words, it seemed probable that in the near future some theory other than that which was now accepted might be necessary to explain all the facts. Within recent years certain diseases thought to have been due to a vegetable micro-organism had now been definitely associated with the protozoa, and there was at the present time a growing suspicion that the history of enteric fever was not in complete accord with the view that the disease was caused solely by the organism with which its presence was commonly associated—a conclusion which would appear to receive some support from the different standards which have from time to time been put forward as necessary for establishing the presence of *Bacillus coli*. Obviously most of these changes were the inevitable result of the increase of knowledge, as one of the most important points brought out by Dr. Hamer's paper was as to how far this increase of knowledge is connected with a maintenance of the original bacteriological position. Probably Dr. Hamer's theory in substitution of current beliefs might not receive acceptance very readily; indeed, Dr. Bulstrode suspected that it might have been put forward rather with the object of raising discussion than a desire for its acceptance. If this had been the case Dr. Hamer must have been much gratified, seeing that his paper had produced a debate which had brought into prominence several very important issues, as it would seem that a large number of Dr. Hamer's criticisms had been just accepted by the bacteriologists themselves, some of whom were apparently asking whether in certain particulars a modified attitude would not have to be adopted.

Lieut.-Col. DAVIES desired to support Dr. Hamer's position. He believed in the specificity of bacteria, but he did not believe that specificity to be unchangeable, otherwise one could not be a believer in evolution. He believed pathogenic power could be involved as well as evolved. He had not heard any speaker refer to the progressively infective nature of certain epidemics. If there was progressive infectivity, as in diphtheria, there might equally be a retrogression or progressive involution, as apparently occurred in cholera epidemics. Therefore there must be some change, not in the intimate nature,

but in the physiological properties of the organism. It was the same organism, but it lost some of its properties or added others to them. Much harm had been done, he thought, by applying to organisms of a lowly type, such as bacteria, the definite Linnean nomenclature applicable to higher forms of life; everyone would allow that there were varieties of coli and of typhosus bacilli. There should not be too much dogmatism with regard to the absolute properties of those organisms. And individual susceptibility had also much to do with the effect produced. Dr. Savage had pointed out that with reference to typhoid-carriers dosage had much to do with the pathogenic power of the bacilli. They had to make their way through the stomach, and many would not survive the passage, as a healthily acting mucous membrane with copious acid secretion would destroy them, probably in the same way that cholera *spirilla* were destroyed. Epidemiologically, therefore, the healthy condition of the stomachs of the population attacked might be of as great importance in determining the severity of an outbreak as the pathogenic power of the particular organism attacking. There could, indeed, be little doubt but that both these factors were variable. The pathogenic power of the specific organism might vary in the same way that indol formation, or some particular kind of sugar fermentation, varied, sometimes increased, sometimes delayed, sometimes inconveniently absent. Morphological characters to determine "species" of bacteria are absent; physiological, or cultivation, characters apparently are not constant; therefore specificity is not absolutely immutable in bacteria any more than in the higher forms of life.

Dr. MEREDITH RICHARDS desired to offer a few words as an administrator. Whilst listening to Dr. Hamer he had been reminded of the dictum of Occam that one should never assume causes which were more numerous or more onerous than were necessary to account for the phenomena. He failed to understand what Dr. Hamer's ferment really was. If it was like trypsin or pepsin then that enzyme or ferment had to be present in quantities proportional to the effect produced. If so, where did that ferment come from? If it was the product of a living cell what was the cell? Was it of the animal suffering from the disease, or was it the bacillus? With regard to fermentation of sugar by yeast it was possible to say it was caused by the enzyme excreted by the yeast plant, but he did not think that was a better explanation from the brewer's point of view. A simpler and equally scientific one was to say that the fermentation of the sugar was due to the activity of the yeast plant. With regard to the interchangeability of scarlet fever and diphtheria, something was known about diphtheria occurring in scarlet fever convalescents and vice versa, but he did not think that was the same as saying the two were interchangeable, and the more one worked at the subject the more distinct the two diseases seemed to be. Again, it had been suggested that the Klebs-Löffler bacillus should be regarded as secondary. That was unimportant; whether primary or secondary was immaterial if it were the fact that bacillus was always present and that epidemics did not occur in its absence. With regard to Dr. Hamer's giant simile, if that giant found that he always got diarrhoea, increase of temperature

and an enlarged spleen whenever he ate one of the human beings who carried rifles, he would rightly assume that the rifle-carrying human being was the cause of his typhoid, and it would not matter whether that rifle-carrying bacillus was or was not related to other human beings who had not rifles, the point being that the rifle-carrying human beings were the cause of those three symptoms and to be carefully avoided by those who wished to remain free from disease.

Dr. ARKWRIGHT desired to refer to one point in connexion with carriers which had not been mentioned. This was particularly important in the case of cerebro-spinal meningitis. The meningococcus was so very sensitive to external conditions, such as dryness and cold, that it had been difficult to understand how it could pass from one patient to another, especially as the patients themselves were not, apparently, very infectious, and it was comparatively rare for several cases to occur in the same house. Recent published work had shown that carriers of the meningococcus were found quite frequently during epidemics. In this way the carrier had come as a help to the bacteriologist instead of to confound him, as Dr. Hamer thought. The greatest number of carriers had been found in the early stages and at the height of the epidemic in Silesia, and at the end of the epidemic the number of carriers was only one-fifth or one-sixth of that found at an early period.

Dr. HAMER, in reply, said he was glad to avail himself of the President's suggestion. Dr. Gordon had made a remark about "debauching the microbe." He understood Dr. Gordon to suggest in this connexion that he (Dr. Hamer) had not devoted sufficient attention, among other things, to the question of the susceptibility of the individual attacked. The difficulty he had in mind was this: Consider the cases of, say, a number of children in Southwark and a number of children in Lewisham, of similar social class and living under similar conditions. He believed it was admitted that examination of both groups of children would yield approximately the same percentage of positive results *qua* Klebs-Löffler bacilli. Yet in Southwark there might shortly afterwards be an epidemic of diphtheria spreading in a school, while in Lewisham there was nothing of the kind. Again, in Southwark, on distribution of an infected milk supply, there followed an outbreak of diphtheria, while the Lewisham children, in the absence of such supply, remained exempt. He (Dr. Hamer) considered that these phenomena might be explained by assuming that something had "debauched" the Southwark microbes under those two sets of circumstances. An alternative view would, of course, be that in Southwark, on each occasion prior to the outbreak, special "debauched" bacilli, unlike those previously encountered in the children's throats, were newly introduced and caused the mischief. Dr. Gordon said influenza was a mere term, and that "the studies of various observers had shown that different organisms were concerned in various epidemics." According to the general view, if Pfeiffer's bacillus were present, it was influenza; if not, it was not influenza. The point which troubled him (Dr. Hamer) was whether distinction could be drawn, epidemiologically, between successive prevalences of the "various epidemics," caused,

say, by Pfeiffer's bacillus, *Micrococcus catarrhalis*, *Bacillus coryzae segmentosus*, or the bacillus of Friedländer. If he could be told where to draw the line, after 1890, or after 1891-2-3-4, or 1895, or when, he would be better able to appreciate the position with regard to these supposed causal organisms. From an epidemiological standpoint regard must be paid to the dictum of Hirsch that "few among the acute infective diseases have manifested in their prevalence, at all times and in all places, the stamp of uniformity so strongly in the aggregate of symptoms as influenza." Epidemiologically considered, it was monstrous to suppose such uniformity could be the outcome of mere chance manifestations of entirely disconnected activities, by first one and then another of a series of unrelated and quite "different organisms." Dr. Gordon said the exact value of the agglutination test was not known, and that up to a certain point one might expect "all organisms would be agglutinated by all sera." It would be interesting to learn how much importance Dr. Gordon attached to the test, upon which Dr. Bainbridge relied, for differentiating between *Bacillus paratyphus* (B) and *Bacillus Aertrycke*. As Dr. Gordon held that "it was known that the idea of specificity was not altogether borne out with regard to the agglutinins to the degree ascribed to it," it might, perhaps, be presumed that he would side with Uhlenhuth and Hübener rather than with Dr. Bainbridge in this matter. They were left in doubt as to how far Dr. Gordon was disposed to rely upon evidence as to pathogenicity, for he had admitted that we "must not expect too much from laboratory animals." In dealing with fermentation tests, however, Dr. Gordon seemed to consider he was on solid ground, provided always the purity of the sugars used could be relied upon. This, alas! it appeared, was, as things stood, an unattainable ideal. The observations on street dust were interesting, but it was always understood, the world being wide and the existence of natural disinfecting agencies admitted, that it was possible to obtain material, even to the amount of 500 gm., from which the tubercle bacillus could not be isolated. Dr. Gordon had remarked that the "relative proportion of bacillus-carriers to those free from the organism had not yet been adequately determined, and pending this too much stress should not be laid on carriers." This appeared to him (Dr. Hamer) a weighty observation, and there was, moreover, a practical point with regard to this relative proportion. In Germany, as they were aware, recourse was made to severe measures ("zu drakonischen Massregeln"), and experience showed that even a carrier will turn. It was naively recorded,¹ apropos of a proposal to isolate a carrier for a period of a year, "Man würde ihn unglücklich machen." It had been suggested, indeed, that the police must be called in to deal with recalcitrant carriers. Account was given (loc. cit., p. 553) of three cases in which the heroic measure of extirpating the carrier's gall-bladder had been practised, but it seemed that in the two instances in which the patients operated upon were followed up, it had been ascertained that the bacillus-carrying propensity still persisted. As long as the carriers were in a

¹ *Klin. Jahrb.*, xix, iv.

minority, they could be made to submit, but should it chance that the carriers outnumbered the sound, the consequences might be awful to contemplate. The question would arise, "Quis custodiet custodes ipsos?" It was a matter of no little interest, therefore, to find (loc. cit., p. 521) that in cerebro-spinal meningitis the carriers did far outnumber the actual cases exhibiting symptoms. The sick person, they were told, was a mere incidental product—"ein Zufalls-product, das Opfer einer ihm eigenthümlichen Disposition"; he was of comparatively little importance so far as spreading the disease was concerned (p. 525). Bochall found forty-two carriers for every recognized case of illness. Kirchner, taking the more modest estimate of twenty carriers to a case, had computed that it would have cost the State some 8,640,000 marks had the attempt been made to deal effectively with carriers in Prussia in the year 1908, and, as a matter of fact, the outbreak would presumably have subsided, whether or no. The observation that the disease was spread entirely by healthy carriers, the sick people being practically harmless ("die Verbreitung der Krankheit allein durch Bacillenträger stattfindet, während die Kranken so gut wie ungefährlich seien" (p. 474), might at first sight appear disconcerting to the believer in the causal rôle of the meningococcus; but it had been explained (loc. cit., p. 521) that the occurrence of cases in an epidemic, apparently entirely unconnected with one another, made it clear that healthy carriers must be very numerous, and that it was therefore gratifying to find that bacteriological investigations fully bore out this theoretical prediction. It was admitted, however, that with bacillus-carriers everywhere it was mere folly (loc. cit., p. 527) to think of isolating particular carriers brought under notice in the course of bacteriological investigation. All that could be done was to give carriers printed instructions, setting out, in large type, the precautionary measures to be adopted; to hope this advice would be supplemented from the proper quarters by word of mouth admonitions; and, further, that the daily Press would take the matter up (loc. cit., p. 552). Park, in America, seemed practically to have reached a similar conclusion so far as typhoid-carriers were concerned. America, moreover, was a free country, and there, it seemed, carriers declined to have their gall-bladders extirpated.

Dr. Ledingham admitted that the *Bacillus typhosus* was unable to produce the clinical picture of typhoid fever in laboratory animals, but he said there was a large mass of evidence connecting the bacillus with the disease. Of course, he (Dr. Hamer) did not deny this connexion; the point to be determined was whether the bacillus was the causal organism of typhoid fever, or was merely associated with the disease as a secondary invader. In the case of small-pox, no one was likely to have the hardihood to deny association of pyogenic organisms with the pustules, but it was none the less generally agreed that these organisms were not the cause of small-pox; indeed, it was recognized that they did not appear upon the scene until the first week of the disease. Dr. Ledingham agreed (in the case of typhoid fever) that the bacillus was not demonstrable until the disease was similarly fully established, but he undertook to search for the *Bacillus typhosus* in the early stages of typhoid fever provided

Dr. Hamer would supply him with a case in the incubation period. He thus evinced a desire to throw upon Dr. Hamer the burden of supplying proof that the *Bacillus typhosus* was there from the beginning. This *onus probandi* must, however, rest with the bacteriologist. Pending further inquiry with regard to the matter, two facts claimed their attention. First, that although blood examinations had now been made in a considerable number of instances in Germany, America, England, and elsewhere, in only one instance had it been reported that a person not then recognized as having typhoid fever, but whose blood yielded a positive result, subsequently developed the disease.¹ Secondly, while instances had been reported in which the *Bacillus typhosus* had been casually encountered—for example, in water (as by Beck and Ohlmüller in 1904)—it had not been found under those circumstances to be causing inconvenience to the consumers of the water; and, furthermore, bacilli had been again and again found in the blood and excretions of patients who were not suffering from typhoid fever. In these last-named instances the presence of the bacillus was held to imply previous attack (say, at any time up to fifty years ago) by the disease. In fact, it would seem, if the usual method of dealing with these matters were recognized as being satisfactory, that the position, so far as the typhoid bacillus was concerned, was unassailable. If, for example, material from a case of possible typhoid fever in the incubation stage were submitted for examination, and the bacillus were found and the case turned out to be one of typhoid fever, the bacillus won; if it were not found, and the case proved to be one of typhoid fever, the bacillus won as before, for experience went to show that the bacillus was not as a rule found until the disease was established; if the bacillus were not found and the case turned out not to be one of typhoid fever, the bacillus was once more justified; while if finally the bacillus were found and the case turned out not to be one of typhoid fever, then, of course, the patient was a healthy carrier, and thus the bacillus was once again triumphant. In no event, according to the rules under which the game had thus to be played, was it possible for the bacillus to be discomfited. Dr. Ledingham admitted that mutations might occur, but said there was a "tendency to revert to the original properties." He would not, however, admit difficulty in distinguishing between *Bacillus typhosus* and allied forms. Baumann's paper, which had been referred to in this connexion, rather confirmed Dr. Ledingham in this opinion. And yet it was now agreed that in addition to Schotmüller's bacilli there were other paratyphoid bacilli (Zupnik two years ago referred to five, and others had been since described). He (Dr. Hamer) was indebted to Dr. Theodore Thomson for a reference to a paper² giving account of a bacillus intermediate between *Bacillus paratyphosus* A of Brion and Kayser and the *Bacillus typhosus*. And any one of these bacilli might occur in typhoid fever, together with the typhoid bacillus, constituting a "mixed infection," and such admixture of bacilli might

¹ Conradi: *Deutsch. med. Wochenschr.*, 1907, xxxiii, p. 1684.

² *Comptes rendus Soc. de Biol., Par.*, 1908, lxiv, p. 1093.

be observed not merely in individual cases, but might be conspicuously exhibited in particular outbreaks of typhoid fever. None of the speakers had commented upon the view held by Dudgeon, Statham and Peabody, and referred to by him (Dr. Hamer). If typhoid fever might be caused by "quite a number of allied but distinct species of bacteria," and if, as was agreed, mutations might occur, why should there be unwillingness to admit that difficulties with regard to identification might arise? Suppose, for example, a bacteriologist were asked to examine material from a case of typhoid fever, due to an organism which was not the *Bacillus typhosus*, though closely allied thereto, and one, moreover, which chanced to have undergone mutation and was thus temporarily exhibiting the characters of *Bacillus typhosus* itself. In this case, at any rate, until such time as the organism "reverted to its original properties," there might be room for misapprehension. In the same sort of way, when Dr. Bainbridge used the method he had referred to, for distinguishing between *Bacillus paratyphus* (B) and the Aertrycke bacillus, there was the possibility that some insignificant mutation might spoil results. In the one case, as in the other, it would, of course, be claimed that the points at issue could only be appreciated by bacteriologists. But having regard to the differing views held by specialists, there was no doubt that some there to-night, who were not bacteriologists, would like to be present in spirit when those gentlemen tried to settle these problems among themselves.

Mr. Goadby hinted that his (Dr. Hamer's) notions concerning enzymes were mediæval. He could only reply that he had assumed that here, at any rate, he was up to date, having regard to the recently expressed views of the Professor of Biochemistry at Liverpool. Mr. Goadby, unlike Dr. Gordon, attached little, if any, importance to the fermentation test—indeed, he had remarked that it was "notorious that chemists said it was impossible to procure certain sugars pure, and yet those sugars were used and the results published." It seemed hard that after allowing himself this licence Mr. Goadby should proceed to observe that he (Dr. Hamer) ought to have limited his criticisms to points upon which bacteriologists were agreed. As a matter of fact he was on the look out for such points, but really after what had fallen from the various speakers he was in despair.

Dr. Arkwright noted with satisfaction the fact that carriers were frequently met with during outbreaks of cerebro-spinal meningitis, and claimed that after all the carrier came to console and not to confound the bacteriologist. In Silesia, he said, the number of carriers was highest at the height of the epidemic. It might, of course, be argued that this would be the case even on the assumption that the meningococcus was a secondary invader. Dr. Arkwright added that at the end of the epidemic the number of carriers declined. Various explanations of this fact might be adduced. But, on the other hand, Kirchner¹ found that when a cholera epidemic was declining the primary bacillus-carriers increased in numbers; and Conradi thought that, in typhoid fever epidemics,

¹ *Klin. Jahrb.*, xvi.

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the appearance of carriers indicated loss of virulence of the organism. On this hypothesis it might, perhaps, be argued that the larger the number of carriers the better.

Dr. Goodall and Dr. Meredith Richards both complained that he made the facts of disease difficult to understand. But whatever mistakes he had committed he had at least a clean slate in that regard. Those gentlemen should address their complaint to Nature. He (Dr. Hamer) was glad to have provoked this discussion. As one of the Secretaries of the Section he had from time to time endeavoured to induce some of those who had spoken to contribute papers, and, though not successful hitherto in this, he had at any rate been instrumental in bringing them this evening to participate in this discussion, and for the profit derived from their observations he cordially thanked them.

Epidemiological Section.

February 26, 1909.

Dr. A. NEWSHOLME, President of the Section, in the Chair.

The Bacteriology of Summer Diarrhoea.

By H. DE R. MORGAN and J. C. G. LEDINGHAM, M.B.

WE propose in this communication to give a brief review of our knowledge of the bacteriology of summer diarrhoea, dealing more particularly with the results obtained during the last four years by Morgan and other workers in the Lister Institute. Details of bacteriological technique will in great part be dispensed with, as our main object is to elicit discussion on the possibility of correlating current epidemiological opinion on this question with bacteriological findings.

The difficulties experienced by clinicians in accurately differentiating the true summer diarrhoea of children from others occurring during the epidemic season are felt also by the bacteriologist who naturally desires to investigate a series of selected cases of the disease concerning which clinical opinion is unanimous. Unfortunately, this desideratum can never be completely realized. The cases vary greatly in severity in the same season and in different seasons, and accordingly the bacteriologist has to control the results obtained in a selected group of clinically typical cases with those obtained in extensive control series of less severe diarrhoeas, diarrhoeas occurring as complications of other diseases, and, finally, in a series of apparently healthy children.

The first attempt on a large scale to investigate the bacteriology of summer diarrhoea was made as long ago as 1887 by Booker in America. Up to that time the flora of the intestinal tract had received but scanty attention. Only two years before, in 1885, the *Bacillus coli communis* and the *Bacillus lactis aerogenes* had been described by Escherich and

for some time the importance of these micro-organisms was considered to be a dominant one.

The work done by Booker was very complete so far as bacteriological methods permitted, but with our present knowledge little or no etiological significance can be attached to the micro-organisms isolated by him in summer diarrhœa. Booker himself felt that no one micro-organism of the many varieties recovered from his cases could be considered as the specific agent. In an attempt to correlate his bacteriological findings with clinical phenomena he adopted the following grouping:—

Group I, 35 cases: The patients in this group are emaciated and exhibit toxic phenomena. The stools are at times frequent and at other times infrequent. Among other bacilli isolated (including the *Bacillus coli communis*) the *Bacillus proteus vulgaris* appears to occupy an outstanding position.

Group II, 27 cases: The cases are serious and show general toxic disturbance. The stools are fluid or soft, often green and slimy, and vary in frequency from three to twenty in twenty-four hours. Micrococci (streptococci) preponderate in these stools, in conjunction with pus-cells.

Group III, 6 cases: The cases are serious, the stools vary in frequency and often have a putrid odour. The *Bacillus coli communis* is numerous in all these cases along with the *Bacillus lactis aerogenes*. A large number of inconstant varieties are also found.

Group IV, 24 cases: The cases are mild and of short duration. The *Bacillus coli* predominates in all cases and occurs sometimes in pure culture.

Some general observations made by Booker with regard to the distribution of coliform organisms in the intestinal tract and in the fæces are of interest. He noted that while the *Bacillus coli communis* and the *Bacillus lactis aerogenes* do not altogether disappear from diarrhœal fæces, they occur in much fewer numbers than in the fæces of healthy milk-fed children, and, in fact, tend to diminish in proportion to the severity of the case, their place being taken by varieties which occur inconstantly in the healthy intestine. This substitution of the normal flora by inconstant varieties in diarrhœal fæces will be referred to later.

In considering the work of American authors on the diarrhœas of children one must pay close attention to the nature of the cases they dealt with. It will be mentioned below that the clinical picture of

summer diarrhoea in many of the epidemics investigated in America is entirely different from that which prevails in this country. One very crucial point of distinction is the condition of the stools. In cases met with in London the stools may be characterized generally as green, watery, slimy and putrid with varying acid or alkaline reaction. Blood and mucus are rarely, if ever, found. Unlike most of the cases subsequently investigated in America, Booker's cases did not present blood or mucus in the stools, and they are, accordingly, clinically comparable with our summer cases in London. As, however, the dysentery bacillus of Shiga was not described until 1898, it is impossible now to know whether Booker's cases may have been associated with bacilli of the dysentery group.

The next advance made in the bacteriology of infantile diarrhoea was made in 1903 in America, when the subject was investigated by a number of observers, notably Wollstein, Park, Collins and Goodwin, Flexner, Duval and Schorer, Weaver and Tunnicliffe. The Flexner type of the dysentery bacillus was discovered in 1899, and was at this time not completely differentiated from that of Shiga described in 1898. The object of the investigations above referred to was to determine whether these infantile diarrhoeas were also associated with dysentery organisms. The clinical features in most of the epidemics recorded were very similar. The stools invariably contained mucus, and as a rule an admixture of blood.

Wollstein examined the stools of 114 cases of infantile diarrhoea, and isolated the dysentery bacillus (Flexner type) in 39 cases. From all those cases which showed both blood and mucus in the stools, the dysentery bacillus was recovered. Agglutination reactions with the sera of the patients were obtained in a large percentage of the positive cases, but not during the first week of the disease. In autopsies the dysentery bacillus was never found outside the intestinal tract.

In the same year (1903) Park Collins and Goodwin examined cases of summer diarrhoea associated with excessive mucus and occasional blood. The majority of such dysenteric cases showed Flexner bacilli in the stools. Duval and Schorer (1903) reported the finding of dysentery bacilli in 94 per cent. of summer diarrhoea cases, including all grades of severity. They did not believe that the character of the stools was a certain indication of the presence of dysentery bacilli, but they admitted that these bacilli were most likely to be present in cases accompanied by bloody mucus. In opposition to Wollstein they also isolated dysentery bacilli from the stools of normal infants, but gave no figures in support of their statement.

Although normal children living amongst infected cases might quite conceivably harbour dysentery bacilli, we must also reckon with the possibility that these dysentery bacilli from normal infants may not have been true dysentery bacilli, but paradysentery bacilli similar to those occasionally got by Morgan, and to be later referred to. Other American epidemics associated with dysentery bacilli were reported by Cordes, Weaver, Tunncliffe, and others. It may be noted that Cordes got a positive agglutination reaction (1 in 40 to 1 in 50) with the sera of 22 per cent. of his cases.

That the dysenteric cases of summer diarrhœa occurring in America are due to infection by bacilli of the dysentery group is thoroughly established. Only scanty attention, however, has been paid to the bacteriology of the non-dysenteric cases (so-called cholera infantum) similar to those occurring in this country. Park, Collins and Goodwin examined some cases of cholera infantum, but failed to isolate dysentery bacilli. They also failed to recover the dysentery bacillus from cases of subacute diarrhœa in institutions where no cases of the dysentery type existed. Also, the blood-serum of cases of cholera infantum has no agglutinating effect on dysentery bacilli in dilutions higher than 1 in 10.

Schwarz confined his attention to cases of typical summer complaint which were characterized by profuse watery, green stools with little or no mucus. Dysentery bacilli were not recovered from any one of the thirty such cases examined, and the serum reactions were all negative.

In 1905 Jehle and Charleton, in Vienna, examined numerous cases of acute gastro-enteritis (cholera infantum) during the hot weather, but never found dysentery bacilli. Agglutination tests also proved negative.

In this survey we have thought fit to mention only those bacteriological investigations on summer diarrhœa which deserve attention, from the fact that they have been made on an extensive scale and scientifically worked out. Isolated observations on single cases or small groups of cases are of little value, and will not be referred to.

SUMMER DIARRHŒA IN LONDON.

We shall now proceed to an analysis of the results obtained by Morgan and others in the bacteriological investigation of summer diarrhœa in London during the four years 1905-1908. In these researches chief attention has been paid to that group of the intestinal bacteria which does not ferment lactose. To this group all the pathogenic bacteria associated with affections of the intestinal tract belong—viz., the typhoid and paratyphoid bacilli, the organisms of the food-poisoning group and the dysentery group.

Reports of Morgan's work during the epidemic seasons of 1905 and 1906 have already been published, but it will be convenient to discuss the results of these two years with those of 1907 and 1908, when researches on an extensive scale were carried out, demanding the labour of many laboratory workers and the kindly co-operation of hospital physicians and health authorities.

SOURCE AND AMOUNT OF MATERIAL EXAMINED DURING THE FOUR YEARS.

1905: The fæces of 58 cases of infantile diarrrhœa under treatment at the Hospital for Sick Children, Great Ormond Street, and the Victoria Hospital, Chelsea, were examined bacteriologically, along with any available post-mortem material. Of these 58 cases the clinical diagnosis in 28 was acute infective diarrrhœa, and in 30 catarrhal diarrrhœa, but the stools in the latter cases did not differ appreciably in appearance from those of the acute cases.

1906: During the epidemic season the fæces and autopsy material of 54 cases from the special ward set apart for diarrrhœa at the Hospital for Sick Children, Great Ormond Street, were investigated.

1907: Preparations were made this year for a very extensive investigation of the subject, but, unfortunately for the purposes of research, very few cases occurred, and these, as the clinicians agreed, were atypical and of only moderate severity. The material came from Paddington Green, Shadwell, Great Ormond Street, and Victoria Hospitals; 191 cases in all were examined, both in-patients and out-patients. Material from ten autopsies came also under review. Many of the cases were examined repeatedly.

1908: One hundred and sixty-six selected cases of summer diarrrhœa occurring at Shadwell and Victoria Hospitals were investigated. When possible the stools of each patient were taken for examination on three consecutive days, and in the event of the patient's death complete bacteriological examination of the organs was made. Material from thirty-three autopsies was available.

BRIEF DESCRIPTION OF TECHNIQUE.

Whatever the nature of the material, the technique followed remained essentially the same. In the case of fæces, intestinal scrapings at autopsies, portions of various organs, &c., an emulsion of the material was made in sterile broth. A loopful of the emulsion was then spread

over three plates containing MacConkey's bile-salt-lactose neutral red agar. The colourless (or non-lactose) colonies which developed were picked off and submitted to a series of biological and chemical tests—viz., indol-formation, liquefaction of gelatin, motility and fermentation reactions on various sugar media. In 1908 mannite-bile-salt agar was employed for reasons stated below (see Tables I and II), pp. 148 and 149.

The annexed table (Table I) shows all the varieties of non-lactose, non-liquefying micro-organisms met with in the fæces of children suffering from summer diarrhœa, and the percentages in which they occur. The biological characters of these micro-organisms have been completely worked out, and they have been classified according to their affinities to well-known pathogenic members of this group. During the years 1905, 1906, 1907, the exact percentages of these micro-organisms were ascertained, but during 1908, owing to the employment of a modified medium (mannite-bile-salt agar), which was justified by previous experience of the predominance of Morgan's bacillus, only the non-mannite group, to which this bacillus belongs, came under review. We do not propose to discuss in detail all these various micro-organisms, many of which may be familiar to those experienced in the bacteriological examination of excreta. Some special points, however, deserve attention.

In normal fæces the non-lactose group lags in frequency far behind the lactose-fermenting group, of which the *Bacillus coli communis* may be taken as a type. During the investigation of fæces from series of normal persons, adult and infantile, one finds colourless or non-lactose colonies only in a small proportion of the cases. The predominant group, at least in normal stools, is that of the lactose fermenters.

In diarrhœal cases, on the other hand, the lactose-fermenting group is still met with, but in less marked frequency; while the non-lactose varieties relatively increase, and may even in rare cases lead to the complete exclusion of the coliform group.

It will be seen, from the columns giving the percentages of the various non-lactose fermenters met with in the four years' work, that the bacillus designated No. 1, which will be hereafter referred to as Morgan's bacillus, predominates conspicuously over all the other varieties. The group to which it belongs—i.e., the non-mannite, non-liquefying group—is an exceedingly small one. The only known pathogenic member of this group is the dysentery bacillus of Shiga, which, of course, is readily distinguishable from Morgan's bacillus by at least three important characteristics—viz., absence of motility and absence of indol-formation and gas-production.

The second group possesses some affinities to the Flexner type of the dysentery bacillus and to the *Bacillus typhosus*—at any rate in so far as the fermentation reactions on glucose and mannite are concerned—but their reactions on litmus-milk, and their properties with regard to indol-formation, serve to differentiate them readily.

(Table II shows the reactions of the well-known pathogenic members of the non-lactose group.)

The Bacillus No. 3, unlike Flexner's, has no action on litmus-milk, and it produces acid in sorbite, which the Flexner bacillus never does. From the small percentages in which these paradysentery bacilli occur, there is no evidence that dysentery or pseudodysentery bacilli have any significance in the etiology of summer diarrhoea, as it is met with in London.

The Gaertner group, of which there are several varieties (judging by fermentation reactions alone), is headed by the true *Bacillus enteritidis* of Gaertner (isolated once in 1905, four times in 1906, and three times in 1907).

The remaining groups require little attention. Groups X, XII, XIV, and XV are common frequenters of normal and adult stools, and Group X is often isolated from drinking water. Group XV, a variety which does not possess any action on sugars, corresponds to the *Bacillus faecalis alkaligenes* of Petruschky.

OBSERVATIONS ON THE EPIDEMIC SEASONS.

It will be apparent that Morgan's bacillus occupies a predominant place among the non-lactose fermenters in epidemic diarrhoea. The fact that this bacillus was so conspicuous in the years 1905-6 led to the extensive investigations of 1907-8. So far as mere association of the bacillus with the disease is concerned, this point was proved absolutely, as the figures show. The epidemic of 1907 deserves special mention. The research in that year extended from the first week of August to the first week of October. During the first three weeks only six cases were examined, and Morgan's bacillus was not isolated. It was not till the ninth week of the research (last week of September) that Morgan's bacillus began to be isolated with frequency from the cases examined. The percentage of success in that week reached 56.5. An interesting point in this connexion is the fact that the mortality curve for summer diarrhoea in London (1907) gradually rose to a maximum during the last week of September, whereas for the preceding five years the average

mortality curve showed a maximum rise in August. Clinicians were also agreed that this last week of September was the only true epidemic period in the summer of 1907.

EXAMINATION OF AUTOPSY MATERIAL.

In 1906 Morgan's bacillus was isolated from the intestinal tract in three cases, and once from the mesenteric glands. It was never isolated from the spleen in 1906. (N.B.—Invasion of the spleen occurred in feeding experiments on young rats and young rabbits, but not in monkeys.) In 1907 material was obtained from ten autopsies. In two only of these cases had Morgan's bacillus been found in the fæces *intra vitam*, and it was in one of these two that Morgan's bacillus was recovered from the spleen. The others were negative. In 1908 Morgan's bacillus was recovered in sixteen autopsies out of thirty-three. It may be convenient to detail the sites in which the bacillus was found: jejunum once, colon thirteen times, liver twice, spleen, kidney and mesenteric glands three times, lung once, bile twice, urine three times, and heart blood three times. In some cases, therefore, it would appear that Morgan's bacillus may give rise to a septicæmia.

EXAMINATION OF NORMAL FÆCES OF ADULTS AND CHILDREN.

Throughout all four seasons the possibility of the presence of Morgan's bacillus in the fæces of other than diarrhœal cases has been borne in mind, and numerous control series have been investigated. The results in these cases may be briefly summarized. In 1905, during the winter, 20 normal children were examined, and Morgan's bacillus was found in one case. In 1908, during the epidemic season, 99 normal children were examined and the bacillus was isolated twelve times (12 per cent.). Of these cases, 60 were examined by Dr. Eyre and Dr. Minett at Guy's Hospital during the season of 1908, and four positive cases were found (= 6·6 per cent.). The remaining 39 cases came from the Paddington district, and of these eight harboured Morgan's bacillus (= 20 per cent.). Adults: 90 cases were examined during the winter of 1907-8, and the bacillus was obtained in one case only; 33 cases were examined in the spring of 1908, and the bacillus was recovered once.

CHILDREN DYING FROM OTHER DISEASES.

During the winter 1907-8 post-mortem material from 30 cases was examined, and Morgan's bacillus was recovered in four. The cause of death was broncho-pneumonia, and two of these four had had diarrhoea.

CHILDREN SUFFERING FROM OTHER DISEASES, WITH DIARRHOEA AS
A COMPLICATION.

In the season of 1908 22 such cases were examined, and the bacillus was recovered in two cases. Morgan's bacillus is therefore met with in a small percentage of cases of healthy children and of children suffering from other diseases complicated by diarrhoea. Adults only very rarely harbour the bacillus. It is to be noted, also, that the percentage of carrier cases is less during the winter season than during the epidemic season, when, it may be presumed, facility of contact is much greater.

EXAMINATION OF MILK, FLIES AND OTHER MATERIAL WHICH MAY ACT
AS VEHICLES OF INFECTION.

Milk.—In view of the fact that milk has been considered by many observers to be the vehicle for the transference of the infection of summer diarrhoea to children, attempts have frequently been made on an extensive scale to isolate Morgan's bacillus from milk samples. In the beginning of the season 1907 samples of milk from feeding-bottles, swabs from rubber teats and comforters were examined, but negative results were invariably obtained. During the season 1908 samples of milk from twenty-nine infected houses in Paddington and from twenty-six uninfected houses in the same district were examined, but only in one instance (from an infected house) was Morgan's bacillus isolated. Such a result was puzzling, especially when one considered the prevailing view of the transference of infection by milk. Some laboratory experiments made in this connexion throw a little light on this matter. When unsterilized milk was inoculated with Morgan's bacillus and incubated at room temperature for six, twelve, twenty-four hours, it was impossible to recover the bacillus by the usual methods. In sterilized milk the bacillus grew readily and was easily recovered. Similar experiments with condensed milks have not so far been made.

Flies.—A good deal of attention has been paid within the last two years to the conveyance of infection by the common house-fly. The matter was considered in detail by the London County Council in 1907 and reported on by Dr. Hamer in 1908. He came to the conclusion that flies could not be the sole agents for the conveyance of summer diarrhœa from patient to patient because flies were found in houses in the late autumn after the epidemic had ceased. It must be remembered, however, that, with the onset of cold weather, flies become much more sluggish in their movements and would therefore be less likely to spread infection rapidly. Batches of flies came for examination from infected and uninfected houses in Paddington and from a country house situated many miles from London, where no cases of diarrhœa had occurred, at any rate within a radius of two miles. The flies were killed with ether vapour and crushed with a sterile rod in peptone broth. The result was that Morgan's bacillus was isolated from nine of the thirty-six batches from infected houses and from one of the thirty-two batches from uninfected houses. It was also got in five out of twenty-four batches from the country house.

Dust.—Samples of dust (from ashbins) collected from eleven infected houses in Paddington were examined, with negative results.

Cow and Horse Fæces.—Morgan's bacillus was isolated once from fresh cow's fæces (eighteen animals examined). It has not been recovered from the fæces of the horse (thirteen animals examined).

PATHOGENICITY OF MORGAN'S BACILLUS FOR LABORATORY ANIMALS.

In 1905 feeding experiments were carried out with young rats and young rabbits. Death took place within twenty-four hours and was preceded by violent diarrhœa. The bacillus was recovered from the spleen and sometimes from the heart blood.

In 1906 Morgan's bacillus was again virulent for rats by feeding. Four small but full-grown monkeys received with their food one agar tube each of Morgan's bacillus. All four died after a period of diarrhœa which came on from two to twelve days after feeding. The diarrhœa was an acute and progressive one and was followed by rapid emaciation and death. No vomiting was observed.

The strains of Morgan's bacillus isolated during 1907 and 1908 were, for some reason still unexplained, much less virulent for experimental animals by feeding than those recovered in previous years. The monkeys

which were fed with Morgan's bacillus in 1908 developed diarrhoea which lasted seven to eight days, but all ultimately recovered.

TOXIN PRODUCTION.

This subject will be only very briefly referred to. Filtrates of broth cultures of Morgan's bacillus grown for one month are toxic for the guinea-pig, the minimum lethal dose by intraperitoneal injection being 5 c.c. The toxin can be concentrated by precipitation, so that the minimum lethal dose becomes 0.5 c.c. to 1 c.c. Great difficulties have been experienced in the immunization of the horse and donkey with Morgan's bacillus, but a serum has been obtained which possesses marked antitoxic properties.

AGGLUTINATION REACTIONS WITH THE SERA OF PATIENTS.

During the first season, 1905, forty-four samples of serum were tested against known micro-organisms like the *Bacillus typhosus* and the *Bacillus enteritidis* of Gaertner. In two cases only was a reaction obtained (1 in 60) with these micro-organisms. Also one case in which Morgan's bacillus was present gave a reaction (1 in 30) with the *Bacillus typhosus*.

In 1908 this question was attacked in a more extensive fashion. Samples of serum were taken at various stages of the disease and during convalescence. Each specimen was tested against various strains of Morgan's bacillus, including the homologous strain when possible, and also against two or three strains of the *Bacillus proteus vulgaris*. Sixty-five patients in all were examined, and thirty of these gave a positive reaction with Morgan's bacillus. One gave a reaction with the *Bacillus proteus vulgaris*. Of twelve healthy control children only one gave a slight reaction (1 in 10) with Morgan's bacillus. The dilution in which a reaction was obtained never exceeded 1 in 40, and as a rule 1 in 20 was the limit. The best results were obtained at or near convalescence. Of nineteen patients at this stage fifteen gave a positive reaction (78.9 per cent.). Of twenty-seven sera taken within a few days of admission only five gave a positive reaction. At this early stage agglutinin-development is, as one might expect, slight. In a few instances where repeated samples of blood were taken during the progress of the disease, it was noticed that agglutination occurred in

gradually higher dilutions as convalescence approached, and in gradually decreasing dilutions after the child had left hospital till a reaction was no longer obtained. The longest period after convalescence at which a reaction appeared was thirty days, and in one case it had disappeared thirty-seven days after convalescence. Of the sixty-five patients examined, Morgan's bacillus was recovered in forty-two, and of these forty-two cases a reaction was obtained in twenty-four. Of the remaining twenty-three cases from which Morgan's bacillus was not isolated six only gave a reaction. It should be noted that those cases which gave no reaction with the homologous strains frequently agglutinated stock strains of the bacillus, and vice versa.

THE QUESTION OF A FILTER-PASSER AS THE INFECTIVE AGENT.

During the season 1908 some preliminary experiments were performed with the view of testing this hypothesis. The blood and organs from three typical fatal cases of summer diarrhœa were mashed up in salt solution and passed through a Berkefeld filter. With this material feeding experiments in young cats and inoculation experiments in rats were made. The results were invariably negative.

GENERAL SUMMARY AND CONCLUSIONS.

We have, in the above statement, attempted to give a brief sketch of the very laborious researches which Morgan and other workers have carried out during the last four years on the bacteriology of summer diarrhœa. The necessity for making a careful research in the non-lactose group for some specific micro-organism in this disease was, as we have already indicated, justified by the knowledge which has accumulated concerning typhoid, paratyphoid, dysentery and food-poisoning bacilli. The outcome of this search has been that a certain bacillus (Morgan's bacillus) has been proved to occupy a predominant position among the non-lactose fermenters in the excreta of summer diarrhœa patients. In selected cases of the disease it has been isolated in the very high percentage of 63—a percentage which compares very favourably with that obtained—*e.g.*, in typhoid. Further, in the course of this work a large mass of information has been obtained concerning a very important group of the intestinal flora, and this, it is hoped, will prove of great value to future workers on these lines. To one searching for

well-known members of the pathogenic non-lactose group in the excreta, the convenience of having at one's disposal such a table as that shown you for constant reference is obvious. The association of Morgan's bacillus with summer diarrhoea has, we think, been abundantly established. Mere association, however, does not necessarily prove etiological connexion, though it must form a very important link in any chain of evidence connecting a bacillus with an infection. The fact that this bacillus is found in a small percentage of healthy children both during and apart from the epidemic season, and in a still smaller percentage of adults, does not conflict with present-day notions regarding the natural history of pathogenic micro-organisms affecting the intestinal tract. That acute diarrhoea of the summer type may also occur sporadically during the winter season is well known, and it seems quite reasonable to attribute such cases to contact infection from carriers. Extended observations on the duration of the bacillus in the intestine of convalescents are at present being made, and they are expected to yield valuable results.

With regard to the pathogenicity of Morgan's bacillus for experimental animals, we have found that rats and monkeys are susceptible to infection by feeding and, after a period of diarrhoea, succumb. Some strains of the bacillus appear to be much less virulent than others in this respect. Whether the virulence of the strain can be correlated with the severity of the epidemic, we are not yet in a position to decide. By experimental inoculation, both the bacillus and its toxin are markedly pathogenic for rats, guinea-pigs and goats.

It is, however, on the score of association, pathogenicity by feeding and otherwise (especially the results with monkeys), and the serum-reactions on the part of the infected hosts, that the claim of Morgan's bacillus to be considered an important etiological factor in summer diarrhoea mainly rests. Whether that claim will ultimately find general acceptance it is not for us to predict. We are fully alive to the many problems which its habitats outside the infected host suggest. At any rate, this bacillus is in no danger of dying out for want of an animal host to carry it on from one season to another.

The presence of the bacillus in flies from infected houses has an importance which is not necessarily lessened by the fact that flies, in uninfected houses of the same district, may also harbour the bacillus. The intercourse that may occur between flies belonging to the same district is a question on which one cannot dogmatize.

We are at present unable to offer any explanation of the positive

results obtained in flies from the country, though there is some evidence that Morgan's bacillus may be found in the bovine intestine. If this proves to be the case, the question of milk infection will assume another phase.

From the actual bacteriological results obtained, we are inclined to the view that during the epidemic season direct infection by contact with carriers or infected flies may play as important a role as infection through the medium of the milk. Sporadic cases in the winter can only be explained on a carrier hypothesis.

In conclusion, we desire to thank all those physicians, medical officers of health, nurses and sanitary inspectors whose generous co-operation and interest made these researches possible.

Our special thanks are due to Dr. Dudfield, Medical Officer of Health of Paddington, for the very valuable assistance he has given us in the collection of specimens.

It may be mentioned that a detailed report of the bacteriological work in connexion with the investigation of summer diarrhœa will appear later in the *Journal of Hygiene*.

Dr. MORGAN: I have practically nothing to add to Dr. Ledingham's and my joint communication, for although it is only a sketch of the results of four years' work I think it covers nearly all the ground. As regards the association of Morgan's bacillus with the disease, we have found that it is isolated much more frequently from severe cases of summer diarrhœa than it is from mild ones. This was shown by the low percentage in which it was isolated in 1907, when the epidemic was of a mild type. It is still more forcibly shown by the figures obtained during 1908: Seventy-four severe cases were examined from Shadwell Hospital with a mortality of 45·9 per cent., and from these Morgan's bacillus was isolated in 63·5 per cent., whereas from the fifty mild cases from Paddington, of whom none died, this bacillus was only isolated in 34 per cent. Of course, there still remains a great deal of work to be done in relation to this bacillus; at present we are investigating the "carrier" question—in other words, we are examining the stools of the children from whom we isolated Morgan's bacillus last summer and who recovered, to see whether the bacillus is still present; we are also trying to find the best method of producing an efficient antitoxin. The main object, however—that of stamping out the disease—can only be effected by the co-operation of the various sanitary authorities, and it is to them we look for help, not without confidence.

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TABLE I.—BACILLI THAT NEITHER FERMENT LACTOSE NOR LIQUEFY GELATINE.

Bacilli	Morpho- logy	Glucose	Mann- ite	Dulcitol	Lactose	Cane sugar	LITMUS-MILK				Indol	Sorbitol	Gela- tine	PERCENTAGE IN			
							First day	Third day	Fifteenth day	1905				1906	1907	1908	
Type I	1	M.B.	A.G.	—	—	—	O.	O.	Alk.S.	+	—	—	48.2	55.8	16.2	53.0	
	1A	N.M.B.	A.G.	—	—	—	O.	O.	Alk.S.	+	—	—	0.0	0.0	0.5		
	2	N.M.B.	A.G.	—	—	—	O.	O.	Alk.S.	—	—	—	5.6	0.0	4.7		
Type Flexner	3	N.M.B.	A.	A.	—	—	O.	O.	O.	+	A.	—	8.6	8.8	5.2		
	4	N.M.B.	A.	A.	—	—	O.	O.	A.C.	—	—	—	3.4	2.9	1.05		
	4A	N.M.B.	A.	A.	—	—	A.	A.	A.C.	—	—	—	0.0	14.7	6.8		
	4B	M.B.	A.	A.	—	—	A.	O.	Alk.	+	—	—	0.0	2.9	0.0		
	4C	M.B.	A.	A.	—	—	A.	O.	Alk.	—	—	—	0.0	2.9	0.0		
Type V	5	M.B.	A.	—	—	—	O.	O.	Alk.S.	+	—	—	6.9	11.7	8.3		
	5A	N.M.B.	A.	—	—	—	O.	O.	Alk.S.	+	—	—	0.0	0.0	3.6		
	5B	M.B.	A.	—	—	—	A.	Alk.S.	Alk.	+	—	—	0.0	0.0	0.5		
Type Gaerthner	6	M.B.	A.G.	A.G.	A.G.	—	A.	O.	Alk.	—	A.G.	—	1.7	11.7	1.6		
	7	M.B.	A.G.	A.G.	A.G.	—	A.	Alk.	Alk.	+	—	—	3.4	11.7	1.6		
	7A	N.M.B.	A.G.	A.G.	A.G.	—	A.	O.	A.C.	+	—	—	0.0	2.9	2.6		
	7B	N.M.B.	A.G.	A.G.	A.G.	—	O.	O.	Alk.	+	—	—	0.0	0.0	5.7		
	7C	M.B.	A.G.	A.G.	A.G.	—	O.	A.C.	A.C.	+	—	—	0.0	0.0	3.6		
Type X	8	N.M.B.	A.G.	A.G.	A.G.	—	A.	O.	A.	—	—	—	1.7	0.0	0.0		
	9	N.M.B.	A.G.	A.G.	A.G.	—	A.	A.	Alk.	—	—	—	1.7	0.0	0.0		
	10	N.M.B.	A.G.	A.G.	A.G.	—	A.	A.	A.C.	+	—	—	6.9	2.9	0.0		
Type XII	11	M.B.	A.G.	A.G.	A.G.	—	O.	O.	Alk.	+	—	—	5.1	5.8	2.6		
	11A	M.B.	A.G.	A.G.	A.G.	—	A.	A.	Alk.	—	—	—	0.0	8.8	0.0		
	11B	M.B.	A.G.	A.G.	A.G.	—	O.	A.	A.C.	+	—	—	0.0	2.9	0.0		
	11C	N.M.B.	A.G.	A.G.	A.G.	—	O.	O.	Alk.	+	—	—	0.0	0.0	4.2		
Type XIII	12	M.B.	A.G.	A.G.	—	—	A.	A.	A.C.	—	—	—	3.4	0.0	0.0		
	13	M.B.	A.G.	A.G.	—	—	A.	A.	A.C.	+	—	—	1.7	8.8	0.0		
	13A	N.M.B.	A.G.	A.G.	—	—	A.	A.	A.C.	+	—	—	0.0	5.8	0.0		

DISCUSSION.

The PRESIDENT (Dr. Newsholme) conveyed the thanks of the Section to the authors for their valuable paper. After the last meeting it required some courage to present such a paper, but their courage was justified by results, for the paper had brought them nearer a bacteriological solution of the problem of epidemic diarrhœa. Morgan's bacillus seemed to be likely to have a future before it. The authors of the paper showed great wisdom in realizing the importance of fitting together evidence from the bacteriological and epidemiological points of view, and from both these there was ample material for a good discussion.

Dr. NASH sent the following contribution, which was read by Dr. Hamer: Dr. Morgan and his coadjutors have made out a good case for the bacillus first isolated and fully described by Morgan as one of the organisms probably responsible for a large percentage of the cases of summer diarrhœa. If the definition of epidemic diarrhœa is to be limited to cases of diarrhœa where the stools have certain well-defined characteristics they might even have grounds for claiming that Morgan's bacillus is the organism which causes that form of diarrhœa. To my mind the paper contributed by Dr. Morgan and Dr. Ledingham adds additional support to the views I hold, and which I have urged since the year 1902 on many different occasions in various contributions to the medical Press, as well as in special reports on epidemic diarrhœa and in annual health reports. Briefly, I urge two main points: (1) that there is no one and only bacterial organism responsible for *all* cases of epidemic diarrhœa. In my opinion the diarrhœa-causing organism in one locality may differ in some small degree or "in toto" from the diarrhœa-causing organism in another locality. Further, I hold that ordinary putrefactive bacteria which, taken in smaller numbers in colder weather, are neither active enough nor numerous enough to be pathogenic, on the other hand in warm weather multiply rapidly, and withal become more active and produce larger quantities and more powerful toxins, and hence tend to become pathogenic in summer. Even Morgan's bacillus, though found in a larger percentage of cases examined than any other one particular bacillus, was *not* found in nearly 40 per cent. of the cases so carefully investigated. (2) I have held on the grounds of careful observation, epidemiological investigation, by process of exclusion, and by the doctrine of probabilities, that flies—more especially house flies—are among the main agents or factors in the spread of epidemic diarrhœa in warm weather. I venture to think the facts adduced in the paper support that doctrine also. To my mind it is so obvious a mode of spread—if we believe in bacterial infection at all—that the scientific proof of flies carrying the organisms is almost an insult to common sense, and may be likened almost to proving that the sun is the source of daylight. Only those who have worked at the isolation, cultivation and study of bacteria can fully realize the arduous labours which the authors of the paper

have undertaken, and I would only wish in conclusion to congratulate them on having achieved so much good and useful work.

Dr. RANSOME expressed his high appreciation of such a valuable paper. It showed not only great labour, but extreme candour. He did not notice that anything was said about the natural history of the bacillus held to be the cause of diarrhoea; more especially, no mention had been made of facts which would account for the appearance of the disease after a certain temperature had been reached. The prevalence of a mean temperature of 60° F. for a week was the point which seemed to start an epidemic. Had the authors' experiments with the bacillus enabled them to account for that fact? Flies would only partially explain the temperature coincidence.

Dr. TWORT said he desired to speak of the diagnosis of so-called new organisms by minute differences in their fermentative action on various sugars. The chief difference given on the chart between Morgan's bacillus and the dysentery bacillus of Kruse, so far as its fermentative action on the sugars was concerned, was that it produced acid and gas in glucose instead of acid only. Going down the list of bacilli and their reactions which was exhibited, one found a large number of small reaction variations, and he did not think there was any hard and fast line of demarcation between the various members of that group. The artificial line of demarcation employed resulted in groups and subgroups; and the latter were subdivided into numerous varieties, so that the typhoid were subdivided into typhoid and paratyphoid, the paratyphoid into paratyphoid A and B and so on. He believed that no one was justified in asserting that a certain organism did ferment a particular sugar and another did not; it was purely a matter of degree. When tested, variations were found which were left out in any chart; the reaction was simply put down as acid, + or -, as indicated by litmus, the quantity of acid produced, the time taken, and the various amounts of alkaline products which at the same time were being formed by the breaking up of the peptone varied with each organism tested, and also with the exact composition of the medium used, yet all these points seemed to be ignored. It was well known that the quantity of alkaline products produced might mask a slight acid reaction and so entirely alter the chart grouping of the organism. There appeared to him to be no scientific reason why, for instance, an organism showing marked acidity in glucose, after some days' growth, should be placed in the same group as an organism showing marked acidity in twenty-four hours, while at the same time they refused to admit into that group another organism producing a slightly less quantity of acid which might be neutralized by the alkaline products of the peptone; or, on the other hand, for refusing to admit into the group another organism which might easily be made to acquire the power of fermenting the sugar. It was probable that in many cases the difference was just as great between vigorous and slight acid producers as between slight acid producers detected by litmus solution and those in which a still less quantity of acid was formed which was masked by the alkaline products of the peptone. He had found that an organism which was supposed not to ferment a particular sugar acquired the power to do so

if it were cultivated for a sufficient number of generations on that particular sugar. The line of demarcation shown in the charts was an absolutely artificial one. If the sugars were replaced by various other substances the whole of the grouping shown on the chart would probably be altered. He had found, for instance, that by using glucosides instead of the usual sugars some of the organisms of the Friedländer group came out of that group and entered another group by themselves; at the same time other new groups were formed which included organisms belonging to several of the existing groups. There was no logical justification for choosing one particular substance and saying that should be the medium for the differentiation of bacilli. He maintained that the particular sugars used were used because they were easily obtainable and not from any scientific standpoint. Change the substances tested and probably the so-called Morgan's bacillus would show a large number of varieties, change the substances again and they might give reactions similar to the dysentery bacillus of Kruse. In testing the organisms of the Friedländer group he did not think there were any two organisms which showed exactly the same characters. He had done some work in connexion with the power of organisms to ferment sugars. He grew a particular organism on a sugar which it would not ferment, to see if, by subculturing it for a sufficient number of generations, it could be made to acquire the power to ferment that sugar. He found that the typhoid and dysentery organisms acquired the power to ferment lactose with the production of acid and still showed considerable agglutination with their respective agglutinating sera. It was probable that any organism could lose as well as gain characters. He believed that any particular organism, when grown outside the body, would gradually acquire the power of fermenting sugars and other carbohydrates; that it gained the power of splitting up and utilizing simple products, when this would help it to survive, and when its energies were not required to act against the tissues and products of the human or animal body. He believed that an organism would become pathogenic far more rapidly than it would lose its fermentative properties. It was well known that the virulence of some colon bacilli, as estimated by dosage, could be increased enormously in a very short time by passage through the body of a guinea-pig for about twelve or eighteen generations, but the fermentative powers of such bacilli would not change in so short a time; from this it could be understood that if an organism supposed to be non-pathogenic were suddenly thrown on to an individual who was feebly resistant, and other conditions were favourable to its multiplication, then that organism might cause a low type of diarrhœa or dysentery. He maintained, further, that having once obtained a hold it would probably gain virulence, and if now its environment were such as to favour its survival and dissemination it would probably attack other members of the community, and so give rise to a small epidemic. This organism might belong to any of the fermentation groups, and since its fermentations would be modified slightly after passing for a number of generations through the human body, certain bacteriologists investigating the outbreak would describe it as a new organism; if, on the other hand,

a non-fermenting organism cast off from the human body grew for a large number of generations outside the body, then certain fermentative powers would be acquired, as had been proved to take place in the laboratory under certain circumstances; if, again, this organism were to pass through the human body and give rise to an epidemic of diarrhoea or dysentery, again it would be described as a new species of dysentery. He maintained that if every micro-organism of the colon group showing slight differences in fermentative powers was going to be described as a new species, then the time would come when the so-called Morgan's bacillus would be tabulated into hundreds of varieties. First we would have a Para-Morgan, then Para-Morgan A, B, C, &c.; then Para-Morgan A, B, C, 1, 2, or 3, until eventually we arrived at Para-Morgan Z 99; and still the division into varieties and species would go on. He thought agglutination was not an absolute test by which one could say that a particular organism was, or was not, the cause of dysentery. Morgan's bacillus gave an agglutination of 1 in 40, but the serum of that patient, in one or two cases, gave an agglutination for typhoid of 1 in 60. If, then, the serum of a patient could agglutinate a foreign organism in 1 in 60, he failed to see that an agglutination of 1 in 40 to Morgan's bacillus was evidence that the latter was the specific cause of summer diarrhoea. Another point concerned the breaking up of the various proteid substances. It was easy to work on the sugars; one tested either for acid or gas, or both; but there was no such nice way of testing the products of peptone decomposition. He believed that micro-organisms could acquire or lose their particular powers to act on the proteid substances in the same way that they acquired or lost their powers of fermenting sugars. He did not agree that the more delicate organisms, such as typhoid, could not live long outside the human body in material containing more vigorous fermenters—i.e., in such substances as water or soil. He placed the typhoid bacillus in a medium containing dulcitate, a substance which it could easily acquire the power to ferment, and inoculated with it another organism—*Bacillus capsulatus*—which, although a vigorous fermenter of most sugars, could not easily acquire the power to ferment dulcitate. According to the prevalent views one would have expected the typhoid bacillus to have died off in a few days or weeks. But there it soon acquired the power of fermenting dulcitate, and although subcultures were taken from the mixture on to a fresh dulcitate tube every two or three weeks the typhoid was found to be active at the end of fifteen months, still growing side by side with the *Bacillus capsulatus*. A short time ago he found gas produced in the particular tubes in which he was growing them, and the question arose as to whether it was the typhoid bacillus which was producing the gas as well as the acid, or whether the *Bacillus capsulatus* had also acquired the power of fermenting dulcitate with the production of gas. Therefore he isolated the organisms out, and he was somewhat surprised to find that the typhoid produced acid only in the dulcitate, which it had acquired the power of fermenting, while the *Bacillus capsulatus* colonies isolated produced no acid or gas in dulcitate, so the question was—Had he failed to isolate the organism which produced the gas, or was it that one of the organisms could only produce gas in

dulcitate when mixed with the other? He therefore took those two organisms and mixed them together again in another dulcitate tube, and found they showed marked acid and gas production as before. He found the normal typhoid produced no acid in dulcitate, or only very slowly after it had been grown outside the body for a long time. He found that the experimental capsulatus mixed with normal typhoid could not produce gas in dulcitate; from this it was clear that the *Bacillus capsulatus* could only produce gas in dulcitate when acting in combination with the changed *Bacillus typhosus*. It appeared, in fact, to be a case of acquired symbiosis, and if a highly pathogenic micro-organism could, by becoming modified, act in symbiosis with another modified but less pathogenic micro-organism in the laboratory, then there seemed to be no reason why it should not do so when in such situations as water and soil, in which case it might survive for a very long time, and not be killed out as most bacteriologists thought. He thought that if Morgan's bacillus could be proved to be the chief causative organism of summer diarrhœa, then it would be better to describe it as a variety of dysentery. He thought the differentiation of such organisms into different species, relying upon slight differences in fermentative powers, had gone far enough, if not too far, and there was no justification for putting Morgan's bacillus into a new group simply because it produced acid and gas in glucose instead of producing acid only, as is done by the dysentery bacillus.

Dr. COPEMAN congratulated the authors on their laborious research. The difficulty in regard to summer diarrhœa had always been that practically nothing was known of its bacteriology. Many micro-organisms, both pathogenic and those which under ordinary circumstances were not so, had been laid under contribution as the cause, and he wished to ask whether there was a possibility that certain anaerobes and other micro-organisms, for instance moulds, or even higher forms such as amœbæ, had anything to do with this epidemic diarrhœa. He thought the evidence seemed to show that various micro-organisms isolated by Metchnikoff and other observers might, on occasion, have some etiological relationship to the disease. The authors said the association of Morgan's bacillus with the disease had been absolutely proved. His own view was that it was not absolutely proved, because they found it in cases which were not of the epidemic variety, and they did not find it in some cases which, clinically, were that disease. They referred to the association in 63 per cent. of selected cases; but they had not, perhaps, heard Dr. Hamer's crushing indictment of the typhoid bacillus itself at the previous meeting. Mere association did not necessarily mean anything in the etiology of a disease. Few people, if any, now thought the *Micrococcus neoformans* had anything to do with the origin of cancer, yet Dr. Lazarus Barlow said that in the last four years it had been isolated in at least 90 per cent. of the cases of malignant growths at the Middlesex Hospital examined in his laboratory. But with regard to the spread of the disease, the authors had exploded one or two bombs among epidemiologists. Most writers had held that milk was the chief carrier of epidemic enteritis, and the provision of sterilized milk was said to have reduced the infant mortality from the disease. It was extraordinary

to hear that Morgan's bacillus died out in milk which had been kept at room temperature for as short a period as six hours. Was it likely that members of an artisan's family, for instance, would get their milk within that time? If that statement was correct, milk could not be an important carrier of the disease, but he would be surprised if milk really had nothing to do with the matter, since investigation by many skilled observers had apparently shown, very definitely, that epidemic enteritis was less frequent among children who were breast-fed than among those who had been fed with cow's milk or artificial food. The authors had said there was some evidence that Morgan's bacillus might be found in the bovine intestine, and that if such proved to be the case the question of milk infection would assume another phase. But what phase would it assume? Because if the organism could not live in unsterilized milk, how was the milk infection question altered by the sterilization of milk? Possibly the cow might occasionally yield sterile milk, but he had never heard the suggestion that it could yield sterilized milk, and therefore, according to the authors' own showing, its milk could hardly be responsible for infection from Morgan's bacillus derived from its own intestine and carried into the milk during the operation of milking. He had been entrusted by the Local Government Board with an investigation into the possible carriage of infection by flies, and he had had the advantage of the co-operation of Dr. Hamer in connexion with the work carried out by that gentleman for the County Council. He was interested to know that the suggestion he made then commended itself to the present authors—namely, that owing to the fact that flies were more sluggish in the latter portion of their existence, they were less likely to infect either milk or the infant. In Egypt ophthalmia was undoubtedly carried direct from one individual to another by flies, and possibly epidemic enteritis might be carried in similar fashion by the same agents. The question of flies collected from infected houses containing organisms, and flies not containing them from uninfected houses, he did not think of much importance, as the authors had suggested, because the flies from uninfected houses might previously have come from infected houses or direct from dustbins. And the authors did not say whether they thought the carriage of infection by the fly was caused by contagion on the surface of its body or in its intestinal tract. As the investigators, in making their cultivations, broke the flies up in nutrient broth, it might be either. He had found that the colon bacillus would survive for a long time in the fly's intestine, as also, apparently, in its dejecta, and it would be interesting to know whether the same was true of Morgan's bacillus. Except in a very heavy wind, he did not think flies were able to traverse a distance of two miles. In some experiments which Dr. Hamer and he carried out with flies, from a glue factory at Bermondsey, which, owing to their green-coloured body, were easily recognizable, it was found that these flies were not to be identified at a greater distance from that factory than 250 yards. The authors said "The intercourse that may occur between flies belonging to the same district is a question on which we cannot dogmatize." He was tempted to ask whether that intercourse was social or

sexual. No doubt the answers to some of the questions asked would be evolved in the later steps of the research.

Dr. BULLOCH said there had been some iconoclastic suggestions from Dr. Twort. If they had been forthcoming from an amateur they would have been put down as foolish; but under his own observation Dr. Twort had carried out the work, and with extraordinary care, and had submitted the results to bacteriologists in Europe and in this country. Personally, he (Dr. Bulloch) had very slight knowledge of that particular group, and he felt glad that he had not wasted his time in studying things which were beyond him. With regard to bacteria acquiring fermentative qualities, Dr. Twort had recently sent a culture of typhoid which fermented lactose to Dr. Ledingham, and he wished to ask Dr. Ledingham whether he considered the culture which he obtained from Dr. Twort was typhoid or not.

Dr. HAMER said he desired to draw the attention of the Section to an interesting paper on "Summer Diarrhœa," in the last issue of the *Zeitschrift für Hygiene* (1909, lxii, p. 199), by Dr. Liefmann, of Halle. The paper dealt more particularly with the use of sterilized milk. Experience at Halle showed that the children fed on sterilized milk suffered from summer diarrhœa to as large an extent as did the children not so fed. Some interesting statements were incidentally made bearing on the bacteriology of summer diarrhœa. Dr. Liefmann observed that all attempts to demonstrate a causal organism had proved unavailing, and he quoted the dictum of Pfaundler that children sickened *ex alimentatione*, and that they died *ex infectione*. This consideration led him to advert to the importance of secondary invaders, which as he said might be spoken of as "nosoparasites," or, again, there might be applied to them the designation "facultative pathogenic organisms" to distinguish them from "obligate pathogenic organisms." Dr. Liefmann argued that in any case one had to recognize the existence of transitional forms between one group and the other, and germs belonging to one and the other group occurred side by side in "mixed infections." If possibilities such as these had been submitted for their consideration at a meeting of the Epidemiological Section there would doubtless have been manifestations of dissent and expressions of incredulity. One member would have complained that the facts of disease were being made more difficult to understand, and another would have threatened them with the razor of Occam which frightened them at their last meeting. The interest of the utterances was, however, that they came not from a mere epidemiologist, but from an expert bacteriologist, and, further, that they appeared in the *Zeitschrift* of Robert Koch himself.

The PRESIDENT remarked that it was unfortunate that the work of the authors had not yet extended to the subject of condensed milk. Some years ago he found—and the observation had been confirmed by Dr. Richards and others—that in proportion to the total number in each group there was a larger amount of summer diarrhœa among the babies who were fed solely on condensed milk than among those fed on fresh cow's milk. That was

important from an epidemiological point of view, because from the standpoint of the present paper either one must conclude that Morgan's bacillus had remained in the condensed milk, or that it had got into it in domestic life after the opening of the tin of milk. In either case the importance of domestic infection and of domestic cleanliness was largely enhanced, so far as the disease now being discussed was concerned, by this consideration. It would thus be most important that the incidence of Morgan's bacillus in domestic milk or in domestic dirt should be further investigated. It was interesting, but puzzling, to find that Morgan's bacillus was present in flies in the open country.

Dr. LEDINGHAM, in reply, said they realized the importance of investigating condensed milk, but last year they had too much work on hand to do it. They hoped this year to be in a position to ascertain whether Morgan's bacillus would remain alive in condensed milk as well as in sterilized milk. Further experiments with unsterilized milk were also indicated, as there always remained the possibility that Morgan's bacillus was inhibited in its growth and not actually killed by other micro-organisms in the milk. With regard to Ballard's views on the relation of the ground-soil temperature to outbreaks of summer diarrhoea, he wished to point out that this temperature relationship was a very general one so far at least as infections of the intestinal tract were concerned. Dysentery, cholera, and to a less extent typhoid, occurred mostly during the warm season, probably because the organisms causing these affections attained their maximum growth at that period. So far, the bacteriological results did not explain the exact correspondence which had been stated to subsist between the attainment of a certain ground-soil temperature and the outbreak of summer diarrhoea. Dr. Twort's views on the fermentation reactions of intestinal bacteria and the question of mutation were very valuable, though they were not quite relevant to the matter at issue that evening. He had been familiar with these opinions of Dr. Twort for some considerable time, but, after all, the lively speculations in which Dr. Twort had indulged rested on an extremely small basis of actual fact. Whether these organisms could be trained to mutate or not did not affect the main thesis that the fermentation reactions of these organisms as found in nature were remarkably constant. When Dr. Twort had the courage to search in water or elsewhere for the *Bacillus typhosus* disguised as *Bacillus coli* and succeeded in unmasking it, he would have even greater respect for his views than he had now. Dr. Twort had stated his belief that Morgan's bacillus might be a dysentery bacillus. This was comforting, but why it should be a dysentery bacillus rather than a typhoid bacillus or a coli bacillus he (Dr. Ledingham) could not understand. In reply to Dr. Bulloch, he could not yet give a definite answer to the question whether the organism sent him by Dr. Twort was a changed typhoid bacillus having the power of fermenting lactose. It was being investigated by a colleague, and so far it appeared to produce a reaction on lactose, but that was not the whole question. With regard to dulcitol referred to by Dr. Twort, there appeared to be some evidence that the typhoid bacillus had the power

of fermenting dulcete (late reaction) without any special training. Dr. Copeman's remarks on the milk question and the fly question in relation to Morgan's bacillus were very apt. Certainly the fact that they had had so little success in the isolation of Morgan's bacillus from milk samples was surprising, but it had to be remembered that the bactericidal property of fresh milk and the presence of other micro-organisms were factors which they could not yet estimate accurately. As Dr. Copeman indicated, horse-dung and not cow-dung formed the favourite breeding-ground of the domestic fly, and it was in the latter that Morgan's bacillus was found. More extensive investigations on these special points were necessary. So far they could not say whether Morgan's bacillus was present in the intestine or on the exterior of the fly. Dr. Hamer had referred to a recent paper by Liefmann on the conditions of summer diarrhœa in Halle and Leipzig. He (Dr. Ledingham) had also read that paper. It contained no contributions to the bacteriology of the subject. What had impressed him (Dr. Ledingham) most was the author's conclusion that infection was as liable to occur from contact as from milk, and that the mortality among children fed on sterilized milk was probably as great as among those not so fed.

Epidemiological Section.

March 26, 1909.

Dr. A. NEWSHOLME, President of the Section, in the Chair.

On the Discrimination of Unrecognized Diseases and on a Disease of Overcrowding in Ships, especially at Malta.

By Fleet-Surgeon W. E. HOME, R.N.

It is sometimes interesting to take a famous book by one of the old masters of medicine and read in it to see whether it may not still have some lesson for us now. What books of these survive were written by very capable men; that is why they have not been forgotten, and the talent these elders brought to the study and solution of their problems will often show us a good method for attacking ours of to-day.

The classics in my branch of the profession are Pringle, Lind, and Blane. Sir John Pringle (1707-1782), Physician-General to the Forces, and later President of the Royal Society, in 1752 brought out his great work on the "Diseases of the Army." He was "the Founder of Military Medicine" (J. F. Payne), the first to prevent dysentery and hospital fever. James Lind (1716-1794), for thirty-five years in charge of Haslar Hospital, proved the usefulness at sea of distilled water and of lime juice to prevent scurvy; 1754, "Treatise on Scurvy"; 1757, "Health of Seamen." Sir Gilbert Blane (1749-1834) went to the West Indies as Lord Rodney's private physician, but, winning the admiral's good opinion by professional skill and personal bravery, was made physician to the fleet. He got lime juice made an issue, which Lind had never achieved, and I believe it was his representation that got a uniform established for our sailors to prevent typhus fever. In 1786 he published "The Health of Seamen."

In the natural history of those days the classification of animals and plants was artificial; not enough knowledge had yet been acquired to make a natural system possible. And it was the same with the diseases. An etiological classification, such as we attempt to-day, a classification by cause, was unattainable; all the differentiation they had was clinical, by signs or symptoms, or by the regions or organs attacked.

Pringle knew intermittents, diagnosed pneumonia, pleurisy and consumption; the other diseases he reports from his field hospital in the spring of 1743 are: "rheumatic pains, with more or less fever;" "inflammatory fever, without rheumatic or pleuritic pain;" "hard coughs without fever;" "fluxes;" "some inflammatory symptoms." "Fever" were the most numerous occurring of all diseases. Lind reports that 2,174 cases were admitted to Haslar Hospital in two years (1758-1760)—38 per cent. of all admissions. Blane reports 806 cases in Rodney's fleet in 1782—44 per cent. of all cases added to the sick list. The exanthemata were not included, but even so the class of "fevers" was so large it had to be broken up somehow, and with the knowledge they had they could only group their cases by the time and place of their occurrence. Thus Pringle and Francis Home, my great-grandfather, both subsequently professors in Edinburgh, who both served with the army in the Low Countries in 1740 onwards, and both wrote books about their service—books which are in the Society's library—grouped the fevers year by year for descriptive purposes (fever of 1741, fever of 1742, &c.), and Blane in the West Indies follows quite the same method.

I submit these facts to this Section, whose study is the natural history of disease with a view to its prevention, that it may be realized why prevention in those days lagged so behind. It is difficult to form a consistent theory of the cause of a single disease—how impossible to analyse the causes of a mixed assortment of diseases—and I desire to simply restate what everyone here knows, that a good classification of diseases is a great help towards prevention. As we now know that phthisis, lupus and tubercular meningitis are not separate diseases, but only separate evidences of the same *noxa*—the tubercle bacillus—a case of either reinforces the demand for the prevention also of the other, as the harmonics strengthen the fundamental note in a chord.

Now from that great mass of fevers which the old observers report there have been extracted and identified several groups—malarial, remittent, for example. Seventy years ago typhus and typhoid were discriminated after long search: Bruce lately extricated the Malta fever conveyed by goat's milk, and there are others; but we who go abroad

still find cases to which we cannot satisfactorily apply the names we see in the text-books, fevers for which we do not know the cause.

It was a great advance to separate typhoid from typhus; difficult clinically, and for two reasons. Unlike other diseases, which, similar at first, seem to differentiate themselves as they progress, these two fevers end in a very similar typhoid condition. This they were particularly apt to do in the olden days, when, as you will remember, patients in fever hospitals had only 600 cubic feet of space. Ventilation was thought harmful, and venesection must, one thinks, have caused all patients to present after long illnesses a similarly exhausted appearance.

There was another and a greater difficulty. These fevers were apt to occur in different places each by itself, and as communications were less free and frequent then than now, authors might not have seen them both. I have always been told that in the thirties of last century there was animated controversy between the schools of Paris and Edinburgh about the natural history and treatment of "fever," each thinking the other unwise, or at least lacking in clinical acumen, whereas both were right, though each saw only his own side of the shield; "fever" in Edinburgh was typhus, "fever" in Paris was typhoid. That neither of them would give in did both schools credit, for so we came to know the truth.

This raking amongst the embers of forgotten controversies will be of benefit to us if it makes us reflect and inquire whether such controversies are not going on amongst us now. Is it at all possible that among the less well-known and unnamed fevers of to-day there are not some as distinct as those others, which, too, when discriminated, might be in their turn prevented? Public health has so greatly improved, we can expect no so marked results as followed the recognition of typhus and its subsequent prevention. Still, it may be worth our while to seek out and differentiate these others that in time, as I say, they may be stopped.

It is a little novel this idea of going to look for a new disease; they are generally discovered more or less by accident. To pick out a new disease is much as if one were in a club where habitually one met some thirty or forty Englishmen, but one night there appeared a Scotchman, and would one notice him? Essentially different he would be, but in appearance he might be very like an Englishman, or he might be very different. If one had no warning to expect a different sort of stranger one might class him in one's mind with some other member one knew like him, unless he spoke and failed us in English, which would be a sort of Widal reaction to test him.

Each new case as we see it we class with some others we have already seen, unless something very striking shows it to be peculiar, or unless many others suddenly begin arriving far more like one another than like any of those we have known about before. Each of us looks at cases from the point of view to which his own experience has conducted him, and his comparisons may be as different from those of his neighbours as was that of the little town-bred child, on her first train journey in the country, who amused us all as we passed some sheep by her excited surprise at the "funny dogs those are." And probably that is how we shall meet a first case of a new disease. It will be "a funny case," "an odd case," an unpaired case that is. It is these odd cases, these residua, that repay investigation. That is how radium was found—in residues. That is how they find gold at the mines.

As to the local assignment of "fever" of which I spoke, I was much struck with this in the nineties on the East Coast of Africa. Durban fever used to be talked of in the seventies. In the nineties you heard no more of Durban fever. Durban had got settled, had sown, reaped, and was done with its wild oats. A reputation for fever now attached itself to the ill-built, untidy, or sparsely populated places up the coast—Beira, Chinde, Quilimane. These fevers all seemed to be malarial, but in those days we had no exact diagnostic, and we had advantage from calling these cases by names of the places where they originated, for we never knew when some other fever might appear, nor from whence—some other fever terrible as the dreaded blackwater fever—and the name it got would be a warning to the newcomer to give as wide a berth as possible to the place whose name that deadly fever bore.

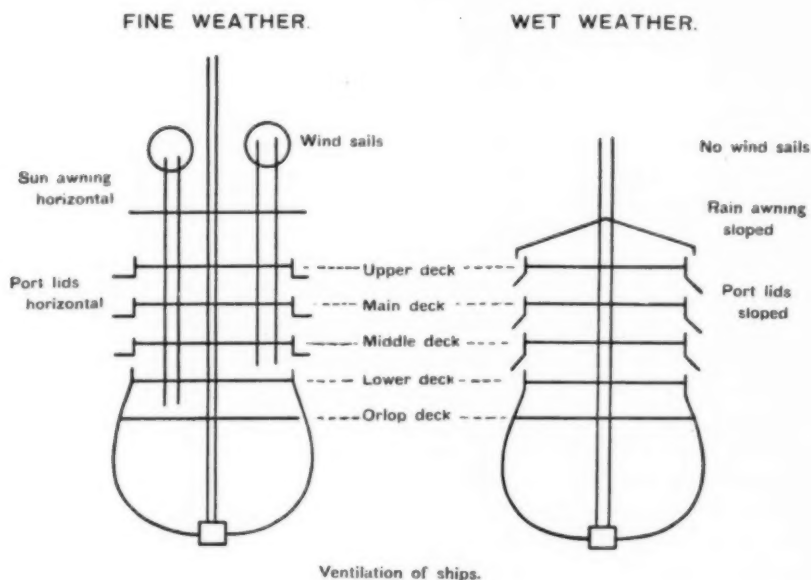
In the early nineties we classified fevers into (1) zymotic, (2) continued, (3) malarial (which might be remittent or intermittent). Zymotic in East Africa (except dengue) we did not see; continued meant typhoid; intermittent are a well-marked class, and all other fevers were returned as remittent and ascribed to malaria, or in those days the same thing—climate. The preoccupation of every surgeon on this East African station was "fever"; it was climatic. You could not tell when it would visit your ship, or when it would leave. It was climatic, so nothing could be done to prevent it, except keep out of the sun, but it was good to see your ship as clean, dry, and well ventilated as possible, for that increased comfort.

When I joined the station I was very anxious about those men, obviously the most exposed to the climate, those who went away in boats cruising after slave dhows, and proposed to my captain I should

medically examine them before they started and only allow the healthiest to go. He, however, who had been on the East Coast for a year, laughed at my anxieties, and assured me they were unnecessary. I could not see how this could be if the fever was, as everyone said, due to climate, but I did not press the matter, and later I found the captain's view correct, for the fever after all was not really due to the climate directly at all.

To anyone reading the reports on the health of the East African station yearly appearing in the Blue-books from 1880 onward it must shortly become evident that within the group, "remittent fever," were included cases of two quite distinct diseases—the first a true malarial remittent, not picked up aboard ship, but always contracted ashore, and generally, almost invariably, by sleeping ashore ten to fifteen days previously. See *Lancet*, October 28, 1899, where I repeat from the Blue-book for 1887 the history of a party of ten who passed the night ashore. The seven men slept and all got fever; the three officers who did not sleep escaped, for, being always kept awake by the mosquitoes and always brushing them off, they prevented any single mosquito from settling and so infecting them. For this fever quinine was specific. The second fever, on the other hand, appeared to have its origin exclusively in ships; neither in boats, whose crews continuously escaped it, nor ashore; nor did it spread from ship to ship; nor was it carried by wind. It might occur in ships at Zanzibar, but in no relation to the occurrence of fever ashore. In 1880 it was severe in H.M.S. *London*, the stationary ship at Zanzibar, when Zanzibar was healthy. In 1881 it attacked both ships and shore. In 1883 the epidemic in the *London* did not spread to the ships near her, so the infection was not carried, or at any rate did not spread, by wind. Further, it used to be noted that the fever diminished in the *London* when a cool wind blew off shore. In 1887 there was a bad season ashore, but ships suffered little. In 1884 it was reported from the *Euryalus* at Massowah, when it specially attacked the boys. She was described at the time as overcrowded, and those of her ship's company who were landed in Massowah were healthier than those who remained aboard, so it was not Massowah which gave it to them. This fever did not attack the natives serving on board ships, and was noted to be specially prevalent at Zanzibar during the great rains in the first quarter of the year, both of which facts were held to prove its climatic origin. My view is that it was due to want of ventilation and overcrowding in the ship herself. It occurred in damp ships like the *London*, which was so full of rotten timbers that she was broken up in

1884. It occurred in the great rains; it occurred in ships which, with full complement, had been quite unnoticeably healthy in harbour but were sent to sea in bad weather, for in each case ventilation was impaired. In Zanzibar Harbour in dry weather every port and scuttle is wide open and the awning, high up and horizontal, allows air to escape from the ship as freely as possible. At sea in bad weather the scuttles, &c., are closed, and in harbour during the great rains ports are half closed and the awnings sloped, imprisoning the air inside the ship. I did not know all this in 1895. I wish I had. That



Ventilation of ships.

Vertical transverse section of 3-decked ship

FIG. 1.

the natives escaped it too does not prove it was due to the climate; their conditions of life were quite different. They had far more fresh air than any white man—they lived on the booms, a sort of flying deck above the upper deck, out in the open air—and this may equally explain their immunity, and now you see why the men away in boats escaped. Every now and then one of them would sleep ashore and get

malarial fever, but more fortunate than their envied neighbours, with all the comforts of a ship, who with these also had the risk of infection, the boat's crew were thoroughly exposed to the climate and so remained healthy.

The fever of which I am speaking may be described as an asthenic pyrexia without special features. After two days' malaise and headache the patient would get nausea, retching and vomiting; temperature 101° F. or 102° F.; pulse about 100; skin moist; bowels confined; headache, pain in loins and sometimes even already marked exhaustion. Stage of advance: Temperature 103° F. to 105° F.; skin dry, often pungently hot; eyes suffused, headache, restlessness, perhaps delirium; tongue dry, thirst, anorexia, perhaps much vomiting; bowels confined; pulse and respiration fast; symptoms worse at night. We thought this proved the fever to be, as we called it, remittent. I nowadays often wonder if it was due to the diminished ventilation, due to the stillness, the airlessness of a tropical night. Convalescence: Great debility, skin clammy; appetite slowly returned. Complications: Hæmaturia reported by one medical officer; I have known it followed by insanity; definite and routine sequelæ there were none; duration ten days, and relapses may occur. Treatment: Quinine is a good tonic but no specific.

In the *Archives de Médecine Navale* for May, 1895, is noted a report on the fevers of Massowah undertaken by medical officers of the Italian Navy, who found three different fevers: (1) Malaria cases, imported only; (2) an indefinite fever, no germ isolated; (3) typhoid; (a) abortive—*Bacillus typhosus* in blood from finger; (b) developed. This fever I am mentioning—that reported from the *Euryalus* at Massowah in 1884—may perhaps be included in their No. 2.

I was in those days serving at Zanzibar in the *Swallow* sloop, and we tried to get her thoroughly aired—windsail in every hatch and wind-scoop in every scuttle; we all felt this did us good. The old proverb is that a smart ship is a healthy ship, and she was pretty busy as well as pretty that *Swallow*, but I regret I cannot give statistical evidence of her health. We got a great deal of malaria when landed, but we thought we did not have so many as usual of our fever cases to invalid.

The lesson of these East Coast fevers may be thus stated: (1) the importance of accurate diagnosis—no prevention of the ship fever could be other than blind so long as it was confused with the fever due to malaria or climate which you could not change. If we had dreamed it

was due to stuffiness of the ship, I am sure the captain would have had the men sleeping away in boats or going route marches to let the ship dry, or something would have gone on being done till the right thing was found; (2) the importance of not being led astray by popular etiology; (3) the little direct influence on disease of that resultant of meteorological conditions called climate.

I must next ask you to consider another example of inaccurate diagnosis which also hindered prophylaxis and did much more harm, as there were far more people concerned. I now refer to what we used to call Malta fever. When I entered the Service in 1885 the crux of the Naval Medical Department, as we who then entered very soon learned, was Malta fever—a great many cases for which we had no satisfactory treatment, with many invalidings though few deaths. No prophylaxis could be hopefully considered, for the disease we knew was due to the climate of Malta, which we could not alter, and, besides (here came in the mischief of faulty diagnosis), it attacked the soldiers as well as us, so we could not think it due to ships. This fever was not described in the text-books; there was no book about it; we believed the people who had seen much of it knew something about it, but it was surprising how little they could tell us.

In January, 1886, I was told off for the *Alexandra* in the Mediterranean, and naturally searched out all the information I could get about this fever. The most weighty came from the Inspector-General at Haslar, who had had long experience of the Mediterranean, and kindly talked to me about this fever, concluding by advising me to keep a great-coat in our mess boat to protect me in going off to the ship at night. In fact, the only prophylactic then known was—avoid chill. We of the new ship's company for the *Alexandra* were sent to her from Portsmouth on February 20 in H.M.S. *Tamar*. Reaching Malta on March 1 we were turned over to H.M.S. *Hibernia* for two months till the *Alexandra* had refitted. The *Tamar* was crowded, and in our own ship's company had a dozen cases of pyrexia, indistinct at first, but later becoming pneumonia or bronchitis, and similar cases continued to occur after we left her till the end of April, when these cases were replaced by others called fever.

Marston, in 1860, wrote a paper on fevers at Malta, which appeared in 1863 in the *Army Medical Department Report* for 1861. He classified them as simple continued, typhoid and gastric remittent. This classification practically it was that was followed in our returns in 1886. The simple continued fevers were the slight cases. Some medical

officers there were who thought this a definite disease like typhoid. Others thought it a limbo for slight cases of fever due to indiscriminated infections; all slight cases of remittent and abortive attacks of typhoid, for example, fell into this group.¹ The prolonged and more serious cases were divided between typhoid and remittent. Those with more definite abdominal symptoms, diarrhoea, three weeks' pyrexia and then lysis, and especially if they had a fatal issue, would be called typhoid, particularly if rose spots had been seen or a post mortem had shown the ulcerated Peyer's patches. The rest that could not be called typhoid would be classed as remittent, and were of all degrees of severity, from the uncertain week-long pyrexia, which would have been called simple continued had not the temperature been so high and the symptoms so severe, to the well-developed case one sometimes saw with a prolonged weary clinical course without strongly marked features but going on relapsing, sometimes with well-marked rheumatic sequelæ (this I used specially to note in cases seen in the military hospitals), and a death-rate of 2 per cent. Except the typhoid we thought all these fever cases to be the same and peculiar to the island. They were returned as remittent, but in conversation we called them Malta fever. Our attention was rather focussed on the bad cases, of which I saw a good many in my first week at Malta, when sent to do duty in the Naval Hospital and later at the Valetta Hospital, where was living Sir David Bruce, who showed me more.

We were then at Malta seeing a quantity of fever cases really undistinguished except by severity between slight febricula and a four or five months' case of Malta fever, so I think there is a little excuse for us that we did not realize that they ought to be broken up into groups. Another difficulty I will tell you in a minute. None of us knew how these cases came, but the most frequent hypothesis was dirt. Bruce talked of soil saturated with the filth of centuries. Gipps, in his paper read to this Society, ascribed the fever to the sewage contamination of the harbour water, and noted that ships that were suffering lost it soon after leaving Malta and did not pick it up in other Mediterranean harbours. Thinking it over again now I feel that the Army and people ashore generally had more rheumatism than fell to the cases in the Service afloat. I did notice then that young soldiers one used to go

¹ Simple continued fever is by many considered to be abortive typhoid. It is reported, however, by Surgeon J. G. Fowler, R.N., that the *Powerful's* landing-party of 250 men who went through the siege of Ladysmith suffered from this fever equally whether below or above the age of 25 years. This is quite a different age-incidence from that of enteric fever (Blue-book "Navy Health," 1899, Appendix).

and see at Cottonera Hospital had a rheumatism much more crippling than that our patients experienced. Here was the hint of a discovery which, alas! I did not make in time. What masked this observation, what gave our people rheumatism, some of them, was that cases sent to our hospital sometimes got fever there (popular lay opinion said every case) from the milk, and these cases might very well have rheumatism even if none had it in ships from their primary infection. Then cases were called remittent fever which obviously were not so. I remember Inspector-General Todd, then senior surgeon in the *Alexandra*, showing me that a pyrexial case which I thought to have intractable Malta fever was really suffering from inflammatory fever and was at once cured by the opening of an abscess, and there were other people then just as young as I.

In 1891 I was sent to do duty at the Admiralty for a fortnight, and, having access to records, read up many reports on this fever, and that winter, at Inverness, in medical charge of but a small ship's company, had time to write a report on Malta fever for the journal we send into office very year. This report, amplified, became, in 1895, my thesis for the degree of M.D. in Edinburgh. In that thesis I tried to establish, by investigation of its epidemiology, that Malta fever was due to overcrowding, and that it could be prevented by improving ventilation. So many lines of investigation seemed to lead to this same conclusion that I was confident this investigation of mine was sound. When, therefore, the Malta Fever Commission was being appointed I sent Bruce a copy of that thesis to help them. He said he would lay it before them, but, to my surprise, indicated that in his opinion my paper would not advance them much.

In 1906 Bruce's Commission had stopped Malta fever, and I found the surgeon I had got in H.M.S. *Exmouth*, G. M. O. Richards, a very capable officer, knew about the recent investigations in regard to this fever, he being just back, too, from Malta, and I trusted my valuable paper to him. He read it indeed, but to my vexation he, too, said it was of no particular interest, adding that these older papers were quite unimportant now and gave no enlightenment, since the cases on which they were based could not—it was not possible they could—have been accurately diagnosed.

I may explain here, as some members may not remember the dates, that in 1886 Bruce found in the spleen of fatal cases of Malta fever a micrococcus, the *Micrococcus melitensis* as it is now called, and by inoculating this into a monkey he proved it the cause of the disease. Several

of us then went about pointing out the great cleverness of the medical profession which could find the cause of a disease so troublesome. We were, however, only met by our lay friends with biting remarks about the uselessness of knowledge of a disease which did nothing to help its cure, and our arguments that advances in knowledge were valuable in themselves had few supporters. However, Bruce's discovery had, at least, made the accurate diagnosis of this disease possible in the post-mortem room, which was something, though otherwise it seemed to have been of no practical benefit, and so things remained for ten years, until the Widal reaction was discovered. Sir Almroth Wright soon noted that the serum of Malta fever cases clumped its micrococcus alive or dead. In about three years this reaction became generally used in Malta. Bruce's discovery had now led to the diagnosis of cases during life, but had certainly not yet improved treatment.

In ten years more we find the Commission hunting all over Malta for this micrococcus, finding it in goats, then in their milk, and so, in July, 1906, this fever was stopped, and there practically has been none since in the Army or the Navy. And it is better to prevent a man from wanting treatment for a disease than to give him, sick, the best treatment that could be.

Does not this all show that it is worth while to obtain any increase in knowledge, for, trifling as it may appear at first, there is no knowing how far-reaching it may prove; 1886: micrococcus found and diagnosis possible in post-mortem room; ten years on (1897 to 1900): serum reaction and diagnosis made possible of cases during life; twenty years after (1906): Fever stopped, and no diagnosis required?

The history of the prevention of this fever is particularly interesting. It was so restricted in area, and with so little to differentiate it in life from other fevers, that one cannot see how it was to be investigated unless Bruce had found the micrococcus—a specially hard nut for us of this Society, who, when we hear of a disease, think of it as something to be prevented. How long would it have been before someone thought it worth while to trace the effect on each of the consumers during a month of the milk of each of a flock of twenty apparently quite healthy goats? Beside such a toilsome research, even Dr. Davies's patient and skilful unravelling¹ of the milk routes by which typhoid came to Clifton seems quite easy.

We must now go back to Surgeon Richards's opinion on my thesis. It was quite clear he was correct; that thesis of mine did seem

¹ *Trans. Epidem. Soc. Lond.*, 1897-8, xvii, p. 78.

valueless. Malta fever was due to a specific infection of goat's milk. Whatever could I have meant by proving it was due to faulty ventilation and overcrowding? Another thing puzzled me much. What were we in the Service afloat doing with all that Malta fever from infected milk, when I suppose no milk was drunk on board ship except by the officers' messes and by their servants? The men never drank milk to my knowledge, unless they were on the sick mess. Where did they get the fever?

Six months ago I was put on half-pay, and in writing up the results of overcrowding for the ventilation section of a book on naval hygiene, I recognised that the Malta fever of which I had written in my thesis was one of these diseases of overcrowding, and, at last, became aware that in Malta, about 1886, there were, at least, two fevers unknown to the text-books of that day—one *Micrococcus melitensis* septicæmia of Bruce, due to infected milk, which specially attacked (1) people ashore, (2) the Army, and (3) patients of the Service afloat who got milk in sick bays or in hospital. This, by much the more important of the two, is the fever commonly known as Malta fever. The other fever, that on which I am trying to focus your attention to-night, which also occurred mainly, though perhaps not exclusively, at Malta, was a fever of overcrowded ships, attacking, so far as we yet know, only the Navy, and of them only the crews of certain ships. This fever has since gradually decreased, till now the structure, management and equipment of ships has so improved that one hears this fever has completely disappeared.

Bruce concerned himself only with the first; I concerned myself only with the second. Again the case of opposite sides of the shield.

So now I have to show you that our cases were due to overcrowding. You might think that overcrowding could not but be recognized and rectified. To show you how much overcrowded a ship could be without people clearly recognizing that it was so, I would quote a paper read to the Epidemiological Society in 1863 by Inspector-General, Sir W. Smart, on epidemic pleuro-pneumonia in the Mediterranean in 1860.

H.M.S. *St. Jean d'Acre* was commissioned in 1859, and by the end of June had reached the Mediterranean. She was considered overcrowded. The ship's company, 850 of them, slept in hammocks on the lower deck, each man having only 28 in. laterally, so that in harbour they formed, as the doctor, Edmonds, said, a compact mass. He notes that on April 8 the temperature of the air below the hammocks was 60° F., while that above was 81° F. So considerable a difference produced strong draughts, which led the people near the ports to close them and thus further impaired

the ventilation. The lower deck is also reported as too damp from over frequent washing. One hundred and fifty-three cases of fever were sent to hospital in her first six months at Malta—continued fever, tending to become “remittent, many assuming a typhoid character.” In fact, exactly like ours of 1886-9. She was sent to Corfu and improved in health, but returning to Malta three months later became, as the old word was, “sickly” again at once; in the succeeding nine months, though the overcrowding of the lower deck was a little reduced, 100 men being taken off it, there were noted these cases:—

Bronchitis	7
Pleuritis	19
Pneumonia	13
Hæmoptysis	7
Phthisis	102
Pleurodynia	90
Cachexia pulmonum (slight cases suggesting phthisis)	117
						285

She was not quite so bad as this suggests, for many of the cases were subsequently decided not to be phthisis, though they resembled it. But six cases did die. It is said that some new class of recruits had been taken in the ship, subjected to medical inspection less rigorous than usual, so I suggest that she was suffering from overcrowding with a considerable phthysical infection larger than usual. Overcrowding was indeed thought possibly a cause, but it was not definitely decided to be the cause. Decaying wood and foul bilges were also held to blame. No one was convinced that overcrowding was the cause. One wonders why all these men were put to sleep on the lower deck when there were two other decks higher up, and with better ventilation, almost untenanted. I expect this was a legacy from the great war and done to leave the guns on those other decks unencumbered in case of a night attack.

Now, to take examples of this remittent fever in the Mediterranean, consider the *Royal Oak* in 1866. In previous years she had suffered much from fever and was now, in June, at Malta, and frequently sending cases of fever to hospital. “On June 18 the lower deck was relieved of 130 men, and from that time a decided improvement set in.” At the same time the ventilation of the lower deck was much improved, and at the end of September the sick list was half that of the previous year. The sick rate, which had always previously increased in the third quarter, now fell off, and that for the Christmas quarter was the lowest hitherto in the whole commission of the ship.

In the eighties fever is frequently reported from the Mediterranean, and the annual reports generally state the number of cases in the ship that suffered most. These below are the big ships:—

Year	Ship	Cases	Percentage of Complement
1882	<i>Monarch</i>	149	30
1882	<i>Superb</i>	196	20
1889	<i>Inflexible</i>	29	6
1884	<i>Superb</i>	124	20 { Specially noted as due to Malta
1884	<i>Alexandra</i>	82	12
1885	<i>Alexandra</i>	88	12
1885	<i>Téméraire</i>	60	10
1886	<i>Alexandra</i>	64	10
1887	<i>Alexandra</i>	70	10
1887	<i>Dreadnought</i>	11	2½
1889	<i>Dreadnought</i>	13	3
1890	<i>Agamemnon</i>	39	10

It is to be presumed that those not mentioned suffered less. Now anyone acquainted with these ships will divide them at once into two groups.

(I) Incidence 30 per cent. to 10 per cent.

	Per cent.
<i>Monarch</i>	30
<i>Superb</i>	20
<i>Alexandra</i>	12 to 10
<i>Téméraire</i>	10

(II) Incidence 10 per cent. to 2½ per cent.

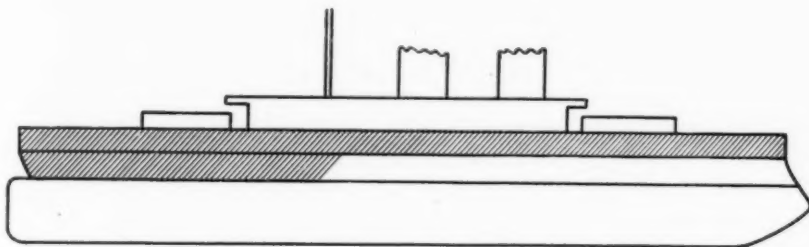
	Per cent.
<i>Inflexible</i>	6
<i>Dreadnought</i>	3 to 2½
<i>Agamemnon</i>	10

No. I, with higher incidence, are called central battery ships. They have hand-worked guns and large complements of men required to work them and smaller space per man. No. II, turret ships with few and power-worked guns, small complements and more room, had small incidence. I will compare two of them in each of which I had the good luck to serve—*Alexandra* and *Dreadnought*.

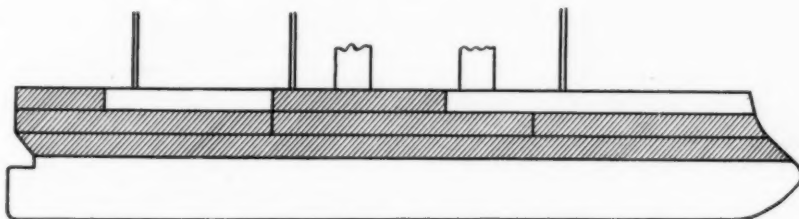
	<i>Alexandra</i>	<i>Dreadnought</i>
Displacement	9,800 tons	10,000 tons
Guns	12	4
Complement	750	450
Tons per man	13	22

The *Alexandra's* men were carried on three decks, so you can easily see those sleeping below must have had less air than the men had in the *Dreadnought*, where very few were on a second deck and those had artificial ventilation. The *Dreadnought* was much drier than the *Alexandra*. When I went to her I took over some clothes that in my cabin in the *Alexandra* were threatened with mildew, which in my lower deck cabin in the *Dreadnought* was killed by the warm dry atmosphere.

We had a very interesting experience in the *Alexandra* in 1886-9. She was kept throughout the commission as clean and dry as possible. Her first lieutenant (now Admiral), F. S. Inglefield, painted the "flats" or lower deck white to show up the dirt. These flats were washed once a week with warm fresh water, carefully dried up immediately after; a deck-cloth was then put down, and this was sent ashore at the end of next week to be washed to save dampness on board. For the first two years she was only cleaned by candle light, and in each year you see she had 10 per cent. of the men attacked with fever. Before the third year



H.M.S. *Dreadnought* (1887).



H.M.S. *Alexandra* (1887).

FIG. 2.

Men are berthed in the shaded parts.

the ship was fitted with electric light, and now not only could a higher standard of cleanliness be attained, but there was less damp and dirt from the burning candles. A boiler was always alight to run the dynamo, and what with the furnace drawing part of its air from the flats and so improving the ventilation, and what with the heating of the ship by conduction from that boiler, the flats became beautifully sweet, clean, and dry; the micro-organisms that used to grow there were removed

or killed, we had only $3\frac{1}{2}$ per cent. of the men this year down with fever, and we are no longer pilloried in the Blue-book.

It is unsafe to over-minutely compare two years with one another, for one does not know quite how much time was spent at Malta, and it was at Malta that the ship suffered from this fever.

It is difficult to exactly assign the importance of humidity in the factors of health; it comes in, in part directly as favouring the growth of micro-organisms, in part indirectly as a measure of insufficient ventilation. In my early days I thought humidity must have great importance as a cause of disease, and in my journal for 1888 I was able to put in a note showing, on two years' observations, that in the *Alexandra*, with 700 men, at Malta in the winter, there was always a case of sore throat at the morning visit if there had been recorded during the previous twenty-four hours a humidity of or above 80 per cent.¹ I thought then that the relation was direct, but the high percentage humidity was certainly an evidence of still weather in which ventilation was at its worst. There was a popular belief in the fleet that the fever was more frequent in still weather.

The experiences of H.M.S. *Trafalgar* in 1891-2 are of great interest. She was a turret ship, but of a new pattern, with a secondary battery, the men, consequently, more shut in than in the *Dreadnought*. The *Trafalgar* was in Malta Harbour near or in dry dock from February 10 to May 5, 1891, and returns 159 cases of continued fever, 85 of remittent, and 24 of enteric from a complement of 600 men. The fever rapidly increased from the beginning of April. The next year she was to spend the same period in dockyard hands, but a definite effort was made that there should be no fever this year. Wind sails were hoisted everywhere possible, every attempt was made to secure cleanliness, dryness, and thorough ventilation of the ship. Also, the men were daily landed by watches and sent to Corradino Parade Ground to air them and give the ship a better chance to dry, and while they were in dry dock this year no fever cases occurred.

But if this fever is only due to overcrowding in a ship why should it

¹ Having obtained statistical evidence of a relation between humidity and the occurrence of cases of disease, I was anxious to find other observations on the subject. For some years I sought in vain. I first found them reported by Lind about 1780, quoted by him from a thesis, "*De Febre Intermittente*," like mine submitted to the University of Edinburgh for the degree of M.D., but about 1750, and by my great-grandfather, Francis Home, already mentioned. He afterwards became a professor in the University and President of the Royal College of Physicians there. He is supposed to be the first person to describe diphtheria under the name of croup. (See Löffler's article in Nuttall and Graham Smith's "*Diphtheria*.")

be called Malta fever? It has, in fact, been reported from elsewhere, as, for example, from the Channel Fleet in 1884, where Inspector-General, then Fleet-Surgeon, G. Maclean, who afterwards came to the *Alexandra*, drew attention to the number of pyrexial cases which came on the list as soon as ships went to sea and had to close their ports and spoil their ventilation, and he notes how closely the cases that then present themselves resemble those known in the Mediterranean as Malta fever. These cases were more frequent at Malta than elsewhere. Why? There are three reasons:—

(1) The biggest ships of the Navy, those with largest complements, were at Malta, and it was generally numbers of cases, not percentages, that were reported and impressed people. The big ships of those days were only in the Channel and Mediterranean, and having only natural ventilation were always worse off for that than the small craft, whose mess decks are quite close to the outer air.

(2) The temperature at Malta was such that the ventilation of these large ships was there at its worst. When air is breathed it is heated, it is also made moister, and by each of these changes its specific gravity is lowered—that is, the expired air is lightened, and these changes make it rise away from us. But it also has its lighter oxygen replaced by heavier carbonic acid, which tends to make it denser. The ultimate specific gravity of the expired air is the resultant of all these changes and varies at different temperatures. Dr. Shaw, Director of the Meteorological Office, in his recent most valuable book, “Air Currents and the Laws of Ventilation,” has calculated 81° F. as the temperature about which these processes balance one another, so that the difference between the specific gravity of inspired and expired air vanishes. As he says: “At such a temperature a crowd would inevitably be poisoned by its own breath.” Now you may remember that 81° F. was the temperature of the air which Dr. Edmonds at Malta found the tars breathing in the *St. Jean d'Acre* during her great epidemic.

(3) The physical conformation of Malta Harbour, so different from that of any other harbour with which I am acquainted, also helped to spoil the ventilation of the ships by keeping off them any possible breeze there might be. The Grand Harbour at Malta is wonderfully convenient for seamen. There is quite deep water close alongside, and I doubt whether ships of the deepest draught are anywhere else in the world so closely packed together, and as the water close alongside the shore is so deep ships are able to lie just under the high fortifications, another most unusual feature of this harbour. These great high walls shut off from

the ships any little breeze there may be which might give us a little change of air on board in a warm night.

A study of the Blue-books will, I think, convince anyone that the incidence of "fever" was greatest on ships lying at the top of Dockyard and French Creeks and in the dry docks. Such was the experience, anyhow, of the *Enterprise* in 1866, *Monarch* and *Condor* in 1882, *Superb*, *Iris* and *Carysfort* in 1884, *Agamemnon* and *Polyphemus* in 1887, and *Trafalgar* in 1891. In 1887 *Carysfort*, doing same work as in 1884, refitting in the summer, escaped fever because, as I think, the cholera

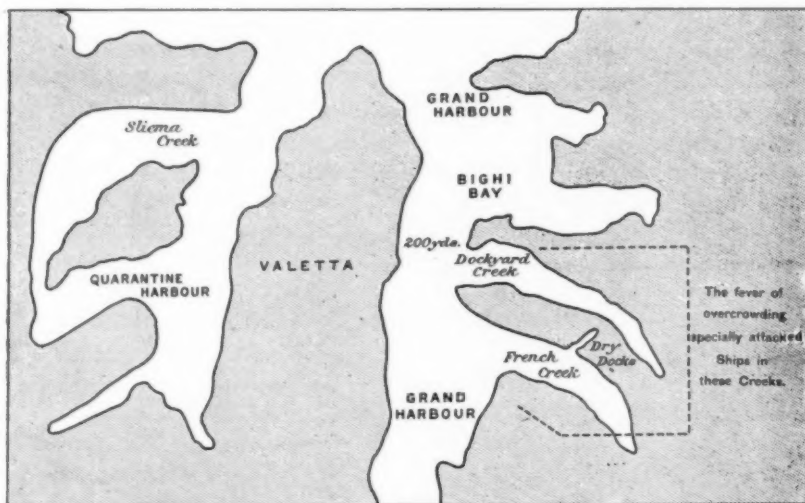


FIG. 3.

The Harbours of Malta.

scare of that year sent her out to Bighi Bay to lie, very inconvenient for her work and costly to the country, a cause probably too of great vexation to her people, but, at the same time, salutary. The case of the *Trafalgar*, in 1892, I have mentioned. Besides, ships suffering from fever in these localities have been relieved by a shift to a more airy billet. Another *Trafalgar*, in 1863, lost the fever when moved from French Creek to Bighi Bay, as did the *Enterprise* in 1886, when moved round to Sliema. A move of this sort seems generally to have done good, perhaps, of course, because it would not be mentioned unless benefit had

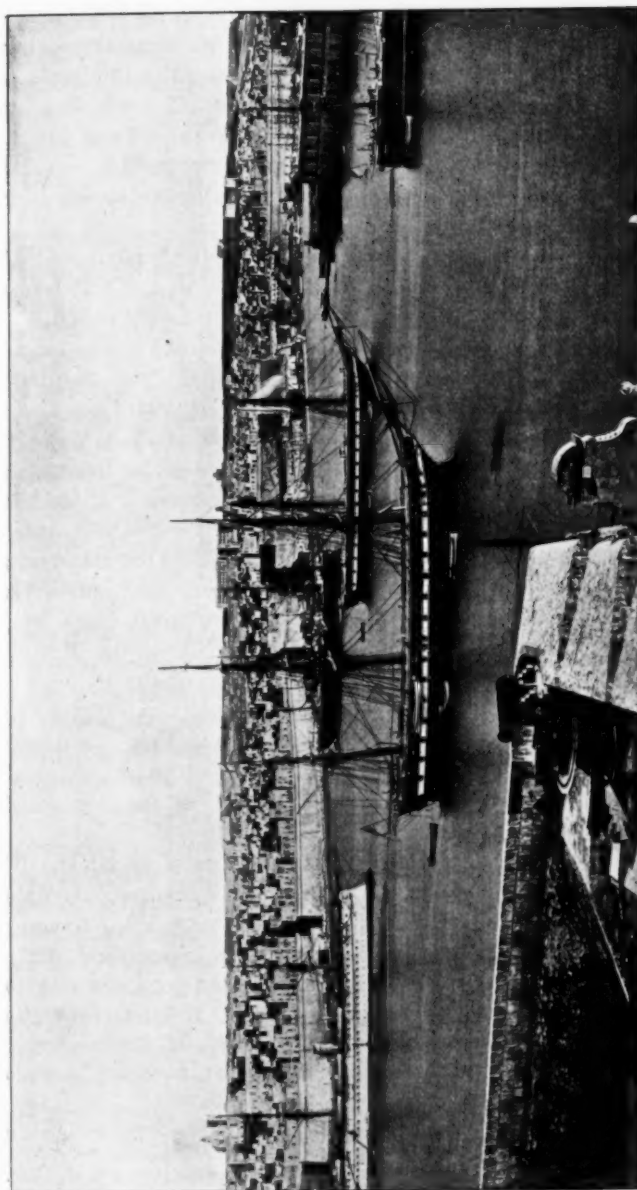


FIG. 4.
French Creek, Malta, about 1880. Note height of fortifications and deep basin in which ships lie.
A, line of fortifications; B, note top-gallant-mast over corner of fortifications.

A-10a

accrued. And a move of this sort seems to do more good than a trip to sea, for the move to sea may be made in bad weather when the ports are closed and ventilation the more diminished. To return to the instructive vicissitudes of the *Alexandra*; in June, 1887, she had much fever, and leaving Malta we cruised up the Italian coast, but we did not begin to lose it, nor the people to thoroughly and clearly improve till we got to Golfo d'Aranci in the N.E. corner of Sardinia, where a strong breeze (force 4 to 5) blew through the ship every afternoon.

What I hope you now recognize with me, I may shortly state: that in Malta, 1886-9, there were two fevers unknown to the text-books—one, Bruce's *Micrococcus melitensis* septicæmia, commonly called Malta fever, attacking people who lived on shore or drank milk, included amongst these last being patients sent to hospital from the Service afloat; the other, this fever of overcrowding on board ship, only attacking the Navy, and attacking it in other places than Malta, but most conspicuously at Malta, because there were there lying in a particularly stuffy harbour, at a specially dangerous temperature, a numerous fleet of our largest ships.¹

Bruce was then concerning himself with his fever only, I specially with ours, while Gipps, working with Bruce at bacteriology, was involved with both. It is distressing to think how much good Gipps and I might have done at that day in earlier prevention if we had not fixed our attention so much on the stagnant filthy water of the harbour and had given more thought to the harbour air. I have taken up your time for too long. In conclusion, I would like to tell you that the management, equipment and construction of ships has so much improved that this fever of overcrowding is not likely to recur in any considerable degree, even in Malta.

The improvement in the health of the Service is manifest. This Society will readily, I suppose, accept phthisis as a measure of the degree of overcrowding. In the *Alexandra*, 1886-9, we had, per 1,000 men per annum, eight cases of phthisis. In the *Galatea*, in Home Fleet, 1901-2, six cases, and in the *Exmouth*, 1905-7, the score had gone down to two. I am sorry I cannot tell you the sick list of the Mediterranean Fleet, but in 1901-2, in Home Fleet, the number generally off duty sick and in hospital was 4 per cent., but a new class of ships then came in, and in

¹ I would here note that when I speak of a disease due to overcrowding I am thinking of an infection, rendered more numerous or more virulent by the increased biological activity of some at present undetermined *noxa*, the increase resulting from the greater heat, moisture and carbonic acid associated with overcrowding.

the Channel Fleet, 1905-7, the *Exmouth*, with flag of Sir Arthur Wilson, being flagship, the average percentage sick and in hospital was only two, and this, in the same climate, halved in four years. The *Exmouth* class had larger cubic space and corticine on the decks. They were the last class without artificial ventilation for the mess decks. So I expect that in new fleets of newer ships you will soon hear of still further marked reductions.

DISCUSSION.

The PRESIDENT (Dr. Newsholme) said that he was sure the members felt themselves greatly indebted to Fleet-Surgeon Home for his paper, in which he had given them the fruit of much valuable experience. A lesson they might all learn from it was the inadvisability of trusting to the categories of disease stated for them in their text-books of medicine, as it was always possible that there might be something beyond them. Fleet-Surgeon Home suggested that what was commonly called Malta fever was really two diseases. One of these, which had almost disappeared, was due to Bruce's *Micrococcus melitensis*, and the other, it was contended in the paper, was due to overcrowding on board ship. Even to landmen this was a matter of great interest, for if they looked carefully enough they ought, on this supposition, to discover these cases of pyrexia on land as well as on sea, assuming that the conditions of temperature, &c., were the same.

Fleet-Surgeon A. G. P. GIPPS asked whether the reader of the paper could make any suggestion as to the pathology of this fever of overcrowding. It seemed to him that it must have some definite condition; it could hardly arise only from bad air due to an overcrowded state. It would be interesting to know whether any cases had been pathologically examined and any growths made from the organs. Fleet-Surgeon Home mentioned the ship *St. Jean d'Acre*, on which a large number of fever cases occurred during her six months' stay at Malta. Afterwards she went to Corfu, where the fever subsided. Yet the overcrowded conditions were the same, for there were an equal number of people on board. Had the reader of the paper any explanation of this? He would also like to ask why he called it "Malta fever" if it was due to overcrowding. He (Fleet-Surgeon Gipps) saw a good deal of Malta fever at the time he mentioned—i.e., 1886-89; and many of the] cases

occurred on shipboard. But so far as the officers' milk was concerned, it was never taken in an unsterilized condition—it was only used in hot tea or coffee—and the men never drank milk at all, so that he was at a loss to know where they contracted it.

Colonel DAVIES said that he might give his experiences of one kind of Malta fever to which Fleet-Surgeon Home had alluded—the Malta fever of the nomenclature, not his fever of overcrowding. He went out to report on the fever in 1905, at a time when, although they knew the actual microbic cause of the disease, they had still to discover wherein it lay, how it got from one place to another, and the manner in which it found entrance into the human body. His method of work was to go as systematically as he could through the various stages of possible infection—air, water, soil, and so on. A lifetime might be spent in carrying out a thorough investigation of this kind, but at that time they had nothing definite to guide them. He endeavoured—he had naturally a predisposition to do so—to fix upon some insanitary condition as the particular cause, or, at least, as the aiding cause. He tried to prove that insanitary barracks must affect the case, but, unfortunately—unfortunately, he meant, from his own point of view—he was unable to prove that lack of sanitation in barracks had any particular effect. Water was practically out of the question. A suggestion had been made by Hughes that it was due to the poisoning produced by drain air, and he endeavoured to find out whether this was the case, but he could discover nothing whatever in that direction. No food appeared to be a likely avenue except milk; and as soon as Horrocks fixed upon milk as the cause orders were immediately given that all milk should be boiled for the troops, and the regulation was carried out first in the Army and then in the Navy. He was unable to find out at that date, 1905, that as regards the garrison as a whole, the boiling of the milk had any influence whatever. Every precaution was taken to ensure that the instructions were carried out, but apparently without effect in stopping the disease. One case, however, was overlooked. It was the case of a rather insanitary barracks, where some extra incidence of the fever might have been expected. On the contrary these troops suffered comparatively little from fever, although there they did not boil the milk; whereas in the other barracks the instructions that the milk should be boiled were obeyed. He (Colonel Davies) did not think at that time that the milk was the predominant cause so far as the troops were concerned. The figures in the case of young infants were worked out very thoroughly—he saw nearly all the British mothers and babies in the island—and the results were about two to one in favour of the milk theory so far as the children were concerned. It was thought possible that, as in the case of malaria, the bite of an infected insect was the cause, that proliferation occurred in the mosquito, and so the germs were carried to the blood. This theory was eventually found not to explain it, and, of course, it could not be

entertained now. To-day everyone knew that the milk was, if not the entire cause, certainly the predominant cause of the malady; and all one need do was to refuse fresh milk of any kind derived from the goat and even from the cow; for, from the dairyman's point of view, the cow was very often regarded as a sort of goat. This fever had practically been abolished amongst the troops in Malta, by the abolition of all milk, except tinned milk only. The history of the investigations showed the good that could be done by pathological research. He was not ashamed, however, to confess his early mistaken impressions, because, after all, the questions of overcrowding, drain air, and other insanitary conditions in relation to a particular disease had to be settled, and even work which was based upon a mistaken hypothesis might prove to be of some use eventually.

Surgeon-General G. J. H. EVATT said that he had never had the pleasure to serve in Malta, but he thought Fleet-Surgeon Home had given them a very important general lesson in the question of the hygiene of masses of men. If they took the acreage of a harbour and treated each of these ships as a lodging-house or barracks it would be found that there was a dense population per acre. He (Surgeon-General Evatt) had been for forty years in the Army, but he had learned more about Malta that evening than he had known before. It was a very important station, and it might very likely one day again become a great "place of arms." They had learned a great deal from the paper concerning the risks of overcrowding men, and the population per acre in a crowded harbour of densely-packed ships was a question worth studying. Barracks were now being built in various parts for sailors, and although they had greatly benefited, he was sorry to see that down at Portsmouth the men were being sent back to live on the ships more than was expected. He was greatly indebted to the Fleet-Surgeon for his interesting paper.

Colonel J. LANE NOTTER said that he served in Malta in 1871-72, when all Malta fever was classified as a sort of typhoid or malarial fever. In those years it was extremely prevalent, and coming out from England, where the theory was held that milk was the common cause for the dissemination of typhoid fever, he took the precaution of only using tinned milk, with the result that during the years he was there neither himself nor any member of his household suffered from Malta fever. The curious thing was that the restrictions with regard to milk were taken as a precaution against typhoid fever and not against Malta fever as it was now understood. He well knew the cases Fleet-Surgeon Home referred to in Malta, for he had had charge of the forts there, and the relative humidity of the climate and other conditions of life in the island were all factors affecting the health of the troops serving in that place.

Colonel MACPHERSON said that Fleet-Surgeon Home had told them mainly one thing—a fact which perhaps they already well understood—that over-

crowding was the cause of ill-health, but so far as the Army service was concerned as compared with the Navy they were very largely at the mercy of the Navy with regard to the accommodation and ventilation of the transports that conveyed soldiers to India and the Colonies. He (Colonel Macpherson) had had considerable experience of those transports, and had investigated the incidence of disease with a view to increasing the cubic space and improving the methods of ventilation. It was found that troops, even healthy troops, conveyed on those transports suffered much more from diseases of the respiratory system, and especially from pneumonia, than troops ashore on any station at home or abroad. It was extremely difficult to convince the authorities that this was the case, and he particularly desired to ask Fleet-Surgeon Home what had been done to improve ventilation in recent years. Up to the time of his last voyage on a transport, from Southampton to Hong Kong in 1904, the ventilation, so far as the artificial ventilation was concerned, consisted of what was called the air-exhaust system. Air was exhausted from the decks by means of a steam jet, and air was introduced between decks to take its place from any source, not necessarily from the open air. It was not introduced from outside the ship, but from somewhere inside the ship. He was told, however, that air could be introduced from outside by reversing the jet, and experiments were made to test this. The candles flickered certainly at the ventilation inlet nearest the propelling force, but at those furthest away they did not flicker at all. This system of ventilation by propulsion by means of a steam jet was apparently of no use. Then it was suggested that electric ventilation should be employed, and electric fans that revolved like paddle-wheels and simply circulated air were introduced. They were not of a construction that ensured fresh air being introduced in a steady stream from outside, but that was the system employed in the hospital of the transport ship in 1904. With regard to this question of the ventilation of the decks of the ships, he had the advantage last year of making a voyage in one of the transports of the United States Army, where they had an excellent system of ventilation by propulsion fans. They had large deck-houses, with special engines for working the electric fans that propelled the air from outside and flooded the between decks with fresh air through air-tubes. It was exactly similar as regards distribution of fresh air to the steam-exhaust system of the British transports, except that in the American system there was a regular current of air, and the decks were absolutely fresh. He would like to know from Fleet-Surgeon Home whether some more scientific method of ventilation had been introduced in recent years.

Professor W. J. R. SIMPSON said that he had only once been in Malta—and that was in quarantine—so that he had little experience with regard to the matter upon which Fleet-Surgeon Home had given them such an interesting paper. He thought that the main interest of the paper lay in the differentiation that he had made between the species of fevers. Of course, it was

not a differentiation such as one would like to see nowadays with regard to pathological investigations and so on, but he thought that he had brought out the fact very well that there was certainly a disease besides the typhoid fever and Malta fever which might possibly affect the ships under particular conditions. He (Professor Simpson) remembered listening to a paper in 1894 by Colonel Crombie, I.M.S., in Calcutta, in which he gave reasons for believing that there was a large number of unclassified fevers quite apart from the malarial fever. Since that time many of these unclassified fevers had been differentiated. Other fevers undoubtedly had to be examined, and they must not be content with the categories they had at present. He quite agreed with Fleet-Surgeon Home that a great deal could be learned from the older books. In looking into the old histories of plague, and only recently in West Africa examining some of the medical records which went back to 1820, he had come across some remarkable notes with regard to diseases. The writers had a good grasp of the conditions that existed at those times, and they were able from the clinical side to differentiate the diseases. With regard to the fever of overcrowding, he thought that with a higher temperature above the hammocks, such as 81° F., and overcrowding, the conditions were such that it was just possible that the cells of the body might from defective metabolism secrete toxins and produce poisonous effects resulting in the symptoms mentioned. The Black Hole of Calcutta might also be cited as an instance of overcrowding in addition to suffocation, and in that case there followed diseases with fever and symptoms of poisoning which were attributable directly to this overcrowded state. In the Navy there might very well be conditions producing such diseases. In Captain Cook's "Voyage Round the World" he showed how a system of good ventilation aboard had got rid of diseases which in other ships had been produced by overcrowding, and, indeed, it seemed as though thorough and proper ventilation would be a safeguard.

Sir SHIRLEY MURPHY said that he would be glad if Fleet-Surgeon Home could say whether influenza was manifesting prevalence in the localities and period to which he referred. He would not say that the whole of Fleet-Surgeon Home's difficulty would be overcome by the acceptance of the influenza theory, but this theory might account for some of the phenomena he described. He (Sir Shirley Murphy) was particularly interested in the statement with regard to the development of this disease in overcrowded conditions. They could see what influenza did in this country in schools and institutions, and wherever there were a great number of people gathered together. In the case of aggregation of people a type of disease is sometimes met with so aberrant as to give an impression that some other malady than influenza is in question. A scapegoat for Malta fever had, indeed, been found, but he supposed that whatever use they might make of the goat they could not put upon it the responsibility for all the evils Malta suffered from, and there might be

cases in regard to which they could not accept the explanation that goat's milk was the cause. He thought that, seeing the erratic and uncertain manner in which influenza behaved in this country, and presumably also abroad, it would be well to take into account the possible responsibility of this disease for the phenomena which Fleet-Surgeon Home had been considering.

Dr. W. H. HAMER said that he had copied some remarks from a paper which Fleet-Surgeon Gipps read before the Epidemiological Society some nineteen years ago.¹ The author said, speaking of micro-organisms in general and of the *Micrococcus melitensis* in particular, that (at that time) he inclined to the belief that the organisms varied in many ways according to the condition of the soil in which they became implanted; "in fact," he said, "their presence and diversity may be more effect than cause." He further said that he was "not altogether inclined to attribute this fever to the presence of these peculiar organisms, but rather to associate them as the result of some specific poison which, acting as a ferment, induces these peculiar forms of germs. That they are intimately connected," he added "(post or propter), with this disease there is no doubt." He (Dr. Hamer) thought it would be interesting to learn whether Fleet-Surgeon Gipps still entertained these heretical views. Sir Shirley Murphy's suggestion that possibly influenza had something to do with the fevers of Malta was a very interesting one. Malta, from an epidemiological point of view, stands where two empires meet. It is situated at the junction of the empire of influenza on the north and west and that of dengue on the south and east. The waves of dengue travelling west and north break in the Mediterranean, and exhibit marked inability to penetrate into Europe. The "mist-born" influenza, on the other hand, showed comparatively little capacity for spreading into the sunnier tropics. It was a very remarkable thing that in Malta, where one would expect to find a good deal of confusion between these two maladies, one heard, save at times of pandemic prevalence, little or nothing about either of them. It was a striking fact that, in the classical work of Surgeon-Captain Hughes on "Undulant Fever," while some space was devoted to the question of diagnosis, there was no reference made in this connexion to either dengue or influenza.

Dr. FARRAR said that he desired to draw attention to one aspect only of the paper. Fleet-Surgeon Home had assumed, what all of them as sanitarians had taken for granted, that the conditions of overcrowding and bad air were unhealthy, and he had gone perhaps rather too far in the direction of connecting this particular variety of disease—if it were a true variety—with the conditions of overcrowding. He had rather assumed this connexion than actually proved it. A paper had lately been issued from the Home Office on

¹ "On Malta Fever," *Trans. Epidem. Soc. Lond.*, (1889-90), ix, p. 76.

the ventilation of factories and the conditions of combined moisture and heat in workrooms, and a great many statements, largely from German sources, had been collected to show that overcrowding and bad conditions of air were after all not so unhealthy as had been commonly supposed. It was stated that the emanations from the human body, which they had been accustomed to associate with conditions of ill-health, could not be considered unhealthy, however disagreeable they might be on other grounds. The question therefore was whether after all overcrowding was, *per se*, an unhealthy condition. That any particular *noxa* would breed better under conditions of overcrowding they would not deny, but their whole position in regard to the effects of overcrowding needed, in the light of this recent paper, to be reconsidered.

Fleet-Surgeon HOME thanked the members of the Section for the kind reception they had given to his paper, and he related the particulars of what he described as the worst epidemic of fever that from overcrowding had occurred in the Navy in his time. It was in 1883, in a ship which had 431 men on her lower deck when she would usually have had only 220. Moreover, owing to her tendency to roll, the scuttles through which outside air might enter her lower deck were kept closed. At Colombo a number of the men fell ill with a fever, and until the vessel reached Hong Kong there was a continuous series of cases. The fever was said to resemble typhus. It appeared to be quite a distinct fever and was very prolonged; the average duration was 145 days. Severe rheumatic pains followed it—the only diagnostic they had for Bruce's Malta fever in those early days. He had not known of an epidemic so severe since that time, but, of course, they did not now take such long slow passages as they did then. Overcrowding, as he understood it, was a function of the cubic space and the ventilation of that space. He could not give information about the pathology of the malady; in those days they had not the means of research they now possessed. The Black Hole of Calcutta furnished an historic example of overcrowding, and he was interested to learn that such consequences followed as stated by Professor Simpson. He agreed with that speaker as to the value of the older books. A very good book on West Africa was written by Lind, 1777, entitled "An Essay on Diseases incidental to Europeans resident in Hot Climates." Lind mentioned that in malarial regions it was a good thing to have a fire inside the hut that the smoke might keep away the fever; the effect of the smoke would be to drive off the mosquitoes. As to ventilation, at the present time they had artificial ventilation, fans for introducing air into ships. The fans they used were not flat but circular. The efficiency of a fan was mainly in the periphery, and in that respect this scirocco fan was admirable. As to the *St. Jean d'Acre*, it must be remembered that at Malta she was lying in a close harbour, while at Corfu she had a wide, open harbour, and was lying about a mile from the shore. He was not responsible for the name of "Malta fever." The reason

why he did not say anything about influenza was because influenza was separately mentioned and discriminated in the returns. When he was in the Mediterranean in 1889 there was no question of influenza. It was not until after the great pandemic outburst of influenza in 1889 that in the issue of the Blue-book for 1890 cases of influenza were found recorded in Mauritius and elsewhere in the tropics. Influenza died out very quickly in the tropics, the poison perishing at short distances, and people in the tropics lived practically in the open air.

Epidemiological Section.

April 23, 1909.

Dr. A. NEWSHOLME, President of the Section, in the Chair.

The Ætiology of Enteric Fever in Belfast in relation to Water Supply, Sanitary Circumstances, and Shellfish.

By L. W. DARRA MAIR, M.D.

At the beginning of 1907 the Local Government Board for Ireland appointed a Commission to inquire into the public health and the sanitary administration of the City of Belfast. This Commission,¹ on which I was called upon to serve, held many public sittings, in the course of which numerous interesting questions and problems in relation to public health had to be considered. One of these problems, of surpassing interest from the epidemiological point of view, was that concerned with the incidence of enteric fever in Belfast.

Belfast has suffered very heavily from this disease; indeed, its experience in this respect has been unique among the cities and towns of the British Isles. During the existence of the Commission I collated the available facts and data, and a detailed report on them appeared as an addendum to the Report² of the Commission. In response to suggestions which have been made to me, I propose, in this paper, to lay before the Section an account of the essential features of this Belfast enteric fever problem, and to indicate whither epidemiological study of them leads.

¹ The Chairman of the Commission was Colonel T. W. Harding, D.L., ex-Lord Mayor of Leeds, and member of the Royal Commission on Sewage Disposal; and the other members were Dr. A. K. Chalmers, Medical Officer of Health of Glasgow; P. C. Cowan, Esq., Chief Engineering Inspector of the Local Government Board for Ireland; Surgeon-Colonel Flinn, Medical Inspector of the same Board; and myself.

² *Report of the Belfast Health Commission to the Local Government Board, Ireland, Lond., 1908, [Cd. 4128].*

The excessive incidence of enteric fever in Belfast may be seen from the following table. It shows the Belfast record, in comparison with that of the principal towns of the United Kingdom, and also with that of towns in England and Wales, which have been more or less conspicuous for excessive mortality from this disease.

TABLE I.—SHOWING, FOR THE DECENNIA 1881-90 AND 1891-1900, AND FOR THE FIVE YEARS 1901-5, MEAN ANNUAL DEATH-RATES PER 1,000 LIVING FROM ENTERIC AND SIMPLE CONTINUED FEVERS IN BELFAST, DUBLIN AND CORK, AND FROM "FEVER" (INCLUDING TYPHUS) IN CERTAIN OTHER TOWNS.

	1881-90	1891-1900	1901-05
<i>Belfast</i>	·51	·86	·57
Dublin	·50	·48	·25
Cork	·27	·19	·09
Liverpool	·50	·33	·21
Manchester	·28	·22	·13
Leeds	·33	·20	·14
Sheffield	·24	·29	·15
Bristol	·17	·11	·09
Birmingham	·16	·20	·14
Glasgow (City)	·27	·22	·15
Grimsby	·25	·42	·35
Rhondda	·49	·39	·30
Great Yarmouth	·44	·41	·14
St. Helen's	·45	·45	·20
Salford	·42	·37	·25
Preston	·49	·33	·23
Sunderland	·35	·53	·22
Middlesbrough	·40	·38	·24
Nottingham	·34	·29	·20
Portsmouth	·52	·27	·18

The next table sets out the fever death-rate per 1,000 in Belfast for each year since 1872, the first year in which the mortality from this disease was separately recorded in Ireland, and it shows that the excessive mortality from fever has not been confined to one small period, but has extended over very many years. It was at a high level in the seventies; and, although it descended to almost its lowest level in the early and middle eighties, it afterwards ascended to and remained at its former level, with some fluctuations, until the end of the nineties, when, during a period of five years, 1897-1901, it reached a level higher than it had ever reached before. In 1898 the death-rate from this disease was 2 per 1,000, while in 1897 and 1901 it was about 1 per 1,000, or a little more. Since 1901 the mortality has diminished almost uniformly year by year until in 1907 it reached the comparatively low level of the eighties. In 1908 a still further diminution took place,

and the death-rate was 0·15 per 1,000, or 33 per cent. less than in 1907. These death-rates, year by year, are shown in diagrammatic form in fig. 1 (*see* p. 190).

TABLE II.—SHOWING THE ANNUAL NUMBER OF DEATHS REGISTERED, WITH DEATH-RATES PER 1,000 LIVING, FROM ENTERIC FEVER AND FROM SIMPLE CONTINUED FEVER IN THE BELFAST REGISTRATION DISTRICT AND IN THE CITY OF BELFAST.

	REGISTRATION DISTRICT		CITY	
	Deaths	Death-rate	Deaths	Death-rate
1872	164	·79	—	—
1873	146	·69	—	—
1874	146	·68	—	—
1875	126	·58	—	—
1876	122	·55	—	—
1877	137	·61	—	—
1878	145	·63	—	—
1879	144	·62	—	—
1880	166	·70	—	—
1881	111	·46	94	·45
1882	194	·51	82	·38
1883	93	·37	81	·37
1884	71	·28	63	·28
1885	72	·28	75	·33
1886	117	·44	121	·52
1887	106	·39	115	·49
1888	109	·40	111	·46
1889	245	·87	241	·98
1890	193	·67	193	·77
1891	158	·54	160	·62
1892	119	·40	134	·51
1893	133	·43	132	·49
1894	169	·54	166	·60
1895	199	·62	213	·74
1896	164	·50	155	·53
1897	402	1·19	370	1·23
1898	664	1·93	662	2·03
1899	286	·81	273	·82
1900	278	·77	269	·78
1901	372	1·00	367	1·04
1902	176	·46	181	·50
1903	151	·39	154	·42
1904	122	·31	119	·32
1905	141	·35	134	·35
1906	104	·25	99	·25
1907	—	—	84	·22
1908	—	—	59	·15

There are certain discrepancies in the two sets of figures in this table—sometimes, for instance, the number of deaths in the city is greater than that in the registration district. These are probably due to the absence of the Registrar-General's scrutiny in the former case, and partly to the fact that the city records are for periods of fifty-two or fifty-three weeks, while the records of the registration district are for calendar years.

When the Commission commenced work in Belfast there were two schools of thought in connexion with this problem. One, led by the Water Commissioners—an independent authority, elected by the usual municipal franchise—claimed that Belfast fever was due principally

to the alleged insanitary condition of the city; the other, and the prevailing one, led by the Corporation of Belfast, claimed that this excessive fever was due to alleged pollution of the public water supply.¹

I may safely say that, at the outset, I approached this problem with the expectation that the water supply would be found to blame, at any rate for the great excess of fever during the critical period 1897-1901, and this mainly on account of its magnitude. Attention was naturally directed in the first place to elucidating the causation of the excess of fever in this critical period. But the Commission had to contend with great difficulties. There were no "spot" maps; the notification of infectious disease had not commenced in Belfast until 1897; the ages and occupations of those attacked by, or even of those who had succumbed to, enteric fever were unknown. The system familiar in this

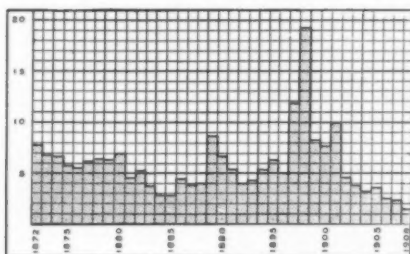


FIG. 1.

Diagram showing annual death-rate from enteric and simple continued fever in Belfast Registration District since 1872.

country by which particulars of every death as furnished to the registrars of deaths are regularly supplied to the public-health authorities does not prevail in Ireland, and the only information the Belfast Corporation possessed, in this respect, were the tabulated figures published in the Registrar-General's official reports. Lastly, there was difficulty as to

¹ On the eve of our first sittings the Corporation had advertised on large posters that a micro-organism indistinguishable from the "typhoid bacillus" had been found by Professor Symmers, of Queen's College, Belfast, in water obtained from two service taps in the town. Very soon, however, it was ascertained that the bacillus in question had characteristics which clearly distinguished it from the typhoid organism. The organism in question has not been definitely identified, so far as I know, but it was stated by Professor Symmers, Dr. Houston, Dr. Gordon, and Professor Frankland, to resemble the *Bacillus fecalis alkaligenes* of Petruschki. Recently it has been found to be agglutinated by blood-serum from cases of cerebro-spinal meningitis.

cases of "continued fever," of which a great number had been notified every year, and which had not been taken into account in previous examinations of Belfast fever. Ultimately, however, "spot" maps for the most important years were obtained, and returns were secured showing the number of cases, both of enteric fever and of continued fever, notified in each week from the beginning of 1897 to the end of 1906, and tabulated according to the sanitary divisions of the city, fourteen in number, in which they had occurred.

In the meantime we had been furnished with reports by Professor Lorrain Smith, F.R.S., who had been consulted by the Belfast Corporation when fever was at its height in 1898, and who had come to the conclusion that fever had been mainly due to the water supply.

The water supply at this time was derived from two separate upland gathering grounds—one at Woodburn, some twelve miles to the north-east of the city; the other at Stoneyford, some fifteen miles to the west of the city. It was the latter source that Professor Lorrain Smith impugned in his reports. Solitary cases of enteric fever had occurred from time to time among the inhabitants of farms on the gathering ground of this supply; there had been some coincidence or rather sequence in time between the occurrence of some of these cases¹ and increase of fever in Belfast; and an area of the city served by this supply had suffered very heavily from fever. Above all, he had pointed out that fever in Belfast had commenced to increase markedly in the year 1889, which he stated was the year immediately succeeding the introduction of this Stoneyford supply. This chain of presumption against the water supply appeared to be strengthened in the following way:—

The Water Commissioners had obtained powers to acquire a part of their two gathering grounds with a view of removing the resident population, and also to introduce water from a third and new source, the Mourne Mountains, thirty miles south of Belfast, the gathering grounds of which were entirely free from human habitations. This

¹ The following table is taken from Professor Lorrain Smith's report of 1903:—

RETURN OF HOUSES ON STONEYFORD GATHERING GROUND IN WHICH TYPHOID FEVER HAS OCCURRED SINCE 1897.

Name	Date	No. of cases	Name	Date	No. of cases
P. ...	January, 1897	... 4	M'K. ...	October, 1902	... 1
M'G. ...	August, 1899	... 1	M'K. ...	March, 1903	... 1
Y. ...	July, 1899	... 1	D. ...	" "	... 7
M'C. ...	March, 1900	... 1	T. ...	April, 1903	... 1
L. ...	February, 1901	... 1	H. ...	" "	... 2
M'M. ...	" "	... 1			

new water supply was introduced into the city at the end of 1901, but the improvement of the gathering grounds of the old supplies did not begin to take effect until some years later. It was claimed that the great decrease of fever in Belfast which commenced in 1902 and has continued since, was due to the introduction of this new supply, and that it furnished strong confirmation of the correctness of Professor Lorrain Smith's incrimination of the older water service.

This was briefly the case against the water supply, and it seemed at first that the Commission's task in this respect would be an easy one. A difficulty soon arose, however. It was shown, conclusively and from documentary evidence, that a mistake had been made about the date of the introduction of Stoneyford water; that instead of this date having been the year *before* the increase of fever in 1889, it had been, as a matter of fact, the year *after*, viz., June, 1890. The next difficulty in my mind followed from visits to the gathering grounds. Although inhabited, the population on these gathering grounds was extremely scattered and chiefly remote from the intakes. The Woodburn gathering ground has an area of about eleven and a half square miles, and the population, before any displacement was effected, numbered about 1,000, of which half lived a mile or more from the reservoirs. The area of the Stoneyford gathering ground is nearly eight and a half square miles, and the population numbered at the most 750, of which about 200 lived within a mile of the reservoirs. Moreover, all the water from these sources was filtered through modern filter beds, and had been so filtered since 1892 in the case of Stoneyford, and since 1894 in the case of Woodburn; that is, for some years before the critical period of fever, beginning in 1897.

Above all, the water from these sources has always been subjected to storage in immense reservoirs. In the case of the suspected Stoneyford supply, the capacity of the reservoir is such that water is stored for 300 days or more—about ten months. The reservoirs on the Woodburn system have a capacity equal to about 180 days, or about six months' supply. The inspection of these gathering grounds, and especially of the vast storage reservoirs, made it seem very doubtful whether infective organisms, derived from the gathering grounds, could possibly survive in the water by the time it reached consumers in Belfast. On this point the Commission had some extremely valuable evidence from Professor Frankland, and Dr. Houston and Dr. Gordon, and an account of some of the special investigations made by Dr. Houston and Dr. Gordon has been published with the Commission's report.

Professor Lorrain Smith had, in the first instance, implicated the Belfast water supply in the following manner: Knowing of the occurrence of typhoid fever on the Stoneyford gathering ground, and having failed to find the *Bacillus typhosus* in the Stoneyford water as delivered to consumers, he nevertheless considered that suspicion of this water could be justified by the characters of the *Bacillus coli* which he did find in it. He had ascertained that *Bacilli coli* which had been recovered from the organs of typhoid-fever patients in Belfast were, some of them, pathogenic to animals, and that they were agglutinated by typhoid-fever blood-serum; and he deduced from the latter circumstances that the *Bacilli coli* in question had taken specific part in the disease, or to quote his words, "were directly concerned in the infection of the disease." Then he ascertained that the *Bacilli coli* which he had isolated from the water samples were also agglutinated by typhoid blood-serum, and he deduced in turn that these also "were directly concerned in the infection of the disease." Accordingly, he concluded that the water samples he had examined did, in fact, contain organisms which could give rise to typhoid fever among those consuming the water.

I have already alluded to the mistake about the date of the introduction of Stoneyford water. The greatest weakness, however, in the case against the water supply was that no correspondence between the distribution of fever in Belfast and the distribution of one or other of the two water supplies had been established. Professor Lorrain Smith had shown, indeed, that in part of Belfast supplied by Stoneyford water, incidence of fever had been very great; but he had not explained its very heavy incidence on many parts of the town supplied by the Woodburn water, and still less had he explained a very great difference in the incidence of fever on two widely separated portions of the town, both of which were supplied by the Stoneyford water.

The arrangement of the Belfast water supplies at this period is shown on the spot map for 1898 (fig. 2). Stoneyford was the high-level supply— $3\frac{1}{2}$ million gallons per day—serving a high area on the extreme west of the town, and another high area on the extreme east of the town. Woodburn was the low-level supply—9 million gallons daily—serving the remainder of the town. The populations were, approximately: Woodburn 250,000, and Stoneyford 100,000. Clearly, therefore, if Stoneyford water had been the main agency of fever at this time there should have been at least more or less equal incidence of the disease on the eastern and western portions of the town served by it. As a matter of fact,

however, there had been no such equality. The attack-rates in the western portion of the Stoneyford area of supply, in the three severest years of fever—1897, 1898 and 1901—had been 13, 26 and 20 per 1,000 respectively, while those in the eastern portion had been 0.5, 11 and

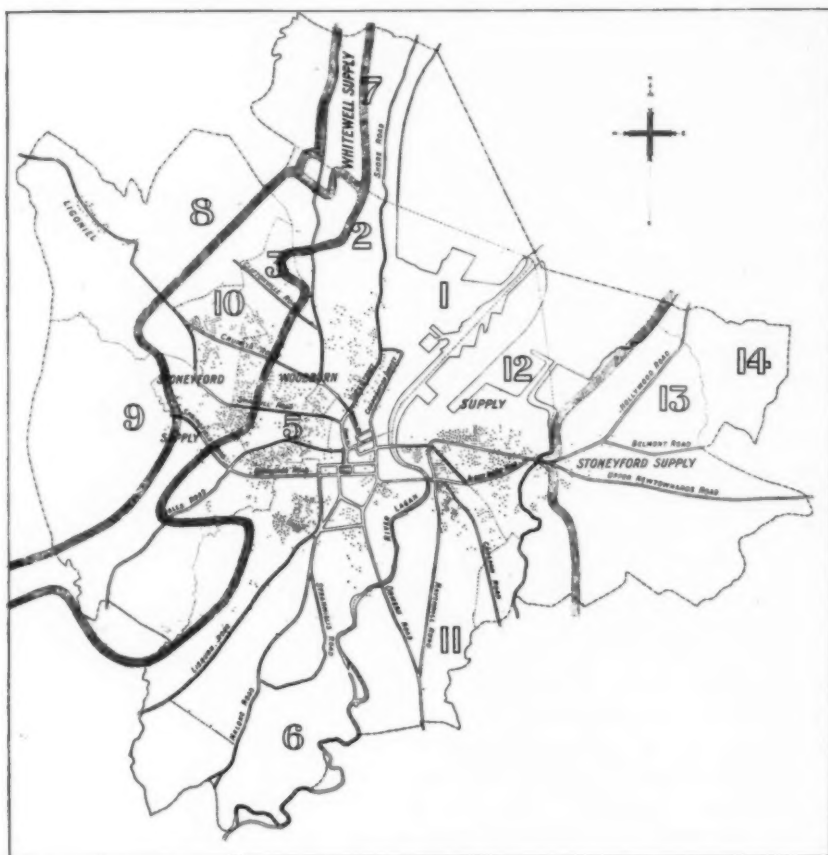


FIG. 2.

Map showing the distribution of enteric fever notified in Belfast during July, August, and September, 1898, the areas of the city then supplied with water from Woodburn and from Stoneyford (area of Whitewell supply also shown), and the boundaries of the dispensary or registration districts of the city. The spots indicate the approximate situations of houses in which enteric fever occurred. The boundaries of the areas of water supply are indicated by broad black lines and of the registration districts by thin dotted lines.

6 per 1,000 respectively. Indeed, while the western portion of the Stoneyford area of supply has been, generally, conspicuous as suffering from fever more than, or as much as, any other part of the city, the eastern portion of the area served by this supply, on the other hand, has almost always suffered less than any other part of the city.¹

Again, the association of the introduction of the third water supply from the Mourne Mountains in 1901 with the decline of fever after that year was found, on examination, not to explain this decline. This new supply displaced altogether the Stoneyford water from the eastern portion of the city, and supplemented, to the extent at times of 50 per cent., the Woodburn supply, but the western part of the city continued to be supplied from Stoneyford alone. The arrangement of the water supplies in Belfast since 1901 is shown in the spot map for 1904 (fig. 3). The attack-rates of these three areas—Mourne, Woodburn and Stoneyford—before and after the introduction of the Mourne water, show that, in the western Stoneyford area, unaffected though it was by the new water, fever had diminished most, and that in the area entirely supplied with the new Mourne water fever had diminished least; while the diminution of fever in the Woodburn + Mourne water area occupied the middle position. Obviously, these attack-rates do not support the proposition that the introduction of the new water caused the decline of fever after 1901, and this, whether the assumption was that Stoneyford had been the polluted supply or that Woodburn had been. The figures are as follows:—

TABLE III.—SHOWING FOR TWO PERIODS OF YEARS, VIZ., 1899-1901 AND 1902-04, IN DISTRICTS NOS. 13, 10, 1, 4, AND 12, OF BELFAST, THE MEAN ANNUAL DEATH-RATE PER 1,000 FROM ENTERIC AND SIMPLE CONTINUED FEVERS.

District	Water Supply	Approximate mean annual attack-rate per 1,000 from fever during		Reduction percentage in	
		1899-1901	1902-04	second period	
No. 13 ...	Stoneyford in first period, Mourne in second period ...	4.7	3.1	...	34
No. 1 ...	Woodburn in both periods, mixed with Mourne in second period ...	15.2	6.3	...	59
No. 4 ...	Ditto ...	7.5	4.7	...	37
No. 12 ...	Ditto ...	8.9	5.5	...	38
No. 10 ...	Stoneyford in both periods ...	13.2	4.4	...	67

When detailed examination of the data supplied to the Commission could be made it became more and more difficult to sustain the case against the water service. The spot maps showed a complete absence

¹ The age and sex constitutions of the populations of these two areas of the town, as disclosed by the 1901 census, did not serve to account for these differences in the incidence of fever; indeed, they were such as to favour heavier incidence on the eastern portion.

of that demarcation of the incidence of fever by the limits of the areas of water supply which was to be expected in a case such as this, in which two distinct water supplies are concerned. Even in the 1898 spot maps, although there was one for each quarter of the year, any such demarcation is totally wanting.

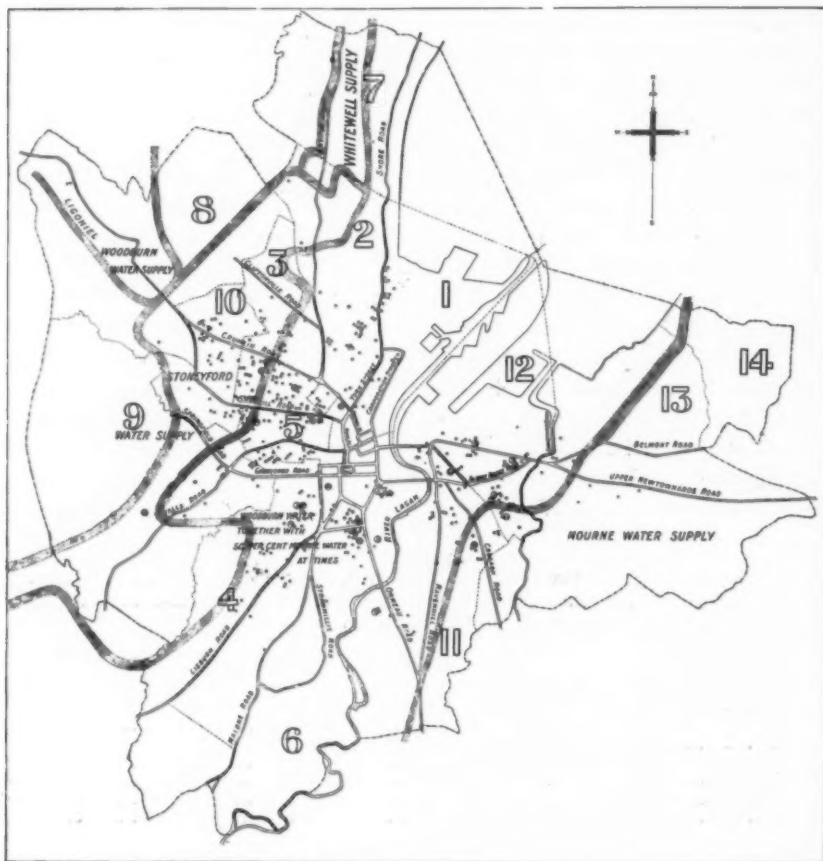


FIG. 3.

Map showing the distribution of enteric fever notified in Belfast during 1904, the areas of the city then (and since) supplied with water from Woodburn, from Stoneyford, and from the Mourne Mountains, and the boundaries of the dispensary or registration districts of the city. The small circles indicate the approximate situations of houses from which single cases of enteric fever were notified, and the large circles those of houses from which multiple cases were notified. The boundaries of the areas of water supply are indicated by broad black lines and of the registration districts by thin dotted lines.

In the absence of spot maps for some of the years, attack-rates per 1,000 were calculated for the different districts of Belfast for each year since 1897. They are shown in the following tables, the water-supply and population of each district being also indicated (*see pp. 198-99*).

Table V shows that in the worst year, 1898, the highest attack-rates were in district No. 4 (27·6) supplied by Woodburn, in No. 10 (26·5) supplied by Stoneyford, and in No. 12 (24·6), and No. 1 (22·7), both supplied by Woodburn; whilst the lowest attack-rates were (exclusive of the very small districts) in No. 13 (11·2) supplied by Stoneyford, and in No. 2 (12·7) supplied mostly by Woodburn. In 1901, another year of exceptional fever, the highest rates were in No. 10 (19·9), Stoneyford, and in No. 1 (18·7) Woodburn; and the lowest were in No. 11 (5·3), Woodburn, and in No. 13 (5·9) Stoneyford. Clearly, this table does not lend itself—any more than the spot maps do—to the belief that there had been any relation between the distribution of either water supply and the distribution of fever in Belfast.

But this table and the spot maps did not altogether exclude a possibility of some correspondence between distribution of water and disease, obscured by a mass of cases due to other causes. Attack-rates were therefore calculated for each of the fourteen districts into which Belfast is divided, not only for each year, but also for every month of each year covered by the notification returns; and also attack-rates of every district for each *week* of the three worst years, 1897, 1898 and 1901. The rates thus calculated were plotted out, and some of them are shown in the diagrams reproduced (figs. 4 and 5). The diagrams also show, distinctively, the sources of water supplied to each of the districts concerned.

I calculated and plotted out in a similar manner the attack-rates from fever, week by week, during some of our great water epidemics of enteric fever, those chosen being Maidstone, Worthing and Lincoln. The result, shown in fig. 6, makes it clear that neither Belfast as a whole nor any of the districts most affected has had that sudden and explosive experience of fever which is characteristic of fever caused by water seriously infected at its source.

The diagrams (figs. 4 and 5) of weekly and monthly attack-rates for the Belfast districts show very clearly that fever has exhibited no preference for either area of water supply. In spite of the minuteness of this analysis of the notification returns, there is no evidence that there has been undue incidence of fever either on parts of the city within the Stoneyford area of supply as compared with parts within the Woodburn area of

TABLE IV.—SHOWING THE NUMBER OF NOTIFICATIONS OF ENTERIC FEVER AND OF SIMPLE CONTINUED FEVER RECEIVED IN EACH OF THE YEARS 1897-1906 FROM EACH REGISTRATION DISTRICT IN BELFAST, TOGETHER WITH THE POPULATION OF EACH DISTRICT AT THE 1901 CENSUS, EXCLUSIVE OF CERTAIN INSTITUTIONS.

	Population, 1901	1897	1898	1899	1900	1901	1902	1903	1904	1905	1906	Enteric fever	Simple continued fever	Total
1	14,734	299	334	197	200	276	116	100	62	81	50	662	1,053	1,715
2	46,743 ¹	534	595	297	415	636	214	176	147	202	104	1,882	1,438	3,320
3	47,214	540	772	269	491	637	215	196	129	212	175	2,739	897	3,636
4	37,386 ²	425	1,031	272	238	336	257	161	103	165	147	2,219	922	3,141
5	17,987	172	312	116	100	226	83	67	48	40	36	967	233	1,200
6	48,122	351	754	358	257	389	217	189	202	126	105	1,924	1,024	2,948
7	1,524	4	16	10	13	24	10	6	5	10	7	61	44	105
8	5,060	7	80	39	13	36	17	7	5	13	3	208	12	220
9	18,823 ²	153	357	145	111	129	63	47	38	40	34	978	139	1,117
10	23,064	313	612	186	270	458	126	107	73	140	84	2,037	331	2,368
11	34,502	398	595	146	168	184	148	114	66	47	58	1,554	300	1,854
12	33,588	464	826	200	240	461	234	218	110	85	96	2,150	784	2,934
13	15,006	8	168	55	70	88	74	44	24	39	18	477	111	598
14	240	—	—	—	1	4	—	—	—	—	—	4	1	5
Total	343,693 ^{1,2}	3,607	6,442	2,290	2,587	3,884	1,774	1,432	1,018	1,200	917	17,862	7,289	25,151

¹ Exclusive of barracks, 1,216.² Exclusive of workhouse, 3,595.³ Exclusive of asylum, 746.

TABLE V.—SHOWING FOR EACH REGISTRATION DISTRICT IN BELFAST IN EACH YEAR SINCE 1897 THE PROPORTION OF NOTIFICATIONS OF ENTERIC FEVER AND OF SIMPLE CONTINUED FEVER PER 1,000 OF THE 1901 POPULATION, AS WELL AS THE AREA OR AREAS OF WATER SUPPLY IN WHICH EACH REGISTRATION DISTRICT IS SITUATED.

District	Area of water supply	1897	1898	1899	1900	1901	1902	1903	1904	1905	1906	Annual average
1	Woodburn	20.3	22.7	13.4	13.6	18.7	7.9	6.8	4.2	5.5	3.4	11.6
2	Almost entirely Woodburn ...	11.4	12.7	6.4	8.9	13.6	4.6	3.8	3.1	4.3	2.2	7.1
3	Half Woodburn, half Stoneyford ...	11.4	16.3	5.7	10.4	13.5	4.6	4.2	2.7	4.5	3.7	7.7
4	Almost entirely Woodburn ...	11.4	27.6	7.3	6.4	9.0	6.9	4.3	2.9	4.4	3.9	8.4
5	Almost entirely Woodburn ...	9.9	17.9	6.7	5.8	13.0	4.8	3.9	2.8	2.3	2.1	6.9
6	Woodburn	7.3	15.7	7.4	5.3	8.1	4.5	3.9	4.2	2.6	2.2	6.1
7	Whitewell and Woodburn ...	2.6	10.5	6.6	8.5	15.7	6.6	3.9	3.3	6.6	4.6	6.9
8	Mostly local wells before, and mostly Woodburn after 1900 (Ligoniel) ...	1.4	15.8	7.7	2.6	7.1	3.4	1.4	1.0	2.6	0.6	4.4
9	Woodburn and Stoneyford ...	8.1	19.0	7.7	5.9	6.9	3.3	2.5	2.0	2.1	1.8	5.9
10	Stoneyford	13.5	26.5	8.2	11.7	19.9	5.5	4.6	3.2	6.1	3.6	10.3
11	Woodburn	9.7	16.8	4.2	4.8	5.3	4.3	3.3	1.9	1.4	1.7	5.3
12	Woodburn	13.8	24.6	5.9	7.1	13.7	7.0	6.5	3.3	2.5	2.9	8.7
13	Stoneyford; Mourne since September, 1901 ...	0.5	11.2	3.7	4.7	5.9	4.9	2.9	1.6	2.6	1.2	3.9
14	Stoneyford; Mourne since September, 1901 ...	—	—	—	4.2	16.7	—	—	—	—	—	2.1
	City of Belfast	10.5	18.7	6.7	7.5	11.3	5.2	4.2	3.0	3.5	2.7	7.3

supply, or on parts of the city within the Woodburn area of supply as compared with parts within the Stoneyford area of supply. It is inconceivable, assuming that these two supplies were kept distinct, that if Stoneyford water had been disseminating typhoid fever, there would not

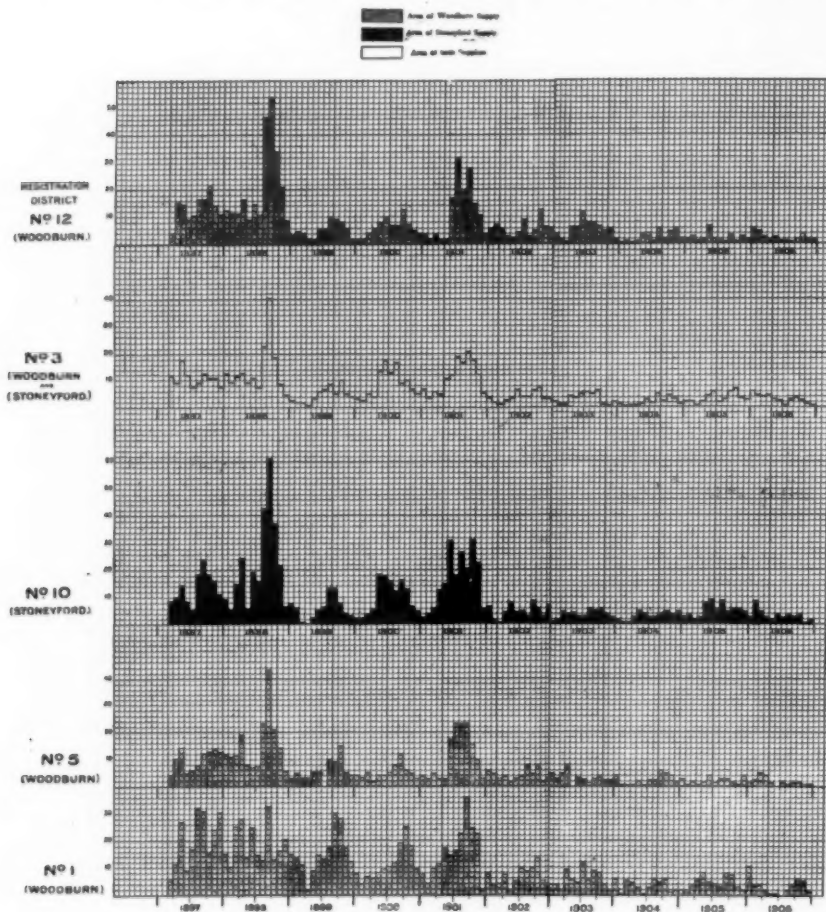


FIG. 4.

Diagram showing for certain of the registration districts of Belfast the approximate monthly attack-rates from fever (enteric and simple continued), per 10,000 of the 1901 population in each case, in each year since the commencement of notification in Belfast. The diagram illustrates the general similarity of the behaviour of fever in various districts of Belfast throughout the period, irrespective of water supply and other local conditions.

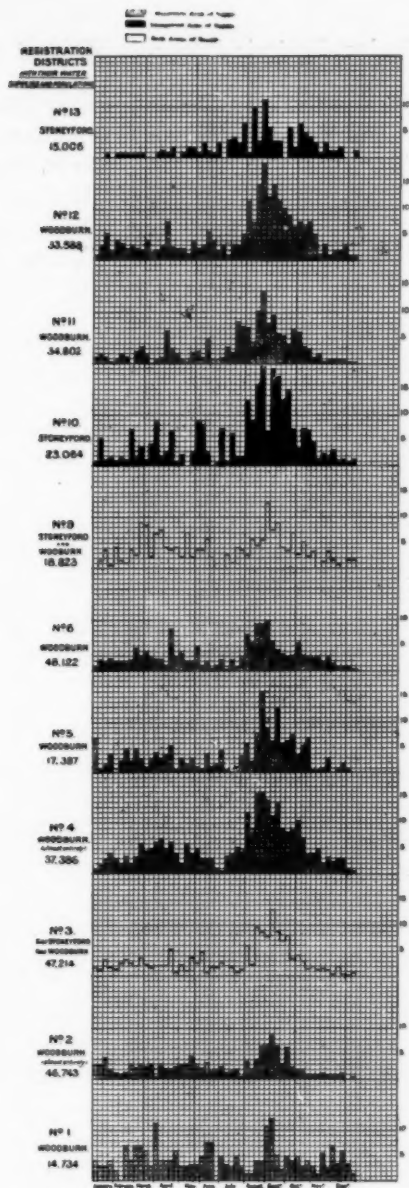


FIG. 5.

Diagram showing the approximate attack-rates from fever (enteric and simple continued) per 10,000 of the population, in each week of 1898, in each of the registration districts of Belfast (except Districts 7, 8, and 14). The diagram illustrates the general similarity of the behaviour of fever in the various registration districts of Belfast, irrespective of water supply and other local conditions.

have been clear indication of it in the diagrams of attack-rates, at any rate at the outset of such an occurrence; and of course the same applies to a supposition that Woodburn water had been at fault.

On the other hand, these diagrams, while they determined the absence of relationship between water supply and fever, brought out for the first time one of the most important and essential facts in

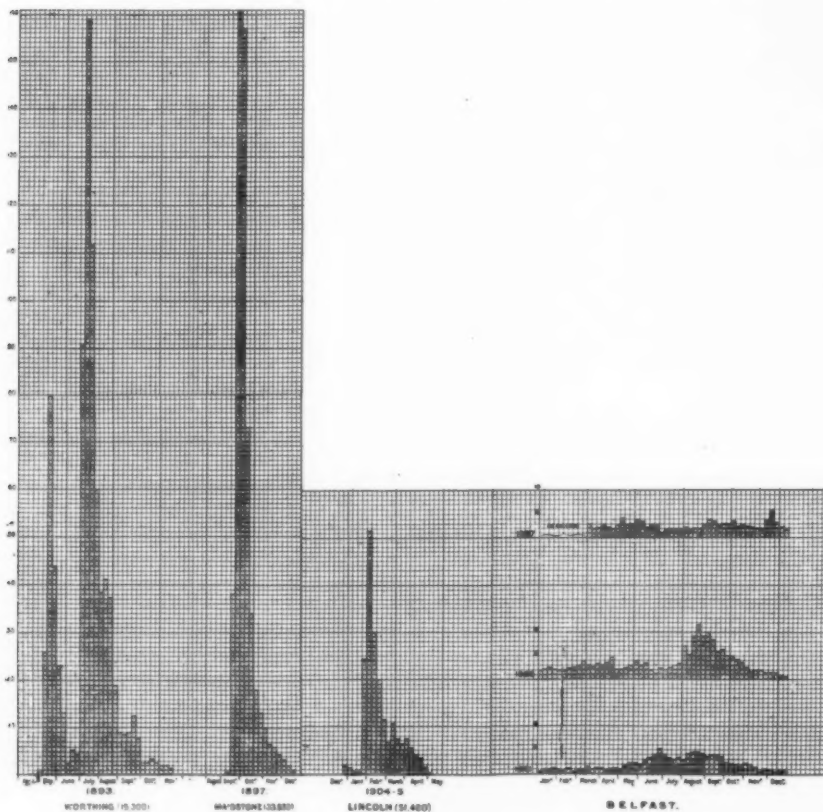


FIG. 6.

Diagram showing the approximate weekly attack-rates from fever in Belfast during the worst fever years, and in Worthing, Maidstone, and Lincoln during epidemics of water-borne fever which occurred in these towns. The diagram represents the proportion per 10,000 of the population in each case, of notifications of enteric fever and simple continued fever received week by week in Belfast during 1897, 1898, and 1901, and of notifications of enteric fever received week by week in Worthing, Maidstone, and Lincoln during parts of 1893, 1897, and 1905 respectively.

connexion with fever in Belfast. They show, firstly, that there was a broad resemblance in each year between the total behaviour of the disease in these districts, even in those which are widely separated from one another and are situated within different areas of water supply; and, secondly, that the modifications which took place within the year in this behaviour were nearly always simultaneous in all districts—that is to say, that when the attack-rate rose in a particular week or month in one district, it likewise rose, more often than not, in the same week or month in the other districts, and similarly a fall, when it occurred, was practically simultaneous in all.

No. 10 district is relatively an elevated district on the west of the town, Nos. 1 and 12 are low-lying districts on the east of the town adjoining the head of Belfast Lough. No. 5 district is the oldest part of the town, Nos. 3 and 10 are relatively modern. Again, Nos. 5 and 12 are made up entirely, and Nos. 1 and 10 almost entirely, of houses of the working class, while No. 3 contains a large proportion of the wealthier classes; and so on. Despite these diversities of position and character, and despite also diversities of water supply and sanitary circumstances, the *behaviour* of fever, as distinct from its amount, has been remarkably similar in all the districts from month to month, and to nearly the same degree from week to week, in almost every year of which records are available.

Consequently, in order to maintain a proposition implicative of the Belfast water supply, it was necessary to explain, not only why incidence of fever on parts of the city within the Woodburn area of supply was equally heavy, or approximately so, as on parts within the Stoneyford area of supply, or vice versa, but also why changes which occurred, week by week, or month by month, in the behaviour of the disease generally affected diverse parts of the city *similarly*, and, above all, *simultaneously*.

In my addendum to the report of the Commission I discussed in detail the significance of these diagrams, and in view of the issues at stake attempted to bring them and other data into conformity with a hypothesis that the one or the other water supply of Belfast had been disseminating fever. In these attempts I failed, but it is unnecessary here to refer to more than the essential points.

The possibility of the mixing of the two supplies is an essential consideration. The waters were certainly not mixed before distribution, and it was physically impossible for the low-level water from Woodburn to pass into the high-level Stoneyford distributing mains; but there

remained the possibility that Stoneyford water might find its way into the Woodburn distributing mains. It was admitted that in emergencies, such as the fracture of a trunk main, Stoneyford water would be turned into the Woodburn distributing mains, but it was denied that this was more than a very rare occurrence, or that when it did occur it was for more than a very brief period. The fact is that this Stoneyford water, before the Mourne water became available in 1901, was exceedingly precious. It was the only high-level supply, and its quantity was strictly limited, so that the managers of the water undertaking had every reason to prevent its use in the Woodburn area of supply, lest the service of water to the higher parts of the town should become disorganized.

Assuming, however, that there was occasional passage of Stoneyford water into the Woodburn mains, could it have been sufficient to explain the manifestation of fever indicated by these diagrams? I think it is clear that it could not. The diagrams show that the fluctuations of fever in the various districts have been so similar, and indeed usually so simultaneous, throughout the period covered by these diagrams, that the only conceivable way to account for this behaviour, from the point of view of Stoneyford water, is to suppose that this water passed into the Woodburn mains not occasionally but constantly throughout a very long period, and also that it so passed in very considerable quantities. In view of the limited amount of this high-level water available, such an assumption has to be dismissed as out of the question.

In regard to the Woodburn supply, although there was no possibility of this water passing into the area usually supplied from Stoneyford, another question had to be considered before suspicion of this supply, from the observed facts, could be disposed of—viz., whether the incidence of fever in the areas supplied from Stoneyford could have been due to the influence of Woodburn water consumed during the day at places of work and business. This might have been a likely explanation of the relatively small incidence of fever on District 13 in the eastern Stoneyford area, but the incidence on District 10 in the western Stoneyford area was altogether too severe to be accounted for in this way. In 1898 the latter district had suffered almost the most, and in 1901 it had suffered actually the most of any district in the city. Any further doubt on this point was cleared up when a peculiarity of this district came to light. District 10 is entirely a working-class district, and, as is characteristic of

Belfast, both men and women are wage-earners. The bulk of the men, however, are engaged in shipbuilding, and the bulk of the women are employed in flax mills; the flax mills being situated in the district itself and supplied by Stoneyford water, while the shipbuilding yards are near the Lough, where the water supply is from Woodburn. In spite of this, the sex-incidence of fever in this district was not abnormal. The attack-rate on males was higher than that on females, but the excess was not such as to be consistent with the supposition that the males only of the district had been subject to infection at their places of work outside the district.

The figures in question are as follows:—(Population of No. 10 district in 1901—males, 10,452; females, 12,612).

		Notifications of enteric and simple continued fever				Attack-rate per 1,000 living			
		1898		1901		1898		1901	
Males	323	...	219	...	31	...	21	
Females	...	289	...	239	...	23	...	19	

Among minor but nevertheless important obstacles to the proposition that Belfast water had been responsible for fever was the fact that the introduction of Stoneyford water in 1890 had made no difference in the incidence of fever (as judged by the mortality returns) on the area to which this water was supplied;¹ that the filtration of the water had been associated with or followed by a great increase of fever instead of a decrease; that public institutions in Belfast had escaped almost entirely; and that, in the case of both supplies, small towns outside Belfast served with Belfast water, and in the case of Carrickfergus, with unfiltered Woodburn water, had also escaped invasion by the disease almost entirely.

¹ At this period registration district No. 3 included the present districts Nos. 3 and 10, and was therefore the district most likely to be affected, one way or the other, by the introduction of Stoneyford water. It appears from the quarterly mortality returns of the Registrar-General that in the four years (*i.e.* 1886-1889) *preceding* the introduction of Stoneyford water the deaths ascribed to enteric and continued fever in this district No. 3 numbered 26, 23, 17, and 37, an annual average of 26; while in the four years (*i.e.* 1890-1893) *succeeding* the introduction of this water they numbered 23, 34, 15, and 23, an annual average of 24. There is nothing in the figures quoted to indicate that there was a marked increase of fever in the newly formed western Stoneyford water area such as was to be looked for on the hypothesis in question in the text. Neither do the figures indicate a decline of fever in this new Stoneyford water area such as would have been expected had Woodburn water been mainly responsible for excess of fever in Belfast. The figures indicate, indeed, that the introduction of Stoneyford water had no influence at all, one way or the other, on the prevalence of fever in Belfast at that time.

The *coup de grâce* to any water explanation of Belfast fever was furnished by the map indicating the location of the working classes and of other classes in the city (fig. 7). This map was prepared by the Belfast Corporation, and personal investigation satisfied me that it is remarkably accurate. The map shows, when compared with the spot maps, that Belfast fever, even when it was at its height, has been limited, almost entirely, to the working classes and that the remaining

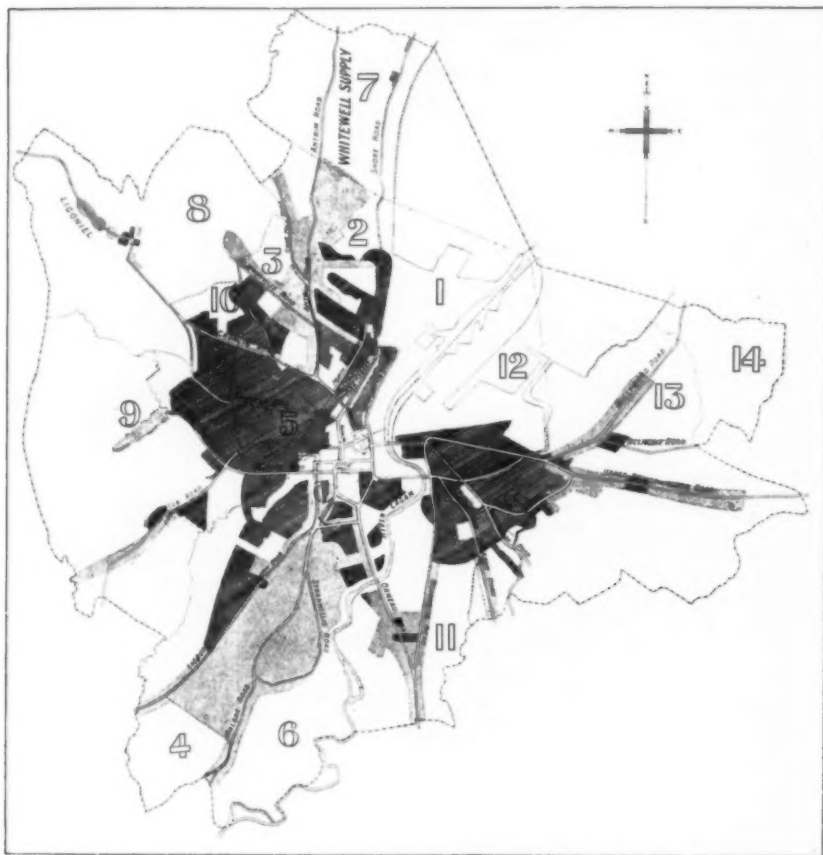


FIG. 7.

Map showing the distribution of the population in Belfast. The dark shading indicates those portions of the city which are inhabited by the working classes, and the light shading indicates the other inhabited portions of the city.

parts of the town have been practically exempt. It is obvious that water infection could not have been limited in this fashion.

Having to set aside the Belfast water supply in connexion with Belfast fever, the possible relation of this fever to insanitary conditions in the city required careful consideration. It did not seem likely, however, that the enormous prevalence of fever during the critical period could be explained in this way, nor, indeed, the history as a whole of the mortality from this disease in Belfast. Research into the fever records of other towns showed that Belfast not only stood alone but stood on a pinnacle as it were (*see* Table I). The nearest approach to Belfast amongst towns of any size in the United Kingdom is Sunderland, where the mortality, throughout the period 1881-1905, has been almost exactly half that of Belfast—0·34 per 1,000 as compared with 0·64 per 1,000. The highest rate in Sunderland during any quinquennium (1891-1895) was 0·64 per 1,000, and in Belfast (1897-1901) 1·18 per 1,000.

Belfast has, no doubt, many insanitary conditions, but in this respect it does not stand alone, still less does it stand on an ignoble pinnacle as in the case of its fever record. In many ways Belfast has solid advantages, as the Commission had opportunity of seeing when visiting other towns in Ireland. It is a new town. The housing, on the whole, is good; the bulk of the houses are occupied each by one family only; rent is low; overcrowding in houses is trifling; every house is self-contained with separate back yard and closet accommodation; there are no common yards; and but few "slums," in the usual sense of the word, exist. Although air space at the rear of houses is frequently small, there is not exceptional crowding of houses on area, one of the most conspicuous features of Belfast being the width of its streets.

On the other hand, Belfast until recently was largely a privy-midden town. Another objectionable feature is that portions of the town are built on ground filled up with ashpit refuse. Those who urged that fever in Belfast could be accounted for by insanitary circumstances relied mainly on the evil effects likely to accrue from the conditions named, and they pointed, in confirmation of this proposition, to the fact that the great decline of fever since 1901 had coincided practically with the conversion of privies into water-closets.

In 1897, the first year of the critical period of fever, 40 per cent. of Belfast houses had privy-middens. In 1899 powers were obtained in a local Act for requiring the conversion of privies into water-closets, and these powers were enforced with such rapidity that by 1902 only

12 per cent. of Belfast houses had privy-middens, and at the present time the conversion into water-closets is almost complete. Clearly, therefore, there has been a coincidence, in point of time, between the decline of fever and the conversion of privies.

On the other hand, this coincidence, although it might help to explain the decline of fever, left unexplained the great increase of fever beginning in 1897. In this connexion, Professor Lorrain Smith, who, in the reports already alluded to, had examined the evidence for and against the relationship between insanitary conditions and fever in Belfast, had carried out a valuable investigation. He had made a detailed sanitary survey of five selected areas in the city, each having a considerable population, ranging from 2,300 to 11,300, so as to bring under review "situations differing from each other as widely as possible." Thus the elevations of the selected areas ranged from 3 ft. to 160 ft.; in some the streets were wide and in others narrow; in some the houses were very old, in others they were quite modern; the proportion of houses provided with privies ranged from 7 per cent. to 61 per cent.; the condition of the drains and sewers varied within wide limits; and, lastly, a special feature of one of the areas was that it had been built "entirely on made-up ground, filled in with road scrapings, ashpit and town refuse."

Professor Lorrain Smith then showed that the incidence of fever on these areas, in 1900, 1901 and 1902, had no apparent relation to their sanitary condition. For example, the area built entirely on made-up ground suffered rather less than the average of the city; another area which proved to be the worst in a sanitary sense—with narrow streets, the largest proportion of privies, and defective sewers and drains—had less fever than other areas better favoured in these respects; and so on. These investigations were robbed of some of their value because continued fever had been left out of account, and because it had not been practicable to allow for differences in the age and sex constitution of the populations concerned. Nevertheless, it was clear that areas which were relatively sanitary and areas which were relatively insanitary had all suffered from excessive fever.

The general distribution of enteric fever, moreover, did not correspond with the distribution of privies, nor has the distribution of the disease been materially changed since the abolition of privies was commenced. The most striking instance of this feature is to be found in Districts 10 and 12, as will appear later. In District 12 privies have always been few, in District 10 they were almost universal prior to 1890;

nevertheless the behaviour and incidence of fever in these two widely separated districts have been remarkably alike, month by month, and almost week by week, throughout the period covered by the notification returns.

The history of Belfast fever, too, in comparison with the general sanitary record of the town, makes it difficult to accept a proposition implicating insanitary conditions in general, as Professor Lorrain Smith demonstrated in his reports. Modern sanitary reform may be said to have seriously commenced in Belfast towards the end of the eighties. The sewerage of the city was then taken in hand. Prior to this, all sewage found its way into the River Lagan and tributary streams, flowing through the city. Before 1892 there had been no public scavenging of privies; a system of free though not very frequent scavenging was then commenced. Building by-laws and drainage regulations were more strictly enforced, and recurring nuisances were more actively dealt with. Conversion of privies into water-closets had, too, been making slow progress since about 1880, so that instead of being almost universal, as they had been then, privies were, by 1897, to be found in only 40 per cent. of the houses.

Pari passu with these sanitary improvements the death-rate from all causes declined in Belfast. From 1871 to 1890 it had remained more or less stationary, in the quinquennial periods, at about 25 per 1,000; but in the three succeeding quinquennia it declined to 24·0, 21·6 and 20·3 per 1,000 respectively. On the other hand, mortality from enteric fever, instead of declining also, remained either stationary at a high level, or actually increased. The figures are shown in the following table:—

TABLE VI.—SHOWING MEAN ANNUAL DEATH-RATES FROM ALL CAUSES, AND FROM ENTERIC AND SIMPLE CONTINUED FEVERS, IN THE BELFAST REGISTRATION DISTRICT DURING CERTAIN PERIODS OF YEARS.

Period	Fever	All causes
1872-1875	70	25·0
1875-1880	64	24·5
1881-1885	40	25·0
1886-1890	60	25·1
1891-1895	50	24·0
1896-1900	97	21·6
1901-1905	50	20·3
1872-1880	67	24·8
1881-1890	50	25·0
1891-1900	76	22·8

There is little in this record to confirm the common doctrine that in the sanitary progress of the latter half of the last century is to be found explanation of the decline of enteric fever. A somewhat closer

examination of the history year by year of Belfast fever brings even less confirmation. This shows (*see* fig. 1) that the fever mortality in Belfast reached its lowest recorded level, with the exception of the last three years, as long ago as in the middle eighties, before any substantial measures of sanitary reform had been initiated, when privy-middens were at their maximum, and when there was no public scavenging; that almost as soon as measures of sanitary reform were commenced fever mortality increased; and that soon after an important section of them had been completed—*i.e.*, the sewerage of the city—the enormous increase of fever during the critical period was witnessed.

A conclusion, therefore, that in "insanitary conditions" lay the explanation of fever in Belfast could not be regarded, on epidemiological grounds, as adequate or as consistent with the observed facts. An additional factor was needed, one that would explain at least the special increase of fever in the critical period, if not also the excessive incidence of other years. It was also necessary that this factor should not conflict with the facts that the fever was limited in the main to the working classes, and that its rise and fall had been similar and almost simultaneous in different sections of the city.

Milk supply could not satisfy these conditions, and when the facts proved to be inconsistent with a water explanation, it remained a puzzle for a time what the required factor could be.

Much had been said about shellfish almost from the time that the Commission commenced its sittings. We had been told that large quantities were obtained from the polluted shores of Belfast Lough; that the principal sources of shellfish were near Greencastle (*see* map of Belfast Lough, fig. 8) within half or three-quarters of a mile of the sewage outfall; that the Belfast population, including even small children, had been large eaters of these shellfish, and especially of cockles and mussels, which were usually eaten raw; that investigations by Professor Lorrain Smith and others had shown that Lough shellfish usually contained micro-organisms of intestinal derivation; that the Belfast Public Health Department had not infrequently suspected that cases of enteric fever had been caused by the consumption of these shellfish; that the Corporation had taken steps since 1902 to warn the public that Lough shellfish were a dangerous article of food, and more recently, by seizures of such shellfish offered for sale, had endeavoured to check their distribution by hawkers in the streets of Belfast; and that in consequence it had become a rare occurrence for shellfish to be hawked for sale in Belfast, instead of the daily, or rather nightly, occurrence throughout the year that it used to be.

It was, of course, taken for granted that, under these circumstances, infected shellfish had caused many attacks of enteric fever in Belfast, especially in years gone by, but it had not occurred to us to regard this agency of fever as more than a minor factor in the matter. Personally, although I was aware that in some towns shellfish had come to be thought of as an important agency of enteric fever, I had been inclined to the view that this aspect of the ætiology of the disease had been somewhat exaggerated.

In searching, however, for local events of outstanding importance corresponding in point of time with the outstanding increase of fever beginning in 1897, it was impossible to overlook the coincidence of the latter with the construction of the great sewerage scheme of Belfast. This coincidence was the more striking because other searches for association between the critical increase of fever and important events in connexion either with the water undertaking or with the general sanitary development of the town had signally failed. It was, indeed, the only apparent point of coincidence in the sanitary history of the town and the increase of fever; but it was not obvious how the two events could have had more than an accidental association.

The sewerage system had involved the construction of many miles of trunk intercepting sewers throughout Belfast, and its object had been not only to provide sorely needed improvements of drainage, but also to put an end to the outgo of sewage into the River Lagan, which was then a stinking river in the heart of the city. In place of this, all the sewage was to be transferred to the head of Belfast Lough, to be discharged into the sea through a wooden outfall sewer, stretching fully a mile from the pumping station along the foreshore (see map of Lough). The object of this long wooden sewer, or "shoot" as it is called, together with a dredged channel beyond it, was to ensure the ultimate discharge of sewage into deep water (Whitehouse Roads); and as the discharge of sewage was limited, by statute, to the first three and a half hours of the ebb tide, it was expected that all the sewage would be carried rapidly away to sea. The new system worked well for a time, but first the "shoot" became choked, and then it broke down altogether, so that sewage was discharged, not at its outlet into deep water, but at intermediate places in very shallow water and on the foreshore. To make matters worse, the volume of sewage to be dealt with grew very rapidly to about 15,000,000 gallons a day, and the discharge could not always be limited to the statutory period. As a result almost the whole of the foreshore at the head of the Lough became covered with a layer of sewage mud to such an extent that parts of this foreshore were appreciably raised, and there was a forward and backward flow of permanently sewage-polluted water at the head of the Lough. It should be added that the tidal currents here are extremely sluggish. At present the

condition of the Lough is a burning local question, and has been so for more than ten years, not only in consequence of the nuisance created by the great expanse of sewage mud exposed at low water, but also in consequence of nuisance resulting from an immensely increased growth of certain seaweeds, which are cast up on the shore in huge decomposing masses.

It is evident from the foregoing description that the increased pollution of the foreshores at the head of the Lough, which the new sewerage system brought in its train, was a matter of very great magnitude; and as shellfish (cockles, mussels, &c.) abounded on these foreshores, it eventually became obvious that if there had been any connexion between the construction of the new sewerage system and the increase of fever in 1897, the shellfish might, by reason of their enormously increased chances of pollution, have brought that connexion about.

The construction of this sewerage system commenced in 1889, in the middle of which year a temporary outfall was arranged near the head of the Lough, but no sewage was discharged through the outfall "shoot" until the end of 1893; and it was not until 1894 or 1895 that all the sewers were connected with this outfall, nor until the end of 1896 that the "shoot" broke down. It then became evident that sewage, instead of passing away into deep water as intended, was being distributed in the shallows and upon the foreshore of the head of the Lough.

There was thus a striking correspondence in point of time between the effective change in the disposal of sewage, the breakdown of the special arrangements for safeguarding the foreshores of the Lough, and the increase of fever in Belfast in the critical period. In contrast to previous futile attempts to find a coincidence between this increase of fever and some event in the water or sanitary history of Belfast there was here no inherent difficulty in a proposition that the change of sewage disposal and the increase of fever had been related, and not merely associated, one with the other, and that shellfish had been the connecting link. In other words, a hypothesis that shellfish from Belfast Lough had been an important factor in Belfast fever made it possible for the first time to explain the critical increase of fever which began in 1897.

On the same hypothesis the limitation of Belfast fever mainly to the working classes became easy of explanation; and also the essential characteristic of Belfast fever, which is demonstrated by figs. 4 and 5. The difficulty in the way of regarding either or both of the water services of the city as responsible for Belfast fever in face of these diagrams arose because they demonstrated that there must have been either one dominant agency of fever in operation throughout the city, or, if more

than one agency, that the several agencies must have derived their infectivity from a source common to them all. The facts disclosed by these diagrams raise no difficulty in the case of shellfish, distributed as they were by hawkers throughout the city day after day, and all obtained from the same grossly polluted source—Belfast Lough.

It has been contended, however, that the characteristic of Belfast fever revealed by these diagrams is not explained by the shellfish hypothesis. The reasoning, as I understand it, on which this contention is based, is that the similar and simultaneous rise and fall of fever in the various divisions of Belfast can only be explained, on this hypothesis, by the further assumption that the different portions of the Belfast community simultaneously developed a desire to increase their consumption of shellfish, and likewise simultaneously diminished their consumption of this article of food. It is claimed that this is a *reductio ad absurdum*, as there is no definite season for shellfish in Belfast, except that there is a greater consumption in the spring and summer than in the winter. The further assumption, however, is not warranted by the facts, and requires, moreover, the premise that the infectivity of every individual shellfish—or at least of every stock of shellfish offered for sale—was always the same from week to week, and from month to month, throughout the year, which is itself a *reductio ad absurdum*.

All that the facts, as disclosed by these diagrams, seem to require for their explanation, on the shellfish hypothesis, is an assumption that the proportion of infective shellfish to the total shellfish offered for sale, though varying within wide limits from time to time, was usually approximately similar in the stocks sold in the different parts of the city. This is a fairly obvious assumption, since on any given day every stock of cockles—the principal shellfish concerned—was gathered from one source and at the same time, *i.e.*, at low water.

The grouping of the most conspicuous fever areas in Belfast follows closely the principal centres affected by the hawkers of shellfish. Perhaps the most interesting instance of this concerned the district known as Ballymacarrett (No. 12) east of the city, and the area known as Shankhill (Nos. 3 and 10) west of the city (*see* figs. 2, 4, and 5). The former is low lying, is supplied with Woodburn water, has always been conspicuous for a relative paucity of privy-middens, and its population is mostly engaged in the shipbuilding industry; the latter, two miles distant, is an elevated area, is supplied mostly with Stoneyford water, was conspicuous in the critical period for a great abundance of privy-middens, and its population is largely engaged in the linen industry. The incidence of fever on these two districts has been usually much about the same, and, as shown by the diagrams, the fluctuation of fever month by month and week by week

has been remarkably similar in both. The districts, though so diverse in character and wide apart, resemble one another in two respects: each has a population almost entirely working class, and each was a prominent centre for the hawking of shellfish. I remember an opinion being expressed to me in Belfast, before the shellfish hypothesis came to be considered, that an explanation which would account for the similar incidence and behaviour of fever on Ballymacarrett and the Shankhill area would probably be found to solve the Belfast fever problem.

An important link, however, in this presumptive evidence was wanting, and, unfortunately, it remains wanting. There was no evidence as to what proportion of fever attacks in the critical years (1897-1901) were associated with the consumption of shellfish in the incubation period, and it was hopeless for the Commission to attempt to supply this omission by inquiries instituted six to ten years after the event. On the other hand it was generally admitted that the consumption of shellfish by the Belfast working classes (by men, women, and especially children) in those days had been very large and that, then, at least, there had been plentiful opportunity for shellfish to operate as an important agency of fever.¹ This was confirmed by many inquiries made of officials of the Corporation, of those who were familiar with the Belfast working classes, and of very many of the working people them-

¹ Dr. T. J. Browne, Medical Inspector of the Local Government Board for Ireland, in his report of 1903 on "Shell-fish Layings on the Irish Coast," states as follows. In the course of this inquiry instances of enteric fever following the consumption of shellfish from suspected sources were brought under my notice. Dr. Whittaker, Medical Superintendent Officer of Health, Belfast, stated that numerous cases of enteric fever had occurred in that city from time to time traceable to the eating of shellfish collected from the sewage-polluted shores of Belfast Lough, and I have received from Dr. Clibborn, Medical Inspector of the District, the following memorandum in support of this statement: "The sale of cockles gathered on the foreshore of Belfast Lough, although not so extensively carried on as formerly, is still of considerable importance; but the quantity gathered by the general public (who come from nearly every part of the town) and carried home for consumption, probably exceeds to a large extent that gathered by those who earn their living by this occupation. As many as fifty to sixty persons have been counted gathering cockles in one day, close to the sewage outfall works, and these people are not so particular as to the cleanness of the shellfish, often picking up those which are highly discoloured and show up on the surface of the slob-land. Many cases of typhoid have occurred in houses where this class of shellfish has been used. There are several persons who make a living by the taking of shellfish from the foreshores of the Lough. . . . The greater part of the mussels, periwinkles, &c., are sent to England and Scotland, the mussels being used principally for bait, but the cockles are disposed of in Belfast. They are hawked in the evenings through the working-class districts, sold to the residents, and in public-houses. Rag-gatherers frequently carry them and exchange them to children for rags and bones. The following are a few instances of recent cases of typhoid which have occurred to persons, who, it is admitted, ate cockles a fortnight or three weeks prior to the onset of the disease. There are many instances where the patients suffering from enteric fever have denied eating shellfish, although the Sanitary Inspector has found the empty cockle-shells in the ashpit."

selves in all parts of the city. It was stated in evidence that the quantity of cockles distributed in the city by regular hawkers was such as would suffice to supply at least 400 people daily. It is to be noted that the largest number of cases of fever notified in any one *week*, when the disease was at its height in 1898, was 404, approximately 15 per cent. of the minimum number of shellfish eaters.

Having found a hypothesis which was consistent with the great increase of Belfast fever in the critical period, the limitation of this fever to the working classes, and its behaviour and distribution in the city, it then became evident that there was much, in other respects, about Belfast fever which could be explained on the same hypothesis. Thus, the generally high level of the mortality from this disease prior to the critical period became intelligible. It was stated in evidence that the shellfish industry for the local market had been a flourishing one in Belfast for at least half a century, while it was obvious that the pollution of shellfish in Belfast Lough must have been very great even before the change of sewage disposal. Not only had all the sewage of Belfast always found its way into the Lough via the River Lagan, but sewage from a number of small towns on the shores of the Lough had also discharged direct into the Lough, over the foreshore, and indeed does so now.

Even the marked decline of mortality from fever in the early eighties seemed accountable on this hypothesis, for it appeared that in 1882 a large reclamation of foreshore was commenced, and that the trade of shellfish gathering had been so much interfered with that the professional gatherers claimed compensation for loss of their means of livelihood. Moreover, the increase of fever in 1889 appears to have been coincident with resumed activity of the shellfish trade, with the construction of an additional local sewer near the shellfish beds at Greencastle, and with the provision of a temporary outfall for Belfast sewage near the head of the Lough, pending the construction of the permanent outfall.

The shellfish hypothesis is also consistent with the diminution of Belfast fever in recent years, for coincident with this diminution there has been a very large reduction in the consumption by the Belfast population of Lough shellfish. The extent of this reduction may be measured by the fact that in the time of the Commission there remained only two or three professional gatherers of shellfish for the local market instead of twenty or thirty as there had been five years previously. The reduction in the consumption of shellfish

commenced about 1902; their pollution then attracted much attention; they were "condemned" by Professor Lorrain Smith and other bacteriologists; people were cautioned by the Belfast Corporation against their use as food; a prominent Jewish citizen, ex-Lord Mayor of Belfast, had pointed out that during the time that fever had prevailed so greatly in Belfast the Jewish community there had entirely escaped, an immunity which he had attributed to the Jewish prohibition of shellfish. After 1905 the Corporation began to take more active measures, involving systematic seizure of stocks of Lough shellfish offered for sale in Belfast and prosecution of the hawkers.

It is possible that the diminution of fever which marked the first two years after 1901 may have preceded somewhat any very great reduction in the consumption of shellfish. The point was difficult to establish with exactitude. The question arises, however, whether this earlier diminution of fever may not have been due, in part, to exhaustion of susceptible material among the population. It is a fact that about this time the Belfast population was not increasing at anything like the same rate as previously;¹ indeed, it is probable that in 1901 and 1902 the population diminished somewhat. With this relative stagnation of the population, the enormous incidence of fever during the critical period of five years—there had been a total of nearly 19,000 cases, or about 5 per cent. of the population—suggests that for a time insusceptibility might have been a not unimportant factor in effecting diminution of fever.

The seasonal incidence of fever in Belfast is important, especially in connexion with this diminution of fever. From tabulated returns of notifications of typhoid fever and simple continued fever received in Belfast week by week during each year from the commencement of notification in 1897 to the end of 1906, a diagram has been prepared showing the mean weekly notifications for the whole of this period, and the percentage departure per week from the mean. In comparison with this is a diagram for London, covering the period 1890-1907, taken from Sir Shirley Murphy's Annual Report for 1907, and one for Manchester, covering the period 1897 to 1906, which has been prepared from data given in Dr. Niven's Annual Report for 1907. These diagrams are shown in fig. 9, and they demonstrate that the Belfast

¹ The number of artisans' new houses erected in Belfast in the years 1897 to 1901 were 941, 1,207, 1,179, 947, and 548 respectively, an average of 964 per year; while in the years 1902 to 1906 the numbers were 195, 151, 130, 75, and 126 respectively, an average of only 135 per year.

seasonal fever curve differs materially from those of London and Manchester, which, on the other hand, have much in common. The Belfast curve has its minimum in the winter months—December to February—and, beginning to rise towards the end of the first quarter, it has a well-marked peak in the second or spring quarter, reaching its main summit in the summer months—July to September. The London and Manchester curves, on the other hand, fall throughout the first quarter, reach their minimum in the second or beginning of the third quarter, and rising in the third quarter do not reach their summit until the beginning or middle of the fourth quarter. In other words, the Belfast curve shows essentially a winter minimum, a spring rise, and a summer maximum; while the London and Manchester curves show a spring or early summer minimum, a summer rise, and an autumn or early winter maximum.

Light seems to be thrown on the Belfast seasonal curve when it is considered from the point of view of shellfish. I have already mentioned that there is not a definite season for shellfish in Belfast or, indeed, in Ireland, I believe, but that the consumption of them, and especially of cockles, is greatly increased in the spring and summer months of the year. The supply of cockles is greater in these months, not only because longer days and more propitious weather render it easier for the usual professional gatherers to make their harvest, but also because with the advent of spring and summer weather the usual gatherers are reinforced by many others, and by people who on pleasant afternoons and evenings gather them for their own consumption. The correspondence between the seasonal fluctuations of Belfast fever and the rise and fall in the consumption of shellfish is, no doubt, considerable.

But the most significant feature of the seasonal fluctuations of fever in Belfast is the marked change in them which has accompanied the decline of fever since 1901. This is seen in the diagram (fig. 10) depicting the average notifications of fever in Belfast per week, during the critical quinquennium 1897-1901, and during the succeeding quinquennium 1902-1906, a striking feature of which is the flattening of the spring and summer peaks in the second as compared with the first quinquennium. This feature derives further significance from the corresponding diagram for 1906 alone, which shows the warm weather increase to have disappeared almost completely. It was in this year that the Belfast Corporation commenced their more active measures against the sale of Lough shellfish in the town. I am without detailed figures for the last two years, but I understand that both in 1907 and

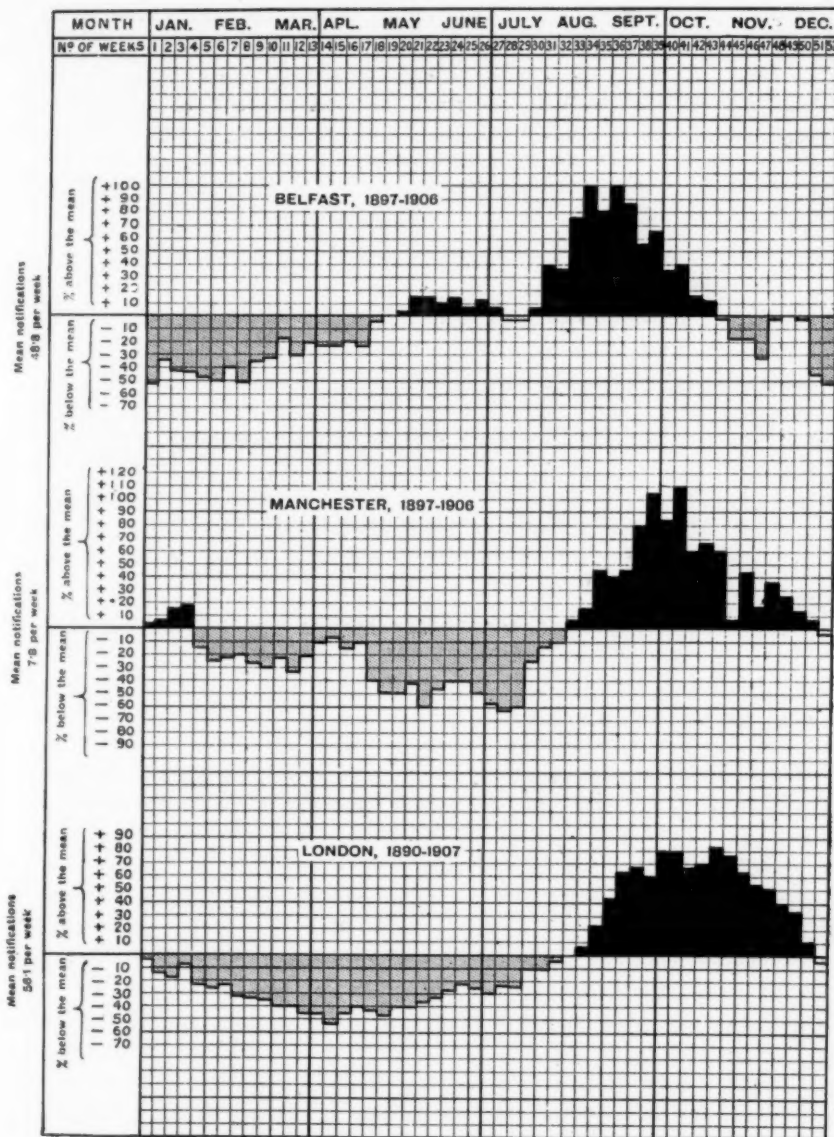


FIG. 9.

Seasonal incidence of enteric fever in Belfast, Manchester, and London.

1908 the warm weather rise of fever in Belfast has been either trivial or absent altogether.¹

These data suggest that the changes in recent years in the seasonal behaviour of Belfast fever are such as might have been expected to accompany a diminution of fever due to a reduction in the consumption of shellfish.

Several minor features of Belfast fever can be explained on the shellfish hypothesis. The escape of the Jewish community has been alluded to, but this community happens to be very small in Belfast. It is suggestive, however, that in a limited prevalence of fever due to infected milk which occurred in Belfast in 1907, Jewish residents did not escape. The escape from fever of the residential neighbourhoods of Belfast, which has always been a marked feature, is, on the shellfish hypothesis, not difficult to understand. This hypothesis serves to explain, too, the great difference in the relative incidence of fever on the eastern and western divisions of the area supplied by Stoneyford water (see page 193), for the eastern division is very largely "residential," while the western division consists almost entirely of the working classes. The difference in the relative incidence of fever in the various sections of the town supplied by Woodburn water can probably be explained in the same way. Another marked feature of Belfast fever has been the almost complete escape of persons resident in public institutions—hospitals, workhouse, prison, asylum—and the like, and this again is easy to understand on the shellfish hypothesis.

It is likewise suggestive that in districts outside Belfast, which adjoin the foreshore at the head of Belfast Lough, fever has been excessive. There are no notification data for these districts, but the mortality returns of the Registrar-General show that in the districts on both the northern and southern shores of the Lough the mean annual death-rate from enteric and continued fever during the decennium 1891-1900 was 0·30 per 1,000. It is probable that this high rate of fever mortality for such scattered and partly residential districts as these are is understated, for the mortality returns in question are not corrected for deaths which may have occurred in public institutions in Belfast. The water supply of these districts is different from that of Belfast.

The Commission interpreted the presumptive evidence in this problem, and they fully recognized that the evidence was mainly of a

¹ The Public Health Department of Belfast have been good enough to furnish me with the figures for 1906 and 1907, and diagrams for these years have therefore been added.

BELFAST.—AVERAGE NOTIFICATIONS OF TYPHOID
FEVER AND CONTINUED FEVER IN DIFFERENT
PERIODS.

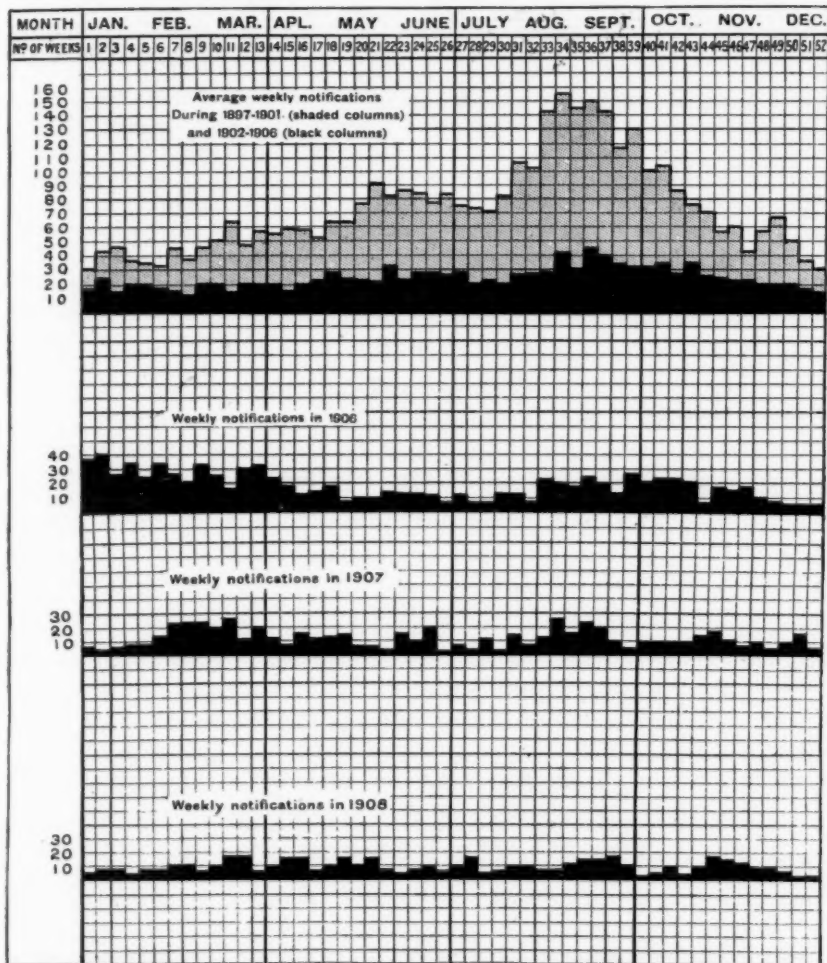


FIG. 10.

presumptive character, to mean that Lough shellfish had been the controlling factor in producing Belfast fever, in the sense that it had been the foundation, so to speak, on which this fever had been built up; not necessarily in the sense that shellfish had been the direct cause of the majority of attacks of fever. I may, perhaps, quote the following from my addendum to the Commission's report:—

The inference to be drawn from the facts appears to be that if this shellfish agency had been absent, the history of fever in Belfast would have been vastly different from what it has been. Instead of this history having been unique, it would have, at the least, approximated that of other towns in the United Kingdom. On the other hand, if the operation of all other agencies of fever could by any possibility have been annulled, and the shellfish agency left alone in operation, the amount of fever in Belfast would also have been much less, although in this hypothetical event the chief characteristics of its history would have remained the same, except as regards amount. . . . It may be imagined . . . that there have been many cases due directly to infection by Lough shellfish, and also numerous cases due to secondary causes of infection, and again, maybe, other cases due, in turn, indirectly to the effect of these "secondary" cases, and so on in cumulative fashion. . . . In the production of secondary cases in Belfast several agencies have, no doubt, taken part, such as personal infection, occasional infection of individual milk supplies, possibly, also, the insuption of infecting material into water mains and service pipes, and, above all, probably, insanitary conditions, using this term in its widest sense. Indeed, it may be surmised that with an agency of fever like shellfish repeatedly in operation—day after day, at times, as it must have been in Belfast—all "insanitary conditions" must have been provided with every opportunity to operate to the full in fostering the disease. . . .

The chief recommendation of the Commission was that power should be obtained to prevent the collection of Lough shellfish for human consumption with a view to protect not only Belfast, but also towns in England to which Lough shellfish are exported. The export of these shellfish, principally of mussels, is very considerable, amounting to over 450 tons per annum; and although much of this export is said to be destined for use as fish bait, it was stated in evidence that large quantities are sent to several towns in this country for human consumption. Periwinkles were stated to be sent even as far as London. The Commission indicated that it was matter for consideration whether the export of shellfish from Belfast should not be controlled under the Public Health (Regulations as to Food) Act, 1907.

Although the conclusion of the Commission as to the enteric fever problem created some surprise in Belfast, it has not been seriously

suggested, so far as I am aware, that the data could be otherwise interpreted. In a reply by the Belfast Corporation to the Commission's report, it was alleged, however, that there were two new facts which seemed to be inconsistent with that interpretation.

One was to the effect that in the summer of 1907 there had been a strike in Belfast, during which "hundreds of people" visited the foreshore of the Lough and gathered cockles, and that, nevertheless, there had not been an "epidemic" of enteric fever in Belfast. This proposition requires the untenable assumption that the infectivity of Lough shellfish is unvaryingly constant, but putting that aside it was not shown, or even suggested, that the attack-rate on these "hundreds of people" had not been relatively greater than that on the many other thousands of the working class in Belfast. In any event a small percentage of attack on these "hundreds of people" need not have caused an "epidemic" of enteric fever or even a material increase in the attack-rate on a population of nearly 400,000. When fever in Belfast was at its acme in 1898 the highest proportion of attacks of fever to possible shellfish eaters was about 15 per cent., and, no doubt, the proportion of attacks, directly due to shellfish, to the total shellfish eaters was much less than this. With fever at least ten times less prevalent in 1907 than it was in 1898, the chances of infection by shellfish would, doubtless, be correspondingly smaller.

The other "new fact" was to the effect that "strict inquiry and investigation" showed that in 342 cases of enteric fever out of 356 notified in 1907, there were not "even the slightest grounds for suspicion that shellfish had been previously consumed by the patients." It was to be expected, on the shellfish hypothesis, that with a reduction of total fever to its then lowest recorded level the proportion of cases associated with consumption of shellfish would be small, but the number of "negative" cases mentioned in this statement was so large that details were asked for. It now appears that the "strict inquiry and investigation" extended only to an examination of a few cases which had been attributed by the friends of patients to the eating of shellfish, and that the absence of suspicion or even of probability in the 342 "negative" cases, as regards the consumption of shellfish in the incubation period, had been very far from established by the inquiries which were made.

Notwithstanding these doubts, measures to prevent the consumption of Lough shellfish have been actively continued since the Commission's report was issued just a year ago, while the report itself has attracted

widespread attention in Belfast to the danger of these shellfish. It is noteworthy that in 1908 the rate of mortality from enteric fever in Belfast fell to 0·15 per 1,000, not only the lowest recorded annual rate for Belfast, but less by one-third than that of 1907. It so happens, also, that during the current year there has been but little fever in Belfast, the notifications for the first three months having been 22 cases of enteric fever and 11 of continued fever, as compared with 189 and 119 in the first quarters of 1907 and 1908 respectively. During the last six weeks, ended April 10, not one case of enteric fever and only four cases of continued fever have been notified, although at this period of the year a commencement of the spring increase might have been looked for.

In concluding this paper I may, perhaps, draw attention to a general consideration which seems to arise, assuming that the main interpretation put upon the facts is correct. In Belfast the opportunities for the operation of shellfish in the dissemination of enteric fever have probably been unique, for it is difficult to conceive the existence elsewhere of a combination of circumstances so favourable to the operation of this agency of fever as in Belfast, with the 12 to 15 million gallons of the crude sewage of its 400,000 inhabitants, discharging daily just outside the city in shallow and sluggish water in the immediate vicinity of shellfish beds, daily drawn upon for local consumption. In this connexion the following extract from the addendum to the Report of the Commission may be quoted:—

It is questionable whether a parallel so favourable to the influence of shellfish in producing enteric fever can be found to this combination of circumstances anywhere in the United Kingdom or, indeed, anywhere else. It can hardly occasion surprise if in such circumstances as these the consumption of the food in question has caused disease on an extensive scale; it would be occasion for wonder if it had not done so.

There is obviously good reason in this case, therefore, not to reject a conclusion simply because it involves acceptance of an agency of fever which has usually been regarded as a contributory rather than as a dominant factor. On the contrary, the very fact that the available evidence points to a relationship between a manifestation of disease so exceptional that it has no parallel in the United Kingdom, and a combination of circumstances, likewise seemingly unique, is itself an indication that the trend of that evidence is not far wrong.

It may be inferred, therefore, that the manner in which the course of Belfast fever has been dominated by the fluctuating opportunities for the operation of shellfish has likewise been exceptional. Nevertheless, fever in Belfast seems to have had its chief characteristics stamped upon it so

decisively by the chief fluctuations of this shellfish factor—in other words, the correlation between Belfast fever and shellfish seems to have been so intimate—that the question naturally arises whether in other towns the influence of shellfish on the course of fever in them may not be greater than has been suspected. The experience of this great community seems at least to make it necessary to recognize the power of infective shellfish to influence the incidence, behaviour and course of fever in large communities, and therefore the more important to prevent the operation of this factor.

DISCUSSION.

The PRESIDENT (Dr. Newsholme) said he was sure the members would agree in expressing their hearty thanks to Dr. Darra Mair for his interesting paper. It was as important and possibly a more important contribution than any which had been contributed to the Section since its formation. It contained the results of an important study of the incidence of typhoid fever in one of the larger cities of the United Kingdom, which had a larger number of cases of typhoid fever in proportion to population than any other in the United Kingdom, and the author showed that this excess was probably in large measure due to the consumption of shellfish. Dr. Mair's investigation of the connexion between shellfish and typhoid fever could be differentiated from any other on the same subject. It possessed a historical value of its own. So far as shellfish were concerned, there had been other investigations: first, that of sporadic cases of typhoid fever occurring at intervals, which had been traced on the strength of cumulative evidence to the eating of oysters or other shellfish; that evidence was familiar to the members: secondly, of explosive epidemics of typhoid due to shellfish, as in the Wesleyan University at Connecticut, and the more recent mayoral banquet epidemics which were investigated by Dr. Bulstrode. Dr. Mair's investigation was necessarily different in method, and in this connexion unique of its kind. When he investigated the subject the data were lacking as to the most important fact—namely, the history in the persons attacked of their having eaten shellfish during the period of incubation. Yet, by working out the history of typhoid fever in Belfast, Dr. Mair had brought together a number of convergent lines of evidence which, when focussed, made out a very strong case for the view that typhoid fever in Belfast was preponderantly due to eating shellfish contaminated on its shores. The paper had much educational interest to those who would be making similar investigations. There were three causes to which the typhoid fever was attributed in Belfast, the first being contamination by water. This, as a chief cause, was excluded by the facts laid before them. In regard to the magnitude of the influence of the privy-middens of the city on the causation of its typhoid fever there was scope for more difference of opinion. There could be no doubt, as in the evidence quoted by Dr. Boobhyer, of Nottingham, that privy-middens

furnished many more opportunities for typhoid fever to be spread, and one would expect that with a diminution of those privy-middens, such as had occurred in Belfast, there would be a decrease of typhoid fever. He thought, however, it was clear from the data supplied by Professor Lorrain Smith and Dr. Mair that abolition of privy-middens was not the chief cause of the diminution of typhoid fever in Belfast. In short, notwithstanding the absence of the personal evidence which was usually available, Dr. Mair had been able to show with a high degree of probability that the consumption of contaminated shellfish was one of the chief causes of excess of typhoid fever in Belfast. It was a pity that some beneficent autocrat could not make the *experimentum crucis* by absolutely preventing the consumption in Belfast during the next three years of shellfish.

Dr. J. ROBERTSON heartily congratulated Dr. Mair on his valuable paper, which, he agreed, was one of the most important the Section had had. His town was one which had suffered considerably from typhoid fever due to the eating of mussels. Some of these had been sent from Belfast Lough at a time when the Belfast people were doing what they could to keep this particular shellfish out of Belfast. Until recent years shellfish had not been noted as an important cause of typhoid fever. Now that the total number of cases of the disease from other causes had diminished, those due to shellfish had become more prominent. Another reason why shellfish had not been recognised, and are even yet frequently overlooked, as the real cause of typhoid fever in many cases, was that reliance in the majority of large towns is placed on inquiries made by a sanitary inspector. It is seldom that a sanitary inspector can get information first hand from the patient unless he waits until the illness is over. During the acute stage of the fever also the patient is often unable to give reliable information. Whenever it is possible to make such direct inquiry, it is found that the number of cases in which shellfish are involved goes up considerably. During the last quarter of 1908 over 25 per cent. of the notified cases of typhoid fever in Birmingham had a history of having consumed mussels within a month of the commencement of the illness. Those who investigated epidemics in places where shellfish were consumed would find much valuable help from Dr. Mair's paper, and would give more attention to that source in future. His own attention was directed to shellfish by an epidemic among working people at Sheffield in connection with oysters which had been brought from Cleethorpes. People who went to Cleethorpes for a holiday came back with typhoid. But most of the oysters which came from Cleethorpes were stored, and did not cause so many cases of the disease as did the fresh ones. The attention which was drawn to the matter at that time had the effect of shutting up those beds—some of the largest around the country. Shortly afterwards the number of cases of typhoid dropped considerably, and thus a definite cause of typhoid was removed. The whole question of shellfish had not yet received the attention it deserves. If the histories of the cases were investigated, it would be found that the incidence was much higher than

was usually anticipated; and the paper would have immense value in drawing attention to that particular point.

Dr. R. J. REECE said that in the beginning of November of last year the Admiralty asked the assistance of the Local Government Board to inquire into an outbreak of enteric fever which occurred in the Royal Marine Depôt at Deal and Walmer, and the investigation had been entrusted to him. This outbreak had begun in August, and had continued until the beginning of November. There were only thirty-four cases, but they were not susceptible of explanation in commonly understood ways. The population in barracks, presumably exposed to such cause of enteric fever as might have been operating within the depôt in the early autumn of the year, was 1,700 persons, but the attacks, with a single exception, were limited to recruits. He concluded that the root of the matter could not be got at unless the history of the first nine cases could be definitely ascertained. These nine cases occurred in August and September and were distributed in two barracks, and in six blocks in these barracks seven rooms were invaded and five different companies were implicated. With the exception of one case, the factor they all had in common was the use of the swimming bath. Therefore the circumstances associated with the bath were made the subject of inquiry. The bath, contained in a brick building on the seashore, was filled with sea water by inflow through an iron pipe, 12 in. in diameter, controlled by a penstock. The bath was filled on a flowing tide, and was so constructed that it could not be filled except on the rising tide. It could not be filled at all when the ebb was established. The bath held 70,000 gallons, and was emptied through the intake pipe by gravitation at or about the time of low water. The bath intake pipe was situated on the beach 116 yards to the north of a sewer outfall from Walmer barracks and town, and 500 yards to the north of another sewer outfall, which also discharged sewage from part of the barracks and from part of the town. The beach had a steep gradient and was composed of shingle, and accordingly for discharge at low water the outfall sewers had not needed to be carried any great distance seaward. Like the intake pipe to the swimming bath, they terminated on the beach. The flowing tide set northwards along the coast, with a direction shorewards along the beach, an incurrent which was continued for two hours after high water, notwithstanding that the level of the sea fell with the ebbing tide. Much of the sewage was deposited on the beach. At almost all states of the tide the position of the sewage in the sea could be accurately gauged by the large number of gulls which fed on it. The water as it entered the bath had an appearance similar to that of storm-water running in the gutters in a London street after heavy rain. If the water of the bath were allowed to settle, there was a deposit of what looked like dark sea-sand, with small brackish grey particles in it. Fifty recruits under swimming instruction used the bath at one time, and this sediment must have been constantly stirred up. If the swimming bath had persistently acted as an agent of infection, its ability so to act must have been, in view of the cleansing operations practised, renewed from time to time, satisfactorily

to account for the incidence of the fever in the several months of its persistence at the depot. Having regard, however, to the physical circumstances of the beach at the mouth of the bath intake, to the conditions alike of the bath water and the sediment therefrom, and especially to the relations of bath intake and sewer outfalls, there can be little doubt that the water of the bath on each occasion of filling was liable to contain organic impurity derived more or less directly from sewage. He found the excreta of enteric-fever patients passed to the sewers discharging 500 yards from the bath intake from the middle to the end of May, and again from about June 21 to near the end of July, and that subsequently to August 4, the date of the first attack at the depot, untreated enteric-fever excreta were passing to the sewer on the beach distant 116 yards from the swimming-bath intake. The quantity of infective material passing into the sewers must have been small compared with the total volume of sewage, and when this entered the sea it must have been still further diluted. But on calm days at low tide and slack water the sewage could be seen as a brown pear-shaped expanse sharply differentiated from the surrounding sea, and it remained so until the flowing tide set to the north, when the sewage in a brown stream would stretch away parallel to the beach. Granting that the cases of enteric fever in the depot were due to infection derived from the sewage containing the untreated excreta of enteric-fever cases in the town, there was the fact that the bath was used by 300 men, yet there were only thirty-odd cases of enteric fever. Thus the dose for each man who used the bath must have been small, and the amount of infective material must have varied from time to time. The fact, however, remained that when the swimming practice was stopped, at the end of October, the enteric outbreak ceased, and the bath had not been used since. Possibly small doses of infective material were swallowed by the recruits while swimming. He considered that this outbreak of enteric fever in the Royal Marine Depot was in some measure comparable with the vaster occurrence of the disease in Belfast. If it be accepted that the cockles in the Lough were liable to contain the infective material of enteric fever derived from the sewage at Belfast, then it was manifest that the quantity of this infective material any cockle might contain would be small, and would not be evenly distributed to every cockle, nor partaken of equally by the consumers of these cockles.

Dr. J. T. C. NASH: A correspondent of the *Lancet*, when the Commission was first appointed to inquire into the sanitary affairs of Belfast, commented on the prevalence of typhoid fever in that city and on its probable connection with either milk, water, or imperfect drainage. In an article¹ I contributed to the *Practitioner*, in the autumn of 1906, I referred to the "epidemic influences of shellfish and flies," either or both of which, I suggested, might be at the bottom of the Belfast trouble. I personally expected that the Commissioners would

¹ "The *Ætiology of Typhoid Fever*," by J. T. C. Nash, M.D., *Practitioner*, Lond., 1906, lxxvii, pp 796-7.

find a closer connection between Belfast typhoid and infected shellfish than either water supply or drainage. The masterly way in which Dr. Mair in his paper deals with the evidence which was obtained by the Commission is fairly convincing that such evidence was abundant, was well sifted, and was far and away more conclusive than any evidence against either water supply or drainage. Early in 1903¹ I ventured to lay before the Epidemiological Society of London the thesis that the seasonal incidence of typhoid fever was probably largely due to the consumption of polluted shellfish. Prior to that the leading theses as to seasonal incidence were Pottenkofer's ground-water theory, and (for London) Dr. Corfield's and Sir Shirley Murphy's flood-water theory; both theories assuming pollution to occur through prior befoulment of the soil by manurial deposits, leaking cesspools, &c. Both theories were founded on the commonly accepted and fully proved axiom that typhoid fever is generally a water-borne disease. I emphasized the point that even in the specific pollution of shellfish (and we may say the same of watercress, celery, &c.) the poison was still really a *water-borne* infection, inasmuch as sewage is merely polluted water. At the same time, in April, 1903, I presented mean curves of typhoid-fever incidence which I had collected or worked out for London, Edinburgh, Southend, Ipswich, Great Yarmouth, Brighton, Liverpool and Manchester, and indicated how their differences in character could be explained on the shellfish theory—the same applying in my opinion to charts which Dr. Thresh had incorporated in his annual report to the Essex County Council for the year 1902. In the previous year Dr. Thresh had drawn attention to the great excess of typhoid prevalence in the districts at the mouth of the Thames. In a special report on typhoid fever in Southend in 1902 I accounted for this excess in my view as being due to the fact that shellfish were naturally more freely consumed in the “area of excessive prevalence,” the area being also the shellfish area. Dr. Darra Mair now records that not only in Belfast, but in districts outside Belfast which adjoin the foreshore at the head of Belfast Lough, fever has been excessive, and the Commission in my judgment very rightly interpreted the presumptive evidence to mean that polluted shellfish played the principal part in this.

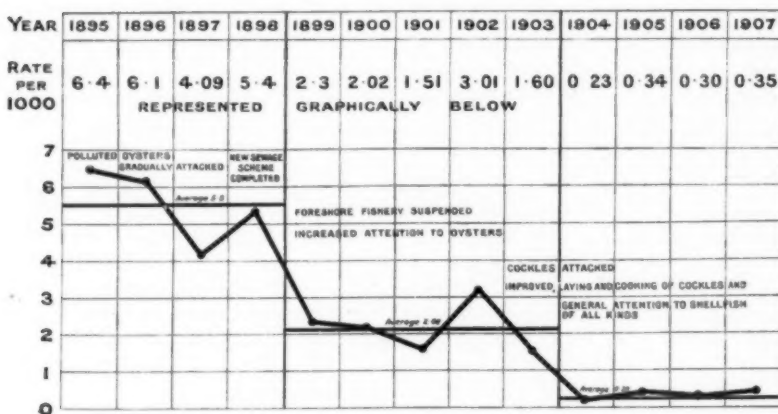
Dr. Mair, in his Tables I and II, gives for certain large cities which have been more or less conspicuous for excessive mortality from enteric and continued fevers the mean annual “fever” death-rates, Belfast in each period heading the list with rates of 0·51 in the decennium 1881-90, 0·86 in 1891-1900, and 0·57 in 1901-5. The Commission appear to have thought the history of fever in Belfast as unique in the United Kingdom, inasmuch as there is not a definite season for shellfish in Belfast. In 1903 I ventured to explain the unusual curve for Brighton as being probably due to the fact that Brighton had a winter season as well as an autumn season, and suggested that the autumn wave of prevalence was probably due to all kinds of shellfish, but probably

¹ “The Seasonal Incidence of Typhoid Fever and Diarrhoea: the Seasonal Consumption of Shellfish probably one of the chief Factors as regards the former, and the Seasonal Prevalence of Flies as regards the latter,” by J. T. C. Nash, *Trans. Epid. Soc. Lond.*, N.S., 1902-3, xxii, p. 110.

more particularly mussels; while possibly the December wave was more directly traceable to oysters, as being the form of shellfish more likely to be indulged in by the class of winter visitors. Perhaps the President of this Section, whose pioneer work in tracing cases of typhoid fever to polluted shellfish is universally recognized, may be in a position to inform us whether the percentage of cases among visitors traceable to "oysters" was on the average higher in December than in September in Brighton, when compared with other forms of shellfish. In the same paper in 1903 I showed two curves for Great Yarmouth—one being the average mean curve of enteric incidence during the years 1897-1902, and the other for 1902 alone. Unusual numbers of cases of enteric fever occurred in Great Yarmouth in the winter season of 1898 and 1899.

BOROUGH OF SOUTHEAD-ON-SEA.

TYPHOID INCIDENCE per 1,000 Resident Population



These were traced by the late Dr. Russell, at that time medical officer of health for Great Yarmouth, to the consumption of mussels. The sale of these was stopped, and forthwith there was an astonishing reduction in the incidence of typhoid fever in Great Yarmouth, as illustrated by the curve I constructed for the year 1902; the incidence during the month of *highest* prevalence for that year (September, 1902) was actually no less than 30 per cent. below the monthly mean for the seventy-two months of the years 1897-1902, which were all the figures I could obtain. In giving evidence before the Royal Commission on Sewage Disposal in May, 1903, I drew the special attention of the Commission to these figures and curves for Great Yarmouth, and ventured to prophesy that I should be able to produce a similar reduction for Southend in the future. I can fulfil my prophecy, and now reproduce the typhoid-incidence

curve for Southend-on-Sea as illustrated in the last annual report I wrote as medical officer of health for that borough for the year 1907. I venture to submit that no clearer proof or more remarkable diagram exists to prove the relation between polluted shellfish and the attack-rate of typhoid fever; and I would further call attention to the fact that even the comparatively few cases which are still occurring are mainly of shellfish origin, and that, were it possible to absolutely stop all shellfish cases, the incidence of typhoid fever in Southend would approximate to the vanishing point.

The calculation of attack rates for each of the fourteen districts in Belfast for monthly or weekly periods, and the plotting out of these rates diagrammatically by Dr. Darra Mair, is valuable and strong evidence in favour of the absence of relationship between the fever and the water supply, and at the same time indicates that there was an agency of some sort acting similarly and simultaneously in diverse parts of the city. In this respect again Dr. Mair's researches corroborate my seven years' investigations into the causation of typhoid fever in Southend. I might here refer to some clinical evidence in support of the epidemiological investigations as to the relationship between shellfish and typhoid fever. I was medical officer to the borough fever hospital at Southend as well as medical officer of health, and since nearly every case of typhoid fever was removed to hospital I had the advantage of treating the cases clinically as well as of prosecuting my epidemiological inquiries to greater advantage. As a result of my combined experiences extending over nearly seven years, I will just now mention only two points of importance:—

(1) The clinical evidence supported the epidemiological in this way: (a) The cases of typhoid fever which followed on the eating of *raw* shellfish, whether oysters, mussels or cockles, and perhaps more particularly raw cockles, were exceptionally severe in general clinical character. (b) The cases of typhoid fever following on the eating of cooked shellfish (cockles) were clinically of a much milder type in general clinical character.

(2) On cross-examining patients, especially during convalescence, when proper mentality had been restored, it was often possible to get a definite and clear shellfish history which had hitherto been denied by friends, and often even by the patient. In a few cases the consumption of the shellfish at a certain date had really been conscientiously forgotten, or the patient was too confused while really ill to remember; in other cases there had been a deliberate attempt at concealment for various reasons; and it was not until the patient found himself in the company of other shellfish victims that he would realize or acknowledge the possibility of shellfish infection and own up to having eaten them. On many occasions I have had to alter the Sanitary Inspector's inquiry card record from a history of "no shellfish" to a definite consumption of shellfish on a clearly definite date. The cross-examination into shellfish histories needs to be accomplished with almost forensic skill in some cases. I found the old plan for getting a history of specific (syphilitic) infection useful also for getting a history of specific (shellfish) infection in cases where I felt pretty certain of my deductions, and in certain cases where I thought the ordinary

question, Have you had any shellfish? or, Has the patient had any shellfish lately? would only invoke a falsehood, I have successfully put the question with the previously assured knowledge in the form, When did you eat the shellfish?

Then, again, Dr. Mair furnishes us with valuable corroborative testimony when he criticises a reply attempted by the Belfast Corporation to the Commission's report; for he shows that what was called "strict inquiry and investigation" had extended only to cases where the friends themselves attributed the illness to the eating of shellfish, while the absence of suspicion or probability as regarded the consumption of shellfish in fully eleven out of every twelve cases had been far from established by such inquiry as had been made. No doubt many epidemiologists are even now in the state of mind of the Belfast Commissioners before they began their inquiry, and look upon the shellfish agency of typhoid fever as quite a minor factor. Dr. Mair himself had been inclined to the view that this aspect of the aetiology of the disease had been somewhat exaggerated. His evidence and the conclusions the Commissioners were forced to, almost against their prior ideas, are therefore of the utmost importance, and many other persons also are rapidly becoming converted to the idea that the shellfish factor is a *major* factor in the incidence of typhoid fever. In 1903, in the paper I have already referred to, I exhibited before the Epidemiological Society a simple schematic diagram which in my opinion still illustrates the chief factors in their relative importance in the seasonal incidence of typhoid fever and the seasonal incidence of diarrhoea.¹

In country districts where sewerage schemes do not exist, and shallow wells are in use which chemically show excess of organic matter in the water, cases of enteric fever are forthwith attributed to the water or the sanitary surroundings when skilful cross-examination would have elicited a clear history of shellfish. To give a case in point: Five cases of typhoid fever occurred at a farm in the heart of the county. The cause of the outbreak was attributed by the local authorities to the insanitary conditions existing at the farm. Having occasion to inquire closely into the outbreak, since the situation of the farm was a possible source of peril to the inhabitants of a large city a few miles away through the drainage from this farm, I found that the first case of typhoid fever at the farm occurred in the person of a young man who weekly went into the city to market and had on each of two Saturdays prior to the onset of his illness partaken of oysters. The other four cases were clearly secondary to the first, and here the probable agents at work carrying infection from one to another were house-flies, which were simply swarming on the premises, on every person, and on every exposed article of food. In this sense, through the filthy conditions existing at the farm proving an ideal nidus for the breeding of flies in countless thousands, the insanitary conditions to some extent accounted for the secondary cases through providing suitable "carriers." Now, although only one of these five cases had eaten shellfish, I contend that but for that one case the other four would not have arisen; and therefore indirectly the polluted shellfish were really responsible for all the five cases. Take again

¹ It is illustrated on page 129 of the *Trans. Epid. Soc.*, N.S., 1902-3, xxii.

human "carriers" of typhoid bacilli. Say one of these visits a seaside resort where shellfish are cultivated in a position where they are liable to pollution. The visitor, not himself ill, may pollute dozens of shellfish, which then prove fresh centres of infection of single cases or groups of cases.

As my contribution is already of considerable length, I propose to notice only one more point touched upon by Dr. Mair, and that is the distinct connection between the enormously increased chances of pollution of the shellfish in the Lough of Belfast through the breakdown of the wooden sewer or "shoot" in 1896 and the critical increase of typhoid fever which began in 1897. The chart I incorporated in my report on the health of Southend-on-Sea for the year 1907 is a striking illustration of a similar direct connection between *lessening* the chances of pollution of shellfish by removing the sewage outfall to a point considerably further away from them, in the year 1898, and the coincidental reduction in the incidence of typhoid fever on a large scale since 1899. Here, again, is a parallelism between the Belfast records and the Southend records, accentuated in value through the parallelism working in exactly opposite directions, diagrammatically shown thus:



But closely associated with the history of Southend typhoid is the history of the town of Leigh, which gives an illustration parallel with that of Belfast, in this way: Leigh is a great cockle centre. The cockles were brought in from the Maplin sands and laid in the creek alongside the town; here, no doubt, they suffered some pollution from a few drains and occasionally accounted for cases of typhoid fever, but after a sewage scheme was inaugurated, which brought down the sewage of the whole town of 4,000 inhabitants to filter-beds some 200 yds. above the principal cockle lays—the effluent from which, discharged during the latter part of the ebb, reached the cockles in least diluted form and so diffused that practically every cockle became polluted—cockle typhoid reached such proportions that I was enabled easily to demonstrate the influence even so-called "cooked" cockles had in the incidence of typhoid fever—an influence up to that time unsuspected, though "raw" cockles had been previously incriminated.

The new records from Belfast support my older contentions, and my older records for Southend undoubtedly should assist to a clearer understanding those inclined to be sceptical as to the correctness of the views of the Belfast Commission, so ably set forth, with the evidence so neatly epitomized, by Dr. Darra Mair in his most important contribution on the etiology of enteric fever. No doubt the conversion of the privies into water-closets has had some effect in reducing typhoid fever in Belfast, and probably the reduction has been most marked in the autumn months, when *flies* are prevalent.

Dr. W. G. SAVAGE said he felt that, like others, he had learned much from the paper as to the value of careful and accurate epidemiological inquiry. He wished to refer to the question of the *Bacillus coli* being agglutinated by typhoid serum. Nine years ago Professor Lorrain Smith contributed a paper to the *British Medical Journal*, in which he advanced the view that the *Bacillus coli* was more likely to have been associated with cases of typhoid fever if it was agglutinated by a typhoid serum. At the time he thought there was much in the supposition, and for several years he investigated the question as far as he had time, but he was unable to obtain any evidence in support of it. He believed that it was nothing more than an interesting hypothesis. Cockles were a likely means of spreading disease. Their infectivity was not destroyed by the amount of boiling to which they were usually said to be subjected. In 1903 he carried out many experiments on the effect of heating to destroy the intestinal organisms in cockles. He found that a minute's boiling did not kill all the bacilli, *i.e.*, boiling in an open vessel (which was not always at 100° C.). Two minutes' boiling generally killed them. They might be killed in the juices, but could usually be recovered from the gut after one minute's boiling. He had never yet found a cockle which did not contain the *Bacillus coli*. They were easily found in some cockles which he himself collected from a place where there could be no suspicion of contamination. The water washing them only showed *Bacillus coli* when as much as 40 c.c. were examined. Professor Klein (Local Government Board Report for 1900) recorded that he infected cockles with typhoid bacilli, and found that cockles, unlike some other shellfish, did not readily clear themselves of the bacilli when they were put into fresh water. He found, in certain experiments, that the typhoid bacilli multiplied in the cockles. That was an important statement which he had not seen confirmed or refuted elsewhere. The fact that he had never found cockles free from the *Bacillus coli* might be explained on the view that the bacilli multiplied in the cockles. If these suppositions were correct, cockles were a very dangerous source of infection. In 1904-5 he carried out many experiments upon the examination of tidal mud.¹ He experimentally infected such mud with typhoid bacilli, and he found he could recover the bacilli at the end of two weeks, but not later. The supposition that infective bacilli might multiply in cockles, and his experiments showing that in tidal mud typhoid bacilli had a considerably longer life than in pure drinking water, agreed with the facts which Dr. Mair had brought forward, and emphasized the view that cockles might be more than subsidiary, that they might be an extensive contributory factor in spreading infection.

Colonel MACPHERSON said the outstanding fact which he had learned from the paper was that enteric fever—that is, endemic—might be due to shellfish. That had been instructive to those who had investigated the distribution of the disease. But he was not satisfied that the author had proved his case as against the privy system, because in all the investigations he had had to make in connexion with the incidence of enteric fever in the army one fact had stood out above

¹ *Journ. of Hygiene, Camb.*, 1905, v., p. 146.

others—namely, that the garrison towns where enteric fever was endemic or constant amongst the military population were towns where there was no proper sewage system but the privy system, and these were not necessarily seaside towns. There was also seasonal prevalence of enteric fever, which was undoubted in all countries in the season when flies were most plentiful. If in Belfast the gradual abolition of the privy system was coincident with the diminution of enteric fever, it seemed to prove in a way the privy origin of the endemicity of enteric fever in Belfast. Although the author had shown by maps that apparently enteric fever was in larger proportion where there were few privies and in smaller proportion where there were many, he thought many other factors might be at work there, such as immunity acquired in early life. If the shellfish were the greatest cause of the endemicity of enteric fever in Belfast, one would think that with the growth of the sewer system, and consequent pollution of the seashore, there would be an increase in enteric fever; but, with the exception of one peculiar rise, this had not been the case. From 1902 onwards, when more sewerage was being thrown on to the foreshore in Belfast, there was less enteric fever. Dr. Mair explained this by the fact that the use of shellfish was more or less prohibited, but it had not been a compulsory prohibition as far as he could gather. No doubt the people who liked to eat shellfish would go on the foreshore and gather shellfish for themselves, if there were any prohibition to shellfish being hawked in the streets. He had found some difficulty in accepting the shellfish as the true cause of the endemicity of the disease; but he fully recognized the value of the paper, and was quite prepared to accept Dr. Mair's observations and facts as of the highest importance in the light of investigation into the etiology of the endemic existence of enteric fever in a particular locality.

Dr. MEREDITH RICHARDS said he agreed in general with the conclusions of the author, but did not think sufficient attention had been paid to the danger of the privy-midden. In years past he had studied the subject in Nottingham and in the colliery districts in Derbyshire. As soon as the Nottingham Corporation began to suspect the relation between privy-middens and typhoid, they began emptying their privies at more frequent intervals, and he thought that was the cause of the fact that with that activity the incidence on privy-midden houses increased because a dose of infection was turned out into the streets at much more frequent intervals. The seasonal incidence in Belfast corresponded with the possibility of considerable privy-midden causation, as middens were specially dangerous in the summer when their contents were readily converted into dust. It did not necessarily follow that the incidence should vary with the proportion of privy-midden houses in the different districts, because the privy-midden in itself was a sufficient cause. He remembered an investigation he made into a colliery epidemic in Derbyshire where all the houses had the same water supply. He was able, by getting hold of the first cases, to trace the passage of enteric fever up one street and down another by ascertaining which privy-middens were first of all specifically infected. In that case every house was a privy-midden house, and the distribution in the village followed the opportunities for specific infection.

Dr. A. W. J. MACFADDEN said the paper had been of particular interest to him, as he was in Belfast during the period with which it dealt. The various factors which had been considered as causal from time to time in Belfast had been well set out in the paper. Among a number of the general practitioners in the town the theory of shellfish infection was one discussed. The conditions of Belfast Lough described by the author could scarcely be realised unless one had seen them. He had travelled along the northern shore of the Lough once or twice a week, and owing to the foul smells which arose from the foreshore it was impossible to exist in the railway carriage unless the windows were shut. The sewage threw up a green seaweed which decayed on the foreshore, and the Lough itself was saturated with sewage. In Belfast all the working classes ate cockles derived from this polluted source; men bought them on their way from work from hawkers at all times of the year, except in winter, and ate them uncooked. He did not think the better classes ate cockles at all. He thought those facts pointed to Dr. Mair's conclusions, and accounted for the class incidence in this case and for the exceptional rises shown on the chart in the earlier months of the year, which contrasted so markedly with the London and Manchester curves shown on the sheet. In the number of typhoid cases he had seen in Belfast he almost invariably got a history of eating shellfish, but the significance of this point was somewhat obscured at the time by the fact that habitual shellfish eating was so universal among the workpeople who escaped. Looking back, however, he thought the eating of shellfish sufficiently accounted for the curves, and that a shellfish curve was superimposed upon the normal curve of typhoid fever in the town. There were, in his experience, many cases of secondary infection in other members of the family in Belfast, a circumstance which was largely accounted for by the unsuitable hospital accommodation available. Except for the workhouse hospital, which many of the workpeople disliked, no hospital existed at that time where fever patients could have been properly admitted. Thus many cases of typhoid which should by right have been treated in hospital were treated at home.

Dr. HAMER said he understood Dr. Nash to suggest that the exemption of the better-class population in Belfast was accounted for by the fact that there were no oysters in Belfast Lough. He wished to ask whether Dr. Nash had considered the claims made by those who attached great importance to bacillus carriers. He assumed, having in view the heavy incidence of typhoid in the city, that at least 1 per cent. or 2 per cent., perhaps even 5 per cent., of the working-class population of Belfast were "carriers." He arrived at that assumption on the basis of methods of estimation adopted by various speakers at the meeting of the Society of Medical Officers of Health at Colchester a year or two ago. If even a small percentage of the food-manipulating class—cooks, domestic servants, &c.—were carriers, that would lead those who favoured the contact hypothesis to predicate a wide diffusion of the disease among the better-class population. If there were, say, 10,000 cooks alone, and only 500 of them were typhoid-bacillus carriers, that would be, according to the contact theorists, a serious thing for the better-class population of Belfast. The milk supply, and

the question of fruit, uncooked vegetables, meat, fish, &c., also needed consideration in this connexion. He was prompted, therefore, to ask Dr. Mair to tell them whether special steps had been taken in Belfast with regard to bacillus carriers. For example, had urotropin been largely given, or had the bacillus been attacked in the gall-bladder and bile-ducts? Had there been many removals of gall-bladders of milk sellers, provision dealers, domestic cooks, &c., in the city? Or, again, had the excreta and the sputum been disinfected in any peculiarly effective way? If none of these things had happened, how did it come about that the better-class population of Belfast had escaped? Another point which was suggested by one of the charts exhibited was that at the time when a marked fall of typhoid occurred in Belfast in the early years of this century, Koch was starting his campaign against the disease in the South-West of Germany. If, instead of working there, Koch's followers had been working in Belfast, they would no doubt have confidently attributed the marked decrease in the typhoid of Belfast to campaign methods. In South-West Germany there followed upon the adoption of the new scheme of operations an actual increase in the amount of typhoid. It was interesting, therefore, to note how readily *post hoc* and *propter hoc* might have been confused with one another, if the methods of Koch had been applied to Belfast instead of to South-West Germany.

Dr. CHALMERS wrote: Dr. Mair is, I think, to be congratulated on having presented a most interesting and extremely lucid description of a most exhaustive inquiry. The facts, as they emerged, have been marshalled in a series of postulates which in succession are shown to be untenable until the final one is reached. Moreover, the postulates fairly represent the successive steps by which the conclusion was reached. Quite at the outset of the investigation and as a general proposition it seemed to be in consonance with experience elsewhere that local insanitary conditions would suffice to explain the incidence of enteric fever, until it was found that the existence of the one and the occurrence of the other did not coincide. In a similar way there was a lack of correspondence between the distribution of the disease and the distribution of the various water supplies. There was the further fact that filtration of both Stoneyford and Woodburn water had been begun before the maximum prevalence of the disease, in 1897-1901, occurred. And while it is true that the reduction in enteric fever has been most striking during the last few years—since, indeed, the extensive substitution of water closets for privies which the 1899 Act made possible—the unprecedented prevalence of the disease in the period 1897-1901, before this substitution was undertaken, comes in, as Dr. Mair says, to prevent us accepting this substitution as the dominant factor in the change. It is in this connection, I think, that Dr. Mair has placed the Society and Epidemiology as a science under a debt of gratitude. The danger of limited views in any investigation is well known to members of the Society, but the special danger in the present inquiry lay in the temptation to find an explanation of the cause of the enteric-fever prevalence by reference only to the facts ascertainable over a few years, and these the most recent ones. This

danger, indeed, was greater than might have been experienced in any other part of the country because of the comparative scarcity of readily available local returns. It is to Dr. Mair's painstaking insistence in collecting the facts which made a historic review—extending over many years—possible that we owe the elucidation of a problem which at the outset was beset with difficulties of very considerable magnitude.

Dr. KING KERR, Chairman of the Public Health Committee of the Belfast Corporation, wrote: Dr. Darra Mair has very kindly sent me an advance copy of his paper on "The Etiology of Enteric Fever in Belfast," to be read before your Society on April 23, with an expression of his desire that, in the event of my being unable to be present at the discussion, I might offer some criticism upon it. With the permission of your Society I beg to submit a few observations. I am unable to accept Dr. Darra Mair's conclusions in their entirety as establishing the responsibility of shellfish for our serious outbreaks of enteric fever, mainly for the reasons referred to in the paper, the simultaneity of the outbreak over large and widely separated areas, which in my judgment would infer one or both of two things—a suddenly increased consumption of shellfish on the part of the populace, or an increased infectiveness precedent to the rise and a correspondent decrease precedent to the fall of the fever. The seasonal curve argument brought forward in the paper is certainly not borne out by the notifications of enteric and simple continued fever in 1908, being for the first quarter 112, second quarter 126, third quarter 131. With regard to Dr. Darra Mair's criticism of our "strike" argument as opposed to his hypothesis, it must not be forgotten that in strike times hundreds of shellfish gatherers represent many more hundreds of eaters, and therefore in our case cause and effect had a fair opportunity of being established, whilst as a matter of fact not only during the period of the strike, but for two subsequent months, there was a markedly great decrease in our notifications as compared with the previous year, being:—

		July		August		September		October	
	1906	...	21	...	45	...	61	...	52
(Strike)	1907	...	20	...	32	...	27	...	26

Whilst I offer these criticisms, it is only right, in an impartial discussion of so important a question, that I should state that towards the end of 1905 or the beginning of 1906, on our late executive sanitary officer, Mr. Conway Scott, placing in my hands a volume of evidence on shellfish pollution given before the Royal Commission on Sewage Disposal, I felt a thorough investigation into this matter was demanded at our hands. Samples of cockles were gathered all along the foreshores on both sides of the Lough, which were sent to Professor Symmers for examination, and on his report of their dangerously polluted condition dangerous areas were mapped out and all cockles gathered at these places offered for sale were seized and condemned. Whether *propter hoc* or merely *post hoc*, three weeks later a marked diminution in our enteric notifications began and continued. Speaking in the Council Chamber at the monthly

meeting in the following June, I stated that the Public Health Department had been conducting investigations into the causation of our enteric fever, and had been taking active steps in this connexion with marked and hopeful results. Whilst I cannot go as far as Dr. Darra Mair in this matter, I did then, as I do still, regard shellfish eating as an important factor in our fever causation.

Dr. P. BOOBYER wrote: I note several points in the Belfast experience which coincide with mine in Nottingham and elsewhere. For instance, the water supply was shut out as a causal factor by the localization of the disease in working-class neighbourhoods. Again, you observe "that the decline of the fever since 1901 has coincided with the conversion of privies into water-closets." I dare say you have noticed that the same thing has happened at Leicester, except that the dry-closets converted there were pails instead of privies. Your case against the shellfish from the Lough appears to be unanswerable, but I think many medical officers of health have generalized too freely about the influence of shellfish in this regard, from such experiences as those of Belfast. To mention Leicester again. This town had formerly as much typhoid as Nottingham. It has now practically got rid of it, while ours continues with slight abatement only. Leicester has abolished its dry-closets during the past fifteen years; we have still more than 36,000. But please note that the shellfish supplies of Leicester are identical as to source with ours, and but little less, I think, in quantity.

Dr. DARRA MAIR, in reply, thanked the President and the Section for their kind reception of his paper, and for the valuable discussion which had taken place, a significant feature of which had been the absence of any suggestion that the facts he had detailed could be explained by a hypothesis that the public water-supply had been responsible for enteric fever in Belfast. In answer to Dr. Hamer, it had actually been proposed that Koch's method of dealing with enteric fever should be adopted in Belfast, but, needless to say, it had not been carried out. Neither had any of the valiant measures against "carrier" cases, hinted at by Dr. Hamer, been attempted. Despite inactivity in these directions, fever had rapidly declined in Belfast as soon as the operation of the dominant factor (shellfish) began to wane. In spite, too, of the inaction against "carriers," the extension of the disease from the working classes to the wealthier residents, even during the period of greatest prevalence, had been of the most trifling description. If "carrier" cases were a power of serious practical importance in conveying the disease from person to person, it should have been manifested in abundance in Belfast, if anywhere. Dr. Robertson had drawn attention to the possible influence of imported Belfast shellfish on fever in Birmingham, and it was noteworthy that Birmingham, like Belfast, showed an increase of fever mortality in the decennium 1891-1900 as compared with the previous decennium (*see* Table I). This increase was exceptional, the only towns among those in Table I showing a like increase besides Birmingham being Sheffield, Grimsby, and Sunderland. It might be useful to ascertain whether this exceptional increase could be explained in connexion with shellfish, and it was rather significant that it had been much greater than elsewhere in Grimsby and

Sunderland, both of which were coast towns. Dr. Robertson's and Dr. Nash's cautions as to accepting the investigation of the sanitary inspector in regard to the consumption of shellfish during the incubation period were very important. In Belfast much of the scepticism of the "shellfish hypothesis" had been due to statements that an insignificant proportion of fever cases had been associated with the consumption of shellfish, and these statements had been based, almost entirely, on the inquiries of sanitary inspectors. He (Dr. Darra Mair) had referred in his addendum to the Commission's report and since to the difficulties of ascertaining the truth about the consumption of shellfish, and he hoped that Dr. Robertson's and Dr. Nash's experience would be noted carefully in Belfast. Dr. Robertson was no doubt right in thinking that insufficient attention had been paid to the possible influence of shellfish on the course of fever in large towns, and though Dr. Boobyer might be right, too, in thinking that, in some instances, there had been too loose generalization as to this influence, he would probably agree that there had also been similar looseness of generalization as to the influence of the privy-midden.

Lieutenant-Colonel Macpherson had dwelt on this privy-midden aspect of the matter as the result of his Army experience. The question naturally arose whether shellfish might not be a potent cause of fever among soldiers in barracks, as among other working-class sections of the community, and whether this factor had been fully considered in their case. Privy-middens, however, must necessarily have been responsible for very many cases of fever in Belfast; it would be wonderful if they had not been. But there were great difficulties in explaining the high fever mortality of many years, and particularly the enormous increase of the critical period (1897-1901) by the influence of this factor. If it had been paramount, as Colonel Macpherson suggested, how did he explain why in Belfast alone it had been capable of causing fever far in excess of that in any other town in England, Scotland, or Ireland? No other town in the United Kingdom—or even, it would seem, in Europe—possessed a quinquennial fever record like that of Belfast during the quinquennium 1897-1901. In this prolonged period the mean annual rate of mortality (1·2 per 1,000) in Belfast had been practically double that of the town (Sunderland) which had the next highest record for a similar period, and treble those of other towns conspicuous for sustained high mortality from this disease. When, in addition, it could be shown that many towns in Ireland—Cork, for instance (*see* Table I)—in which privy-middens prevail had a very low mortality from enteric fever, it was begging the question to attribute Belfast's exceptional mortality to this as the predominant factor. Colonel Macpherson suggested that although the distribution of fever in Belfast had not followed the distribution of privies, this discrepancy might be explained by assuming that in the part of the town with few privies (District 12) some other cause might have been at work. An occasional discrepancy of this kind might have been so explained, but the facts of the case were that the behaviour of fever in this part of Belfast had been almost exactly similar, over a long period, to that in another part (District 10), two miles distant, where

privies were almost universal. The similarity of the fluctuation of fever in these districts showed itself not only for one year, but for every year for at least ten years; and not only year by year, but month by month, and almost week by week, through this long period (*see* Diagrams III and IV). Such facts could not be explained by referring the manifestation of the disease to privy-middens in the one case and to some other factor in the other case. Dr. Richards, while agreeing with the main conclusion of the paper, suggested that the increase of fever in the critical period might have been due partly to the result of increased municipal activity in emptying privy-middens, by bringing about greater disturbance of their contents and their distribution as dust. The increase of fever, however, did not synchronize with any increased activity of this kind. On the other hand, the greatest of all disturbances of privy contents in Belfast took place in 1892, when public scavenging commenced there—according to published accounts, upheaval would better describe this operation—and this had not been associated with an increase of fever mortality (Table II).

The coincidence of the decline of Belfast fever with the abolition of the privy-midden supplied the only reason for connecting Belfast fever with the privy-midden factor. It was, however, a remarkable and one of the most interesting features of the Belfast fever problem, that this decline of fever coincided, more or less closely, with no fewer than three distinct occurrences—viz., the abolition of the privy, the introduction of the Mourne water supply, and the reduction in the consumption of Lough shellfish. But of these three occurrences the antecedents of one only could also be made to fit in with the increase as well as the decline of fever; in addition, the antecedents of the same occurrence fitted in with the observed facts of the distribution and behaviour of the disease, while the others did not. One of his colleagues on the Commission, Dr. Chalmers, had specially emphasized this point in his contribution to the discussion. To set aside the one factor which explained both the increase and the decline of fever, together with so much else that was essential in the facts, and to regard as predominant one of the other factors which, although it might explain the decline of fever, did not explain either its increase or few if any of the other essential facts, could not be sound epidemiology.

Dr. Savage had referred to valuable bacteriological observations made by himself regarding the danger of sewage polluted cockles, even when heated. In Belfast, however, the almost universal practice had been to eat cockles raw—they were opened by pressing one against another—and even mussels were usually eaten there without previous cooking. Dr. Savage's observations as to the survival of intestinal micro-organisms in sewage mud, and his reminder of Dr. Klein's observations that *Bacillus typhosus* multiplied in cockles, were most interesting. Dr. Reece's investigation at Deal supplied a notable example of how a "vicious circle," somewhat analogous to the case of shellfish, might be established, so as to bring about the ingestion by human beings of doses of their own sewage and of the pathogenic organisms it might contain. Dr. MacFadden's remarks were of particular interest coming as they did from one who had been

in practice in Belfast when enteric fever was at its height. His account of how the consumption of cockles by the Belfast working classes had then been practically universal helped to explain why the shellfish factor in the causation of fever had been overlooked for so long there. Although a history of consumption of shellfish should have been obtainable, almost always, among those attacked by enteric fever, the fact that the vast majority of shellfish eaters escaped attack must have tended to disarm suspicion. The scanty hospital accommodation referred to by Dr. MacFadden had no doubt been responsible for many cases of fever, through personal and other secondary means of infection. Hospital accommodation of this kind was deficient even at present in Belfast, for although an excellent isolation hospital was opened in 1906, it had not been fully utilized owing to its distance from the town. Despite the scantiness of hospital isolation, however, the great decline of enteric fever had nevertheless taken place.

The principal difficulty referred to in the communication which Dr. King Kerr had been good enough to send seemed to him (Dr. Darra Mair) to be an essential feature of Belfast fever which could only be explained by the shellfish hypothesis. It was undeniable that practically all shellfish consumed in Belfast came only from one source, Belfast Lough, and that their distribution in the past throughout the city had varied but little from month to month or even week to week; therefore a broad similarity in the behaviour of fever caused by such shellfish in various sections of the city was precisely what might have been expected. The infective power of the daily gatherings of shellfish varied, no doubt, within wide limits from time to time, as also the precise range of the distribution of the shellfish by hawkers, and there seemed to be clear evidence of minor differences in the diagrams of attack-rates, especially in those of weekly attack-rates. Dr. King Kerr's other difficulty in regard to the strike in July—September, 1907, seemed also to disappear on close examination. Diagram 10 showed that there had been an increase of enteric fever in Belfast during the strike. The increase had not been great, but on the rough calculation he (Dr. Darra Mair) had made in his paper, it seemed, *a priori*, probable that in 1907 not more than 1 per cent. of shellfish eaters would be likely to suffer attack from fever; that is, at least 1,000 shellfish eaters in a given week would be required to produce some ten cases of fever. The concluding remarks of Dr. King Kerr showed that to him belonged the credit for the active measures commenced by the Belfast Corporation in 1906 to prevent the hawking of Lough shellfish, and that soon afterwards a marked decline of fever had begun and had continued. He (Dr. Darra Mair) would add that no facts had been adduced since the Commission's report was issued to shake his belief in the fundamental accuracy of the shellfish hypothesis which he had based on his study of the available data of the Belfast fever problem—and a most interesting study it had been—and he saw no reason why, if the consumption of Lough shellfish were thoroughly prevented, the decline of enteric fever in Belfast should not progress until, as an endemic disease, it became practically extinct there.

Epidemiological Section.

May 21, 1909.

Dr. A. NEWSHOLME, President of the Section, in the Chair.

Certain Considerations on the Causation and Course of Epidemics.

By JOHN BROWNLEE, M.D.

THE mathematical and biological theory of epidemics has hitherto been treated by few writers, but it is a priori a branch of the theory of chance, and the mathematics of the theory of chance once properly applied should afford a solution applicable to all epidemics. The ideas which underlie the theory of chance are, however, complex, and there are not many workers who have both the mastery of this theory and the necessary epidemiological knowledge to permit of its application. The results of a preliminary investigation into this subject I propose to lay before you to-night. It makes no claim to be more than a beginning, but as far as it goes it shows that the main laws which regulate epidemics are as simple as the law of gravitation, though the application of them to the diverse conditions which govern epidemics is often of considerable complexity. The chief difficulty arises from the fact that there is no means at present known of directly measuring the power of infectivity possessed by an organism. The value and variation of this power can only be discovered by making various assumptions, and testing the truth or error of these assumptions by the degree of correspondence which the results obtained on each assumption have with the actual facts. This is, of course, but the general method by which the theory of chance has been discovered, as when it is applied to games of chance or to coin-tossing where some hours of experiment settle at once whether theory and fact have any correspondence.

The epidemiological problem is twofold. In its one aspect it concerns the distribution of epidemic disease in space. That is to say, if an infectious disease be introduced into the midst of a uniformly populated district, its subsequent distribution necessarily follows some definite law. Practically a complete mathematical solution of this part of the problem has been given by Professor Pearson, and his result only requires modification to meet particular cases. It is purely an application of the theory of chance. The second part of the problem concerns the distribution of epidemics in time; that is, the manner in which the number of new cases varies from day to day, and it concerns the laws which regulate the ascent and descent of the epidemic curve. This is not a problem in pure chance; it involves a knowledge of the rate at which certain things vary. It must be obvious that one of three things causes an epidemic to die out. Firstly, the conclusion of an epidemic may be due to the exhaustion of susceptible persons among the population. In the second and third place, either a loss of infectivity on the part of the organism or of susceptibility on the part of the population is necessary. This part of the subject is biological and independent of the theory of chance, and will first be considered.

There are two ways in which the biological basis of epidemics may be ascertained: one is by observation of the mode of epidemic progress, either as it occurs in nature or in experiment; the other is by examination of the accumulated statistical information at our disposal. The latter at present lends itself much more easily to discussion. The fact which presents itself at once to our notice is the near symmetry of the form of the epidemic curve. If any typical epidemic of plague, as at present in India, or in former times—say that of 1665 in London—is examined, it is seen at once that as the disease increased, so it declined. That is even more fully seen in those directly infectious cases, such as small-pox and measles. The last large epidemic of small-pox in London in 1901-2 is a case in point. In fact, take any large solitary epidemic (by this phrase I mean an epidemic in which the level from which the disease rises is only a minute fraction of the epidemic height), and the symmetry of the course is an obvious and well-marked feature. The deduction from this phenomenon is direct and complete—namely, that the want of persons liable to infection is not the cause of the decay of the epidemic. On no law of infection which I have been able to devise would such a cause permit of epidemic symmetry. The fall must in

all such cases be much more rapid than the rise, though, on the contrary, when asymmetry is markedly present the opposite holds. We are therefore left to explain the cessation of the disease on the ground of loss of infectivity on the part of the organism, or of a decrease in susceptibility on the part of the population. In either case the form of the epidemic curve allows the rate of this loss to be more or less accurately measured. Dr. Farr already gave the solution as far back as 1868, and his results were somewhat extended in a paper to the Epidemiological Society in 1874 by Dr. G. H. Evans. The form in which, however, Dr. Farr gives his arithmetical law of the epidemic does not allow of the underlying cause directly appearing. It remains a law without a reason. A different form of analysis is necessary to discover this. But the result is not difficult to ascertain. If, as he says, the second difference of the logarithms of the successive ordinates of the epidemic curves is constant, then it directly follows that the loss of infectivity of the organism is approximately in the ratio given by a geometrical progression. That is, if the infectivity of the organism of the epidemic is m , and at the end of a unit of time mg when g is less than unity, at the end of a second unit of time it will be mg^2 , at the end of the third mg^3 , and so on. Dr. Farr seems to have chiefly considered the matter from the point of predicting the course of the epidemic, and there he is subject to special difficulties, for at best a law of this kind only expresses an average, and to make a prediction from the necessarily inexact figures obtainable at the beginning of an epidemic is somewhat futile. It may be taken, however, as certain that something approaching Dr. Farr's law represents the actual facts, and, with modifications to be mentioned presently, it can be made to describe the course of many epidemics with considerable accuracy.

It is very difficult to give evidence at all conclusive as to whether susceptibility or infectivity plays the greater part in determining the course of epidemics, but to my mind the state of the organism as regards its power of infecting is much the more important. The explanation which makes an epidemic end because the whole susceptible population has passed through an attack of the disease is obviously out of count, but a few facts even on this point will perhaps be of interest. In the winter of 1907-8 one of the largest measles epidemics in the history of Glasgow occurred. Public health authorities can at present do very little in the way of limiting the spread of measles, so that artificial measures had practically nothing

to do with the passing of the epidemic, yet it ceased exactly as epidemics are regularly seen to cease. So little, however, did the absence of susceptible persons account for the disappearance of the epidemic that even immediately after the disease had ceased we were admitting child after child to the hospital who had not passed through an attack of that disease. I have extracted from the histories of 263 children suffering from whooping-cough, admitted consecutively between June and September of last year, the details of their previous diseases. Of these, 137 are stated to have had measles at some previous date, but the remainder, numbering 126, to have not previously had an attack of that disease. These children were from the age of 1 year and 6 months up to the age of 9 years. Children under the former age were not counted, as they would have been below the age likely to have been infected during the time of the epidemic. Even at the high ages there were many children who had not had an attack of the disease. As regards the trustworthiness of these histories, I have usually found when dealing with cross-infection in wards that on the average they were fairly exact. This example of measles is as good as any. With regard to small-pox, it can hardly be said that out of the whole population of London in the year 1901-2 only 9,000 were susceptible to that disease. If this be the case, the fact seems to admit of only one interpretation, and that is that the infectivity of the organism is the chief factor in the course of an epidemic. It is easy to understand that a faculty acquired by an organism should be lost at a definite rate, as in its life-history the process of division completes itself every few minutes or hours. It is much more difficult to conceive that human or animal susceptibility should vary in such a manner that on December 1 the susceptibility to small-pox should possess exactly the same ratio to the susceptibility on January 1 as the latter possesses to that on February 1. The other alternative seems much the most probable.

At this stage it is perhaps well to illustrate theoretical and typical forms of epidemics. In the accompanying diagram I have drawn alongside in comparable scales four figures. In fig. A a diagram is given which would show the epidemic form if the disease died out simply from want of susceptible persons. It is markedly asymmetrical, the fall being much more sudden than the rise, which is in contradiction to common epidemic experience, as even when the epidemic is not symmetrical the decline is in general more prolonged than the rise. In fig. B is seen the form of the epidemic of small-pox in Boston, U.S.A.,

in the year 1721. This town at the beginning of the epidemic contained about 5,000 people not protected by a previous attack of small-pox, and of these 4,500 were attacked. This epidemic may thus be fairly considered as one in which the disease ceased because there was no susceptible material left, and though it does not accurately correspond to the previous diagram, yet it shows a considerable resemblance to it. This epidemic is, however, an almost unique example. In figure C

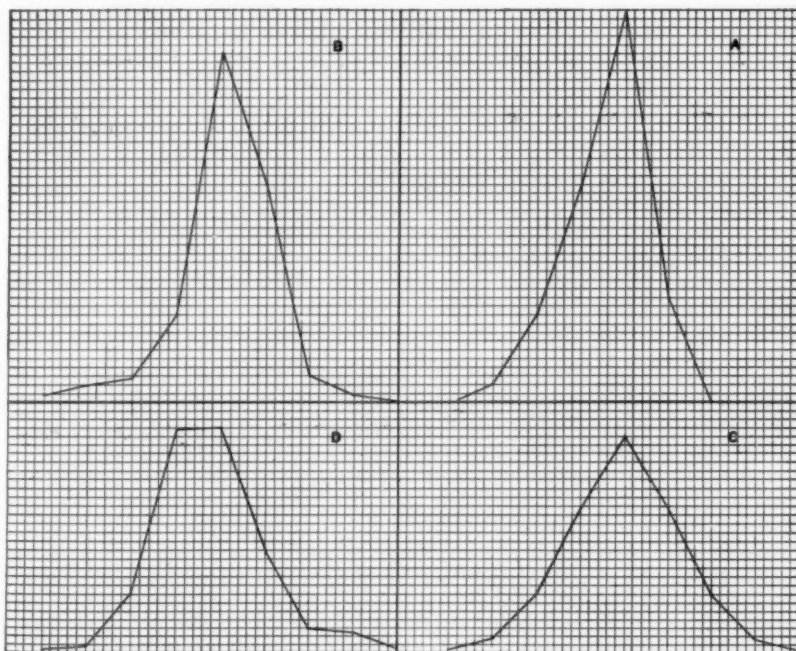


FIG. 1.

A, An epidemic constructed on the hypothesis that the infectivity remains constant and that the disease dies out from lack of susceptible persons; **B**, Epidemic of smallpox in Boston, U.S.A., in 1721, showing epidemic dying out, partly from lack of susceptible people; **C**, Epidemic constructed on the hypothesis that the infectivity decreases at the rate of geometrical progression; **D**, Epidemic of measles in Glasgow in 1808, showing that the decline of an epidemic tends to be longer than the ascent.

the form of epidemic is given which would result if the organism lost infectivity at the rate of the geometrical progression. It is symmetrical, as may be seen, and bears a very considerable resemblance to what is

commonly seen in epidemics. For comparison with this an epidemic of measles occurring in Glasgow in 1808 is given in fig. D. This epidemic can fairly be compared with that of small-pox just referred to, because measles had been absent from Glasgow at that date for a considerable number of years. There was therefore a large susceptible population, and, as the infectivity of measles is hardly less than that of small-pox itself, a similar form of epidemic might be anticipated. The curve, however, is quite unlike that seen in the former instance, and after the epidemic was over, to judge by the subsequent behaviour of measles in Glasgow, there were still plenty of susceptible persons left.

In stating as a law that the organism tends to lose its infecting power at a rate approximating to that of the geometrical progression, it is valuable to know that there are analogies in other parts of the animal and vegetable kingdoms. In the recent book by Professor Minot on growth and death, he shows that the rate of growth of many organisms decreases very rapidly in the early periods of life: thus a child grows in the first month after birth much more than in the second, and so on. In the same way, if the growth of an embryo be traced microscopically, many more cells in proportion to the total number of cells in the embryo may be seen undergoing the process of division in the early days of embryonic growth than in the later stages. It is to be noted that this decline in the rate of growth does not in the later stages of development proceed so rapidly as in the first stages.

The mechanism by which the power of infecting is lost has not been the subject of any assumption hitherto. As the infecting cells are continually dividing, it can easily be conceived that with each division something is lost. The most probable hypothesis is that the power of proliferation, enhanced in some way at the beginning of an epidemic, gradually decreases, and that as the organism loses its power of proliferation the epidemic dies out. As noted above, this loss of power does not in the embryo continue to decrease according to the geometrical ratio in the later stages, but remains at a higher level. Did the same law hold for infecting organisms as has been found to hold for the rate of growth of the embryos of the higher animals, the form which the epidemic would take would be a nearly symmetrical one, but one in which the number of cases at the tail was greater than that given did the law of geometrical ratio strictly hold. Examples of this will be seen later.

To prove directly that organisms vary greatly in infective power is somewhat difficult. The bacteriologist has hitherto confined his

investigations chiefly to the aspect of the virulence or lethal qualities of organisms. In the absence of direct experiment we are forced back on the accidents of infection observed in public-health or fever-hospital administration. As such accidents occur without any control of the conditions, it is often difficult to interpret them correctly and obviously impossible to have a doubtful point reinvestigated. Some facts, however, emerge, and with regard specially to measles the infectivity can be almost directly measured. Again and again patients admitted suffering from one particular disease are subsequently found to have been at the same time incubating measles, the latter infection making its appearance some days after admission to the ward. As measles possesses great infectivity from the beginning, and at the same time is of somewhat indefinite onset, there is as a rule ample time for a ward to become infected before the diagnosis is made. In spite of the fact that in many instances the case is removed on the first day of illness, cross-infection of the ward is very frequent. In the last eight years on thirty-seven occasions cases of measles have developed in wards containing a number of susceptible individuals. The table on p. 250 contains the record of these, and, in addition, the number of admissions to hospital of patients suffering from measles for each month. This number is a rough guide to the epidemic condition of measles in Glasgow. The letter N signifies that a case of measles has occurred in a ward and that no subsequent cases have developed. The letter I signifies that the ward became cross-infected. The meaning of this table is more easily seen when it is synopsised in the two subsidiary tables. In the first subsidiary table the manner in which the ward reacted to the infection of measles is shown in relation to the number of admissions per month, and in the second in relation to the period of the epidemic. It is seen that when these admissions are under fifty per month out of twelve instances infection occurred only four times, or in one third of the total. When the admissions were from fifty to 150 cases per month the condition is exactly reversed, while when the admissions were above 150 cases per month out of thirteen instances infection occurred in every one. In the next table the facts are grouped according as the instance occurred during the rise or decline of the epidemic or the inter-epidemic period, the first month after the crest of the epidemic being included in the rise. During the rise of the epidemic the organism is by far the most infective and least during the inter-epidemic period. One fact of special importance is seen when the last epidemic is considered. This epidemic spread with the greatest rapidity and involved greater numbers

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than any in the history of the Glasgow Sanitary Department. The organism was evidently specially infective, though not markedly virulent. On no occasion did a ward to which the poison was introduced escape infection, though we were always on the outlook for incubating cases, and in many instances the affected persons were removed from the ward on the first day of the disease.

TABLE I.—NUMBER OF ADMISSIONS FROM MEASLES FOR EACH MONTH FROM JUNE, 1901, TILL DECEMBER, 1908.

	1901	1902	1903	1904	1905	1906	1907	1908
January ...	—	52 I	5	4 ¹	32 N	137 N	9	432 II
February ...	—	15 I	13	12	24	68	11 N	207 III
March ...	—	10	19	44	65	89 I	77 I	145
April ...	—	20	13 I	30	141	84	9	64
May ...	—	45	13	27	165 I	68 N	24	35
June ...	82	46	35	10	131 I	69	20	24
July ...	128	18 I	54	11	13	47	8	16 N
August ...	85	12	47	6	53	18	13	17
September	40 N	— I	57 NII	12	42	13	29	5 N
October ...	43	—	101	27	60	7	154 III	8
November	48	10 N	165	34 N	99	2	253 I	—
December	55 N	3	148 I	34	72 I	9 N	342 III	—

¹ Outbreak of smallpox.

A letter is appended in each month indicating that a case of measles incubating was admitted into a ward. The letter N signifies that the ward was not cross-infected, the letter I that it was cross-infected.

TABLE IA.—TABLE SHOWING THE NUMBER OF TIMES A CASE OF SOME OTHER DISEASE INCUBATING MEASLES INFECTED OR DID NOT INFECT A WARD, ARRANGED ACCORDING TO THE NUMBER OF ADMISSION OF MEASLES CASES PER MONTH.

Number of admissions per month	...	0-50	...	50-100	...	100-150	...	150
Ward not infected	...	8	...	3	...	1	...	—
„ infected	...	4	...	6	...	2	...	13

TABLE IB.—TABLE SHOWING THE NATURE OF INFECTIVITY ACCORDING AS THE INCUBATING CASE WAS ADMITTED—DURING THE ASCENT OF THE EPIDEMIC, DURING THE DECLINE, OR DURING THE INTER-EPIDEMIC PERIOD.

	Ascent of epidemic	Decline of epidemic	Inter-epidemic period
Ward not infected	...	4	...
„ infected	...	24	...

The only other disease which spreads readily in the wards is chicken-pox. The consideration of this, however, loses much of its interest from the fact that there are no data by which the periods of the epidemic prevalence of this disease may be recognized. All that

can be said is that of thirty-six occasions on which the poison was introduced, the wards were cross-infected in twenty. The large number of failures to infect is interesting when it is noted how very infectious this disease sometimes is. The table suggests periods of special infectivity, but the number of instances is not sufficient, in the absence of exact knowledge of the epidemic cycles, to permit of conclusions being drawn. It is, however, to be noted that when the number of instances is summed up for each month of the year the winter months show a slightly larger proportion of infecting instances—a result in accordance with the fact that chicken-pox is usually more prevalent in these months.

TABLE II.—TABLE SHOWING, IN A SIMILAR MANNER TO MEASLES, THE NUMBER OF TIMES A PATIENT WAS ADMITTED INCUBATING CHICKEN-POX, WITH THE CORRESPONDING RESULT.

	1901	1902	1903	1904	1905	1906	1907	1908	Total infected	Total non-infected
January ...	N	—	I	—	—	II	—	N	3	2
February ...	—	N	—	—	—	—	—	—	—	1
March ...	—	NII	—	—	—	—	N	N	2	3
April ...	—	I	—	—	—	N	II	—	3	1
May ...	—	N	N	—	NI	—	N	—	1	4
June ...	—	—	—	—	—	—	—	N	—	1
July ...	—	—	—	—	—	—	NI	N	1	2
August ...	—	—	—	—	—	I	—	—	1	—
September ...	N	—	—	N	—	—	—	—	—	2
October ...	NII	—	—	—	I	I	—	I	5	1
November ...	—	NI	N	—	—	—	I	—	2	2
December ...	—	—	—	N	I	NI	NN	N	2	5
Totals, October to February ...						Infected		Non-infected		
„ March to September ...						12		11		
						8		13		

If the infectivity, then, is lost, the question comes to be, at what rate is it lost? As was seen earlier in the paper, Dr. Farr's original epidemic curve, when analysed, requires that the organism should lose its infectivity at a rate corresponding to that of geometrical progression. Assuming that this is the case, and making certain modifications which require for their development mathematical treatment which cannot be explained here, but which is being published elsewhere, a very close approximation to the form of many epidemics is at once obtained. The assumptions required are: (1) That by some means or other the organism acquires very high infecting power, possibly as the result of some pseudo-sexual process, and that this is lost at the rate above discovered; (2) that the infectivity thus acquired by individual organisms

varies around a mean; (3) that the period during which highly infective organisms are liberated occupies only a small portion, not exceeding one-fourth, of the total epidemic period.

As instances of this epidemic theory two diagrams are given. One of these shows the monthly number of deaths from small-pox in Warrington in the year 1743, the other a milk epidemic of scarlet fever in Glasgow in 1892. These instances have been fitted to the theoretical epidemic distribution which is deduced from the principles just referred to. In both the correspondence of the theoretical curve to the actual

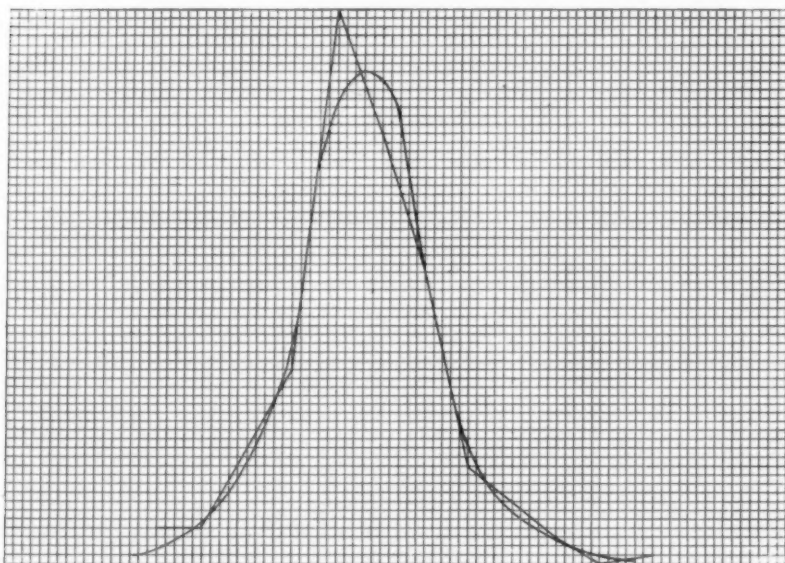


FIG. 2.

Epidemic of smallpox in Warrington, fitted to a theoretical epidemic distribution.

distribution is very close, and these two are but instances of many others. This affords to my mind a considerable amount of proof that the loss of infectivity at the rate of the geometrical ratio on the part of the organism is the main factor in the epidemic disease phenomenon. Of course, it may be equally well explained by a loss of susceptibility on the part of the population proceeding according to the same law, and I can offer no absolute proof that this is not the case. But either explanation relates to a strictly biological process. When we, however, consider

that epidemics are not confined to the lower animal and vegetable forms, but have many analogies among the higher forms, some support is afforded to the former of the two hypotheses. Plagues of locusts,

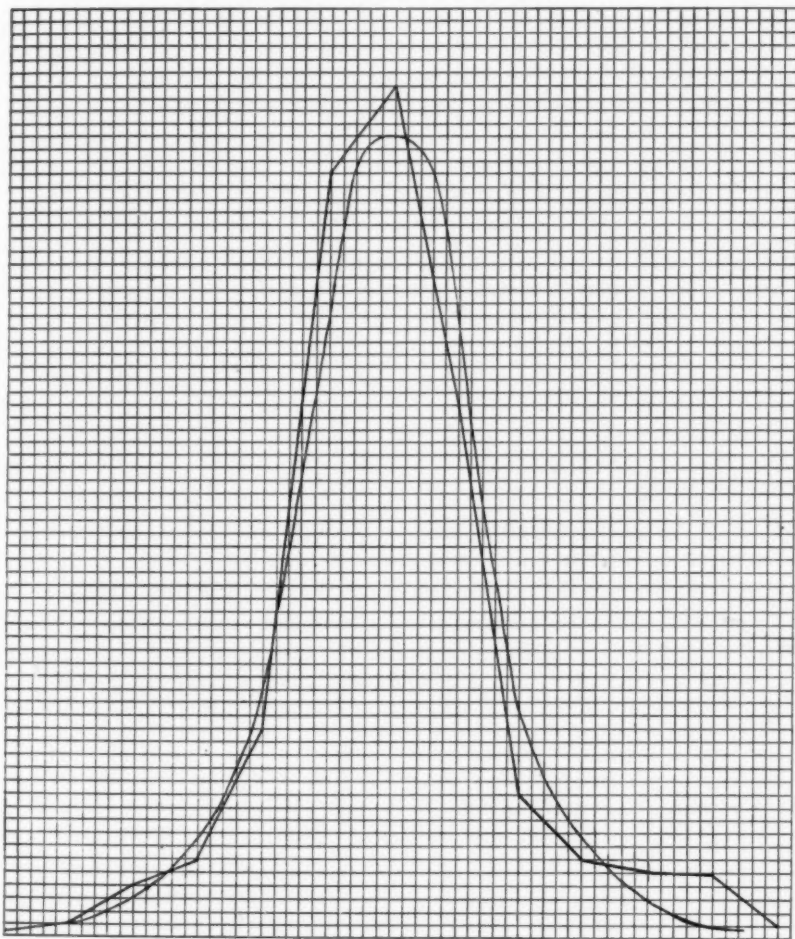


FIG. 3.

Milk epidemic of scarlet fever in Glasgow, fitted to a theoretical distribution.

flies, voles, &c., are well known, and I think that there is little doubt these are phenomena analogous to epidemics. With regard to plagues

of voles, it is recognized by the Commission appointed to inquire into the matter by the Government that for neither their appearance nor disappearance can any satisfactory explanation be offered; in other words, that some increase of fertility of unknown cause takes place, to be followed by a return of this to the normal level.

Epidemics are of two kinds—the seasonal and the solitary. A solitary epidemic is one which occurs at irregular intervals and which starts from a level of the disease, a mere fraction of that reached during the epidemic outburst. Of this type perhaps the most characteristic examples are small-pox and measles. Of the seasonal epidemics, scarlet fever and typhoid fever offer the best examples, and between these stand, as both seasonal and yet conforming to the definition of the solitary epidemic, such diseases as zymotic diarrhoea and plague. Of epidemics in general we have seen that symmetry is a prominent feature, but absolute symmetry cannot be expected of a solitary epidemic. In the case of measles, for instance, the period at which the large schools are invaded would necessarily have a perceptible effect in altering the course. With zymotic diarrhoea the infection might be present in abundance in the soil, and yet the means of conveyance be only irregularly in operation. However, when a composite curve is made for a series of years, it usually happens that the longer the average the more symmetry is attained. This is easily seen by the figures given in the Registrar-General's Annual Summary to be the case for deaths in scarlet fever, while the composite curve of notification from enteric fever for London is another example. Both these are figured in my previous paper, and the illustration need not be repeated here, especially as that for enteric fever is given in Sir Shirley Murphy's Annual Reports, which are in every one's hands. The degree of symmetry is in this case very remarkable.

When the curve of zymotic diarrhoea is examined, however, it is noted to be very markedly asymmetrical. The questions at once present themselves: Is zymotic diarrhoea a specific disease like plague or scarlet fever, or is it spread solely by one means? The answer must at present be doubtful. With regard to the first point, it may be remarked that if two diseases were due to nearly allied organisms the symptoms produced by each might be so closely allied that they would be difficult to separate clinically, or two forms of disease might be recognizable but not yet clinically separated because not looked for.

This difficulty of asymmetry of the epidemic form does not of course necessarily mean that two diseases are present. In the first place, if two

diseases or two modes of spread which act independently be postulated, then two symmetrical epidemics can be arranged to represent the greater part of which is commonly called zymotic diarrhoea. The proof must, however, be sought elsewhere. It is not enough to say that two curves both fulfilling the theoretical epidemic conditions, including symmetry, can be chosen to represent the asymmetric curve of the disease; that might well be an accident. If there are two causes, it is very unlikely that both will always act in the same manner. There should be years when each disease is present almost alone, and also years when both

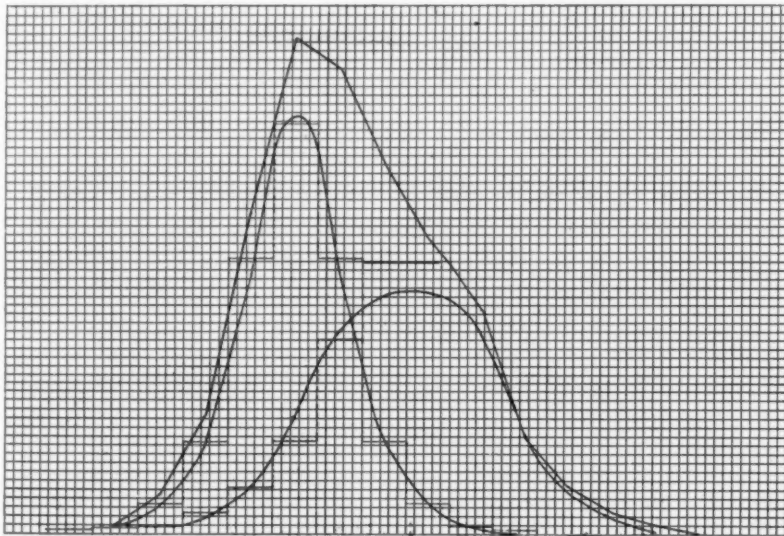


FIG. 4.

Average epidemic of summer diarrhoea in London 1850-1903, fitted tentatively to two theoretical epidemics, as described in the text.

occur together in varying proportions. A study of the disease from year to year should show this, and I think it does. In the accompanying diagram the epidemics of 1895, 1896, 1903, 1907 are given. The first consists almost solely of the early epidemic, the last two equally clearly of the second epidemic, while that of the year 1896 shows a combination of both. For those specially interested the diagrams of Dr. Peters given in his paper earlier in this session may be consulted. Many of the years can be analysed at a glance, and it will be seen that in many years one or

other type predominates. In 1895 almost perfect symmetry of the course is seen. In 1896 the same characteristic is manifest for some time after the crest of the wave has been reached till the rise of the second epidemic is seen, while in the epidemics of 1903 and 1907, both representing the second type, the same symmetry is noticeable. The first epidemic seems to culminate about the thirty-first and thirty-second weeks of the year, and the second from the thirty-seventh to fortieth. The variation in each case is, however, greater than this, and in some cases the first epidemic, being late, runs almost concurrently with an early second epidemic. These considerations help to account for a good many of the vagaries of zymotic diarrhoea. With regard to the clinical side, that there are two diseases I have little evidence to offer. The year 1905 was, however, a year of much zymotic diarrhoea in Glasgow. Both epidemics were present, with a quite distinct interval between them. Dr. David Dickie, who was in charge of the Sick Children's Hospital that summer, says that the August cases were of a quite different type from those at the end of September. The former were more chronic, with evidence of enteritis; the latter more acute, and death ensued in a much shorter time from acute diarrhoea and collapse. With regard to the spread of diarrhoea by flies, the limited evidence at one's disposal is not sufficient to permit of dogmatism, but a few general considerations may be stated. It can hardly be expected, in the first instance, that flies and diarrhoea will have a linear relationship; that is, that they will be in direct proportion. When the number of flies reaches a certain point, a maximum efficiency in causing diarrhoea must be reached. If there are 500 flies in a room it is not probable that chance of infection is twice as great as if there were only 250, unless the percentage of flies causing infection is very small. What relationship or what law it should express is quite unknown. No such case has yet been measured. Dr. Niven's figures for Manchester for the years 1904, 1905 and 1906, however, show some degree of linear relationship between flies and diarrhoea. In that of 1905, for instance, the proportion of flies to diarrhoea deaths is as is given in the following table:—

VALUE OF $\frac{\text{NUMBER OF FLIES}}{\text{DIARRHOEA DEATHS X100}}$ WHEN THE DATES OF ONSET OF ILLNESS ARE TAKEN FOR EACH CASE OF DEATH FROM DIARRHOEA.

Week ending		Week ending	
June 17	1.5	Aug. 12	1.4
" 24	1.8	" 19	1.4
July 1	1.5	" 26	1.6
" 8	2.3	Sept. 2	2.2
" 15	1.1	" 9	4.3
" 22	1.3	" 16	3.2
" 29	1.2	" 23	3.7
Aug. 5	1.3	" 30	3.4

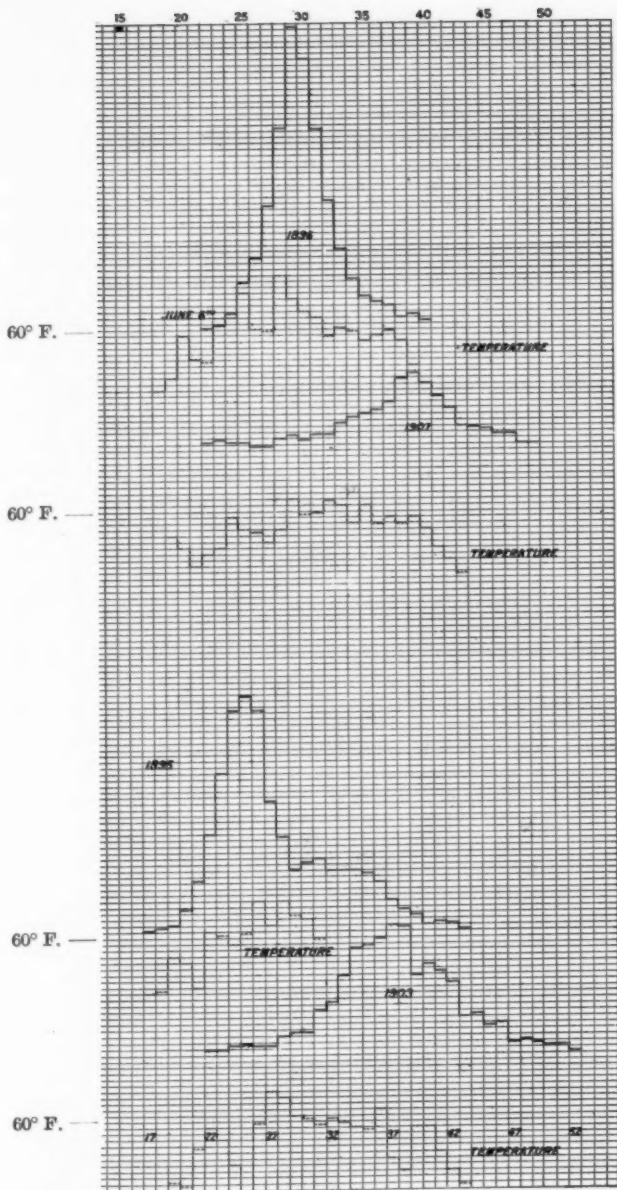


FIG. 5.

Four epidemics of summer diarrhoea in London, charted so as to correspond week by week. To each epidemic the appropriate weekly mean temperature for the year is appended.

Here the principal point which must be noted is that it is at the tail of the epidemic a much larger proportion of flies seems to be necessary to produce a case. This indicates that even if the flies be the carriers there is a want of infective material to convey by the time the epidemic is spending its force. It can hardly be that the flies are much more lethargic in September than they are in June, the temperature of these two months not differing to any great extent. If the diagram of the course of summer diarrhoea in 1895 and 1896 is examined, it will be found that the epidemic in both instances began to fall while the weekly mean temperature of the air was well above 60° F. Dr. Hamer's elaborate investigation into the relationship of flies and diarrhoea in London might be taken also to show that the house fly bred in the dunghill was responsible for the second wave of the disease, while the house fly from other sources was responsible for the primary wave.

Dr. Niven gives no figures for 1907, but it would have been interesting if it could have been noted whether the experience of Manchester coincided with that of London for that year. In spite of the large numbers of flies present during the whole summer, the diarrhoea epidemic was very late in appearing, and its fall preceded that of the fall of the flies. Considering all the facts, it seems to me probable that there are two forms of zymotic diarrhoea, that both tend to run a symmetric course independently of each other, and that though the carrier of the infectious agent be flies, there is a potent factor in the condition of the agent itself.

The Problem of Marital Infection in Pulmonary Tuberculosis.

By M. GREENWOOD, Jun.¹

To determine the relative importance of infection and predisposition in the genesis of tubercular lesions is a matter of such interest to all concerned in public-health problems that I have taken this opportunity of laying before the Section some results which, although not in themselves decisive, may provoke discussion among those qualified to advance our knowledge.

In a recent memoir [16] the late Dr. Pope and Professor Karl Pearson have published analyses which tended to show that there is evidence that the concurrent existence of pulmonary tuberculosis in husband and wife cannot be explained wholly by the fact of assortative mating in the community. Yet the values of the statistical constants obtained indicated unusually great exposure to the chance of infection to be of less genetic importance than the existence of a predisposition to react unfavourably to those means of contamination which cannot under ordinary circumstances be avoided. Among the rich material analysed by Pope and Pearson were some sets of the sanatorium figures published by the Kaiserliches Gesundheitsamt; since Pope and Pearson's memoir was written the eighth part of the "Tuberkulose Arbeiten" [21] has appeared, and contains the statistical data of other sanatoria, together with a summary of previous work. It seemed to me desirable to reduce the further data and to see how far the constants obtained tallied with those published by Pope and Pearson. Table I contains the correlation coefficients calculated for each sanatorium not dealt with by Pope and Pearson, together with those obtained when all the patients in the public sanatoria were grouped together. I determined the coefficients by the approximate method (Q_5) [14], but some of the more striking values were recalculated by the exact fourfold process, and I find, in agreement with Brownlees, Elderton and Pearson [14], that the approximation is quite sufficiently accurate for material of this kind, when the values of h and k are not large. Thus, the Görbersdorf female cases gave for A by the true fourfold method 0.62 as against 0.60 by the approximation, and the total males, A and B , gave 0.10 and 0.22 as against 0.11 and 0.23.

¹ From the London Hospital Statistical Laboratory.

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The "Tuberkulose Arbeiten" classify the affected parents of sanatorium inmates into "probably" and "certainly" tuberculous, and I have calculated r_s with three groupings of the material in each case, viz.: (A) The probably tuberculous are grouped with the certainly tuberculous. (B) The probably tuberculous are grouped with the not-affected. (C) The probably tuberculous are excluded.

TABLE I.—TABLE OF THE VALUES OF THE COEFFICIENT OF CORRELATION FOR THE EXISTENCE OF PULMONARY TUBERCULOSIS IN HUSBAND AND WIFE (GERMAN SANATORIA).

Source of data	Total number of pairs	Number of doubtful pairs	Correlation coefficients ¹		
			A	B	C
(1) <i>Public Sanatoria for Males:</i>					
Weicker-Görbersdorf	2,577	216	0.23	0.40	0.38
Dannenfels	116	23	0.39	0.62	0.59
Lüdenscheid	734	46	0.08	0.18	0.16
Engelthal	199	2	-0.28	-0.26	-0.26
Albrechtshaus	212	12	-0.52	-0.32	-0.35
Waldhof Elgershausen	268	15	-0.04	-0.19	-0.16
Verschiedene Anstalten	669	31	-0.02	+0.06	+0.04
(2) <i>Private Sanatoria for Males:</i>					
Brehmer-Görbersdorf	457	2	0.41	0.40	0.40
St. Blasien	252	17	-0.35	-0.07	-0.10
Verschiedene Anstalten	116	6	0.36	0.42	0.41
(3) <i>Public Sanatoria for Females:</i>					
Vogelsang	706	12	0.04	0.09	0.08
Weicker-Görbersdorf	1,354	57	0.17	0.26	0.24
Cottbus	280	40	0.17	0.57	0.53
Verschiedene Anstalten	241	12	0.45	0.54	0.53
(4) <i>All Cases in Public Sanatoria (inclusive of data reduced by Pope):</i>					
All males	14,997	927	0.11	0.23	0.21
All females	3,927	187	0.12	0.22	0.19
(5) <i>Private Sanatoria for Females:</i>					
Brehmer-Görbersdorf	239	2	0.60	0.62	0.62
St. Blasien	136	5	0.26	0.38	0.32
Verschiedene Anstalten	88	5	0.08	0.27	0.25

¹ The values given under A were obtained when the "probably" tuberculous were grouped with those "certainly" affected; under B are the values yielded on grouping the "probably" affected with the not-affected; and under C the values got on excluding the "probably" affected altogether.

It will be noticed that the values for the last two groupings agree well, but that those for the first set differ markedly from the other two. This fact emphasizes the necessity of caution in drawing conclusions from imperfectly grouped material. An examination of the individual results reveals very considerable differences in the values obtained from various institutions. I do not, however, attach much importance to this for several reasons. In the first place, some of the

most striking figures are derived from sanatoria in which one or more of the groups contained few cases. A good illustration is Albrechtshaus, which gives the paradoxical value of -0.52 . As a matter of fact, there is only one pair both members of which are described as tuberculous, and the probable error of this return is ± 0.67 , so that we can evidently attach no importance to the coefficients. In the second place, apart from the very important question of the large probable errors of the sub-groups, an examination of the age distributions of the inmates and their scheduled trades in the different institutions suggests that although in the main the patients come from the same social stratum and are of comparable ages, yet that there may be some individuality in the institutions. I have not, however, been able to extract sufficient information from the statistics, which were not of course collected *ad hoc*, to warrant any definite conclusion, and regard the point as of secondary importance. When we turn to the coefficients under Table I (4) the figures are in much better accord and agree well with the value obtained by Pearson from a table comprising the whole of Pope's data (consisting of 41,786 cases), viz., 0.17 [16, p. 17]. The question now arises as to how we can deduce from these results a real measure of the degree of infection between husband and wife. The coefficients as they stand do not provide this measure, since they are obtained from a highly selected population, each family including at least one tuberculous child. If we know definitely that there is no such thing as hereditary predisposition the fact of such selection would not be of so much importance, although even in that case we should have to remember the possibility that the parents might be infected by the children. If, on the other hand, some degree of inheritance be allowed, the importance of the non-random character of the sample is great.

Pearson, by the help of a beautiful analysis which should be closely studied by all interested in exact statistical methods [16, p. 17], has shown how to deduce the coefficient of correlation for the general case, given three *quæsitæ*: (1) The correlation for the selected population; (2) the correlation between parents and offspring; (3) the prevalence of phthisis in the whole population. It is, I think, a matter of extreme difficulty to select appropriate arithmetical values for each of these conditions.

Under (1) ought we to use the A, B or C coefficient? The agreement between B and C is close, but either gives values much in excess of A. Since we have no means of determining what really constitutes a "probably" tuberculous subject, we cannot say what error is introduced

by grouping such a person as definitely affected. I think, however, that the agreement of B and C suggests that the difficulty is not really avoided by excluding the doubtful cases altogether, and that, in so doing, we are really carrying out an illegitimate process of selection.

With respect to (2), we have the very definite results of Pearson [15], based upon relatively few but well-authenticated figures. How far, however, the coefficients he obtained measure true inheritance, and how far infection from parent to children, must be discussed later; for the moment it is sufficient to recognize the existence of a measurable relationship between parent and child in respect of the character of having the disease, a relationship measured by a coefficient which cannot fall much below 0.45 and may rise to over 0.62, in accordance with the condition we assume.

I believe (3) represents the crux of the immediate problem. In the absence of general compulsory notification of phthisis, an urgently needed reform, our estimates of the prevalence of tubercular disease in the population at large must be extremely uncertain. Pearson [15 and 16] is of opinion that not far short of 10 per cent. of the whole population of these islands are affected by pulmonary tuberculosis, and goes so far as to suggest that in Germany the proportion may be still higher. Our President [12, p. 63 and p. 101], as the result of a careful study of the available evidence, is of opinion that estimates based on the mortality figures exaggerate the actual prevalence. As he points out, however, the figures he publishes depend on the truth of hypotheses which are not definitely established. I have not been able to find anything with a decisive bearing on the question. The statistics of the Krankenkassen ought, one might think, to solve the problem, but it is impossible to determine accurately the age distribution of members, and furthermore the classification of diseases in various towns is not uniform and the individual figures differ enormously. I quote the following table from Prinzing's work; he specifically mentions that at Vienna "ist Tuberkulose überhaupt mit Einschluss von Skrofulose gemeint."

TABLE II (PRINZING) [17, p. 116].—NUMBER OF CASES OF PULMONARY PHTHISIS (LUNGEN-SCHWINDSUCHT) PER 100 MEMBERS OF THE KRANKENKASSE.

Town		Males		Females	
Frankfurt-on-Main	...	0.21	...	0.17	...
" "	...	0.55	...	0.38	...
Vienna	...	1.0	...	1.2	...
"	...	4.6	...	4.2	...
Breslau	...	0.39	...	0.21	...
Bodenheim	...	0.33	...	0.42	...

Able to work
 Unable to work
 Able to work
 Unable to work
 "
 "

Unsatisfactory as are these figures and such others as I have seen, I cannot help thinking that 10 per cent. is in excess of the real prevalence of pulmonary tuberculosis, and that even 1 per cent. may be too high.¹ I am at present attempting to evade the difficulty by collecting all the data relating to the parents of adults or to adults more than 60 years of age admitted to the London Hospital, in so far as they give with reasonable accuracy the cause of death, or state of health, if living. Owing to great faults in the clinical histories, some avoidable, others—the majority—inevitable, it is a work of time to obtain sufficient data for analysis, but my colleague, Dr. Douglas White (to whose collaboration in the arithmetical work of this paper I am much indebted), and I, hope to report fully on the results later. A “general hospital population,” as I have attempted to show elsewhere [5], differs markedly in many ways even from the urban working-class population from which it mainly comes, but its uses, at any rate for comparison, are considerable. I next turn to the results obtained when various arithmetical values are assigned to (1), (2), and (3). First of all I determined for the data of Table I [4] the coefficients for an unselected married population,² assuming the correlation between parent and offspring to be 0.46 and taking (a) 1 in 5, (b) 1 in 10, (c) 1 in 15 of the population to be tuberculous. The results are tabled in Table III. Taking the mean coefficient of assortative mating in man to be 0.24 both for physical and psychical characters (Pearson, Weldon, Lee, Schuster and Elderton) [16, p. 22], we should infer that there is an appreciable but very slight degree of interconjugal infection, that this is a relatively inconsiderable factor in the genesis of the disease.

¹ Were there no question of infectivity, the death-rate for phthisis would give us the prevalence at once, and 10 per cent. would certainly not be an under-estimate. Under the heading of deaths from phthisis, however, will be included, in addition to cases of relatively chronic disease, rapidly fatal cases—*e.g.*, pneumonic phthisis and terminal infections. What we require to know is, I think, the prevalence of phthisis of the relatively chronic type such as furnishes the majority of sanatorium patients. Such cases with a duration from the onset of definite symptoms of between, say, one and six years might be regarded as possible centres of efficient infection, which is hardly the case when the duration of life from onset is a matter of weeks. A lower limit to the proportion of such cases is 1 in 273, the number estimated by Newsholme [12, p. 69] as occurring in the population at any given time. An upper limit cannot be given until we know the percentage of total phthisis deaths to be attributed to rapidly fatal and terminal infections. A point which requires careful statistical inquiry is the proportion of infective cases which recover and do not figure in the death returns of phthisis at all. Since the success of curative measures is known to depend on the stage at which treatment is initiated, these cases may not, perhaps, largely affect our problem.

² This expression must, however, be used and interpreted with great caution: *see* Pearson and Pope, *op. cit.*, pp. 20-21.

TABLE III.—COEFFICIENTS FOR AN UNSELECTED MARRIED POPULATION, DEDUCED FROM TABLE I (4), ASSUMING THE CORRELATION BETWEEN FATHER OR MOTHER AND CHILD WITH RESPECT TO THE EXISTENCE OF PULMONARY TUBERCULOSIS TO BE 0.46.

Coefficient used (see Table I)	Coefficients for the unselected population		
	(1) If 1 in 5 of the whole population is tuberculous	(2) If 1 in 10 is affected	(3) If 1 in 15 is affected
<i>Males:</i>			
A	...	0.26	...
B	...	0.36	...
C	...	0.34	...
<i>Females:</i>			
A	...	0.27	...
B	...	0.35	...
C	...	0.32	...

I then endeavoured to form some idea of the change produced by supposing a very much lower percentage of tuberculosis in the population. I rearranged Pearson's male pedigrees on the basis of 1 per cent. tuberculosis in the population and found, by the approximate method,¹ the correlation between parent and offspring to be 0.68, so that one would probably not be erring in the positive direction by taking 0.6 for the real value on this basis. The other constants necessary for determining the correlation in the general case were then calculated by Pearson's method, and finally I obtained, using the three values for males in Table I (4), 0.39, 0.48, and 0.47. I do not attach any special importance to these particular values, but they show how necessary it is to arrive at some really satisfactory understanding as to the prevalence of phthisis in a population before we dogmatize as to the relative parts played by infection and predisposition.

On the whole, it is fairly clear both from the work of Pearson and Pope, and from my own confirmatory results, that conjugal infection is probably a fact, but the steps by which this conclusion is reached may well give rise to discussion. The most obviously debatable point is, How far does the parental correlation, whether we take it to be 0.46 or 0.68, really measure the inheritance of a predisposition; how far does it simply measure infection from the parents? That, among the working classes, in view of the comparatively high modal age of incidence of phthisis, direct pulmonary infection is not an important factor may, as Pearson suggests [15], be considered probable. Indeed, even in the case of adults closely segregated some of the positive evidence is of

¹ As a test of the accuracy of the approximation applied to the Crossley material, I recalculated the correlation (by the approximate method) from Pearson's table as it stood and obtained 0.60, the correct value given by Pearson being 0.59 [15, p. 12].

doubtful value. Hamer [6] has, in my opinion, completely demolished the alleged proofs furnished by Cornet from the statistics of the Catholic nursing orders in Germany. This, however, does not settle the point, even if we could disregard the evidence adduced by Newsholme [12, p. 146]. We have to pursue the chances of infection *ab ovo usque ad mala*. Among those who hold the view that prenatal or congenital infection is of paramount importance in apparently inherited tuberculosis, there has been some difference of opinion as to the relative dangers of paternal and maternal infection. Klebs [9], for instance, chiefly on the strength of some tolerably complete pedigrees, has inferred that paternal infection is ten times as dangerous as maternal disease. We have indubitably strong evidence, both clinical and experimental, that intra-uterine infection does occur. Bugge [3], Schmorl and Kockel [20], and Schmorl and Birsch-Hirschfeld [19] have described cases of tubercular disease of the placenta, and foetus in parturient subjects of the disease, the tubercle bacillus having been detected in the lesions.

In animals, Johné [8], Malvoz and Brouwier [11] have found tubercular lesions and bacilli in the foetal liver, both in the cow. Sanchez Toledo [18], on the other hand, not to speak of other observers, inoculated thirty-three pregnant guinea-pigs, intravenously, intrapleurally, or subcutaneously, with virulent cultures of the tubercle bacillus, and an elaborate examination of the sixty-six offspring of these animals failed to reveal bacilli or lesions. Sanchez Toledo, while fully accepting the positive results of Johné, Malvoz and Brouwier, lays stress on the extreme rarity¹ of congenital tuberculosis, and points out that the infection, when it does occur, shows no signs of latency, a contention strongly supported by the observations of Malvoz and Brouwier, and upholds the famous contention of Koch [10]: "In my opinion, hereditary tuberculosis finds its most natural explanation if we admit that it is not the infective germ which is transmitted, but certain peculiarities which favour the development of the germ when it is subsequently brought into contact with the body; this is what one calls predisposition." With regard to the question of latency, however, we must bear in mind the evidence collected by Newsholme [12, p. 74].

In favour of paternal infection the evidence is less convincing. Aubeau [1] claims to have found the tubercle bacillus in human semen, but the observation would not appear to be decisive. In animals

¹ Schmorl and Geipel, however (*Munch. Med. Wochenschr.* 1904, p. 1676), found distinct proofs of placental infection in nine cases out of twenty examined. They hold placental infection to be *relatively* common.

TABLE III.—COEFFICIENTS FOR AN UNSELECTED MARRIED POPULATION, DEDUCED FROM TABLE I (4), ASSUMING THE CORRELATION BETWEEN FATHER OR MOTHER AND CHILD WITH RESPECT TO THE EXISTENCE OF PULMONARY TUBERCULOSIS TO BE 0.46.

Coefficient used (see Table I)		Coefficients for the unselected population				
		(1) If 1 in 5 of the whole population is tuberculous		(2) If 1 in 10 is affected		(3) If 1 in 15 is affected
<i>Males:</i>						
A	...	0.26	...	0.27	...	0.27
B	...	0.36	...	0.37	...	0.37
C	...	0.34	...	0.35	...	0.35
<i>Females:</i>						
A	...	0.27	...	0.28	...	0.28
B	...	0.35	...	0.36	...	0.36
C	...	0.32	...	0.34	...	0.34

I then endeavoured to form some idea of the change produced by supposing a very much lower percentage of tuberculosis in the population. I rearranged Pearson's male pedigrees on the basis of 1 per cent. tuberculosis in the population and found, by the approximate method,¹ the correlation between parent and offspring to be 0.68, so that one would probably not be erring in the positive direction by taking 0.6 for the real value on this basis. The other constants necessary for determining the correlation in the general case were then calculated by Pearson's method, and finally I obtained, using the three values for males in Table I (4), 0.39, 0.48, and 0.47. I do not attach any special importance to these particular values, but they show how necessary it is to arrive at some really satisfactory understanding as to the prevalence of phthisis in a population before we dogmatize as to the relative parts played by infection and predisposition.

On the whole, it is fairly clear both from the work of Pearson and Pope, and from my own confirmatory results, that conjugal infection is probably a fact, but the steps by which this conclusion is reached may well give rise to discussion. The most obviously debatable point is, How far does the parental correlation, whether we take it to be 0.46 or 0.68, really measure the inheritance of a predisposition; how far does it simply measure infection from the parents? That, among the working classes, in view of the comparatively high modal age of incidence of phthisis, direct pulmonary infection is not an important factor may, as Pearson suggests [15], be considered probable. Indeed, even in the case of adults closely segregated some of the positive evidence is of

¹ As a test of the accuracy of the approximation applied to the Crossley material, I recalculated the correlation (by the approximate method) from Pearson's table as it stood and obtained 0.60, the correct value given by Pearson being 0.59 [15, p. 12].

doubtful value. Hamer [6] has, in my opinion, completely demolished the alleged proofs furnished by Cornet from the statistics of the Catholic nursing orders in Germany. This, however, does not settle the point, even if we could disregard the evidence adduced by Newsholme [12, p. 146]. We have to pursue the chances of infection *ab ovo usque ad mala*. Among those who hold the view that prenatal or congenital infection is of paramount importance in apparently inherited tuberculosis, there has been some difference of opinion as to the relative dangers of paternal and maternal infection. Klebs [9], for instance, chiefly on the strength of some tolerably complete pedigrees, has inferred that paternal infection is ten times as dangerous as maternal disease. We have indubitably strong evidence, both clinical and experimental, that intra-uterine infection does occur. Bugge [3], Schmorl and Kockel [20], and Schmorl and Birsch-Hirschfeld [19] have described cases of tubercular disease of the placenta, and foetus in parturient subjects of the disease, the tubercle bacillus having been detected in the lesions.

In animals, Johne [8], Malvoz and Brouwier [11] have found tubercular lesions and bacilli in the foetal liver, both in the cow. Sanchez Toledo [18], on the other hand, not to speak of other observers, inoculated thirty-three pregnant guinea-pigs, intravenously, intrapleurally, or subcutaneously, with virulent cultures of the tubercle bacillus, and an elaborate examination of the sixty-six offspring of these animals failed to reveal bacilli or lesions. Sanchez Toledo, while fully accepting the positive results of Johne, Malvoz and Brouwier, lays stress on the extreme rarity¹ of congenital tuberculosis, and points out that the infection, when it does occur, shows no signs of latency, a contention strongly supported by the observations of Malvoz and Brouwier, and upholds the famous contention of Koch [10]: "In my opinion, hereditary tuberculosis finds its most natural explanation if we admit that it is not the infective germ which is transmitted, but certain peculiarities which favour the development of the germ when it is subsequently brought into contact with the body; this is what one calls predisposition." With regard to the question of latency, however, we must bear in mind the evidence collected by Newsholme [12, p. 74].

In favour of paternal infection the evidence is less convincing. Aubeau [1] claims to have found the tubercle bacillus in human semen, but the observation would not appear to be decisive. In animals

¹ Schmorl and Geipel, however (*Munch. Med. Wochenschr.* 1904, p. 1676), found distinct proofs of placental infection in nine cases out of twenty examined. They hold placental infection to be *relatively* common.

tubercular lesions were induced by Jäckh [7], who injected testicular substance obtained from phthisical persons. Recently, Friedmann [4] has reinvestigated the whole subject. He found in the literature twenty-two cases of tubercular infection by way of the placental circulation, and in twelve of these definite anatomical lesions were present; in thirty other cases such a mode of infection seemed probable. There were, however, only two recorded cases in which the fœtus was tuberculous while the male parent alone was diseased. Friedmann injected bacilli into the vas deferens and testes; in both cases, if the animals were allowed to copulate within four weeks, bacilli were found in the resulting embryos. Intra-pulmonary injections gave negative results. It will be seen that while this kind of evidence renders the existence of congenital and prenatal infection probable, it affords no support to the views of Baumgarten [2]. In the absence of much more accurate statistics of prenatal and infantile pathology than we possess, it would be rash to hold that a congenital infection remaining latent for, on the average, fifteen to thirty years is at all a frequent event.

Lastly, we have to take into consideration the non-pulmonary tubercular diseases. These conditions are universally admitted to prevail extensively in childhood, and abundant proof is furnished by, for instance, Newsholme [12] and Prinzing [17]. I am not acquainted with any distinct statistical proof that non-pulmonary tubercle is more frequent among the offspring of phthisical persons than in the general population, but this is generally believed to be the case, and I will assume it to be true for the purposes of my argument. It is to be remarked at once that this greater frequency would only prove that infection is more important than predisposition if it could be shown that the excessive incidence only falls on children brought up with their parents, not on those segregated shortly after birth. The only possible way of settling this point would be by collecting data regarding the children adopted by the Poor Law or Education Authority and comparing them with those obtained from the corresponding class of the population.¹ The notorious difficulty of obtaining satisfactory family histories in this class may well dash the ardour of the statistician. We cannot, therefore, attribute decisive importance to one or other factor in this connection, but it is of interest to see what effect an increased percentage of offspring affected with non-pulmonary tuberculosis would have on our phthisis statistics.

¹ It is important to remember that such children form not a random sample but selection.

The after-history of the tuberculosis of childhood has not been made the object of a sufficiently complete statistical inquiry. Either (1) the majority die, or (2) they survive with chronic tuberculosis, or (3) they recover completely. If (1) is true, we are exactly in the same case as before regarding pulmonary tuberculosis, *i.e.*, without strong evidence that the factor of parental infection is specially important; but (2) and (3) give us more trouble. If the survivors of infantile tuberculosis exhibit an enhanced liability to suffer from phthisis, then the number of phthisical offspring of phthisical parents will be increased by persons whose condition *may* be due to parental infection during the years of childhood.

But we have no evidence that this is true; in fact the probabilities seem rather the other way. It would seem to be more in accordance with our modern notions of immunity to suppose that survival from a non-pulmonary tuberculosis would have some prophylactic value in respect of subsequent pulmonary infection. Should this be the case we are confronted by a paradoxical state of affairs. In virtue of (let us suppose) parental infection in childhood, a percentage of the children of tuberculous parents are less easy to infect with pulmonary tuberculosis than the general population of children. The tendency of this would be to diminish the number of phthisical children of phthisical parents and thus to lower the correlation determined from an adult sample. Of course, even if this hypothesis be accepted—and I put it forward with the utmost diffidence—the ratio of children who have recovered from infantile tubercle to the rest of the children is, even in the families of tuberculous subjects, perhaps too small to affect our correlation values appreciably. My object in troubling you with this long discussion is simply to put forward the suggestion that, in the present state of knowledge, it is difficult to believe that the parental correlation of between 0.46 and 0.68 for pulmonary tuberculosis is not a measure of inherited predisposition rather than of parental infection.

In view of the important gaps in our statistical evidence it seems to me imperative to reserve final judgment. Our statistical data are as yet altogether inadequate. To mention one obvious blot, we are compelled to make classifications into phthisical and not-phthisical, while in order to solve our problem of infection *versus* predisposition we must obviously make some distinction between cases with and without expectoration. It cannot seriously be disputed that efficient infection, as an experimental fact, depends on the dosage of infective material, and that, from this point of view, separation of our statistical data into two classes is not adequate.

More than forty years ago, at a time when belief in the heritability of tuberculosis was wider spread than now, Oesterlen [13, p. 403] judiciously pointed out the need of caution in interpreting supposed statistical proofs. Even now, when one reads the very confident pronouncements of the laity, both affirmative and negative, one appreciates the force of Butler's remark—"What a wonderful incongruity it is for a man to see the doubtfulness in which things are involved, and yet be impatient out of action or vehement in it."

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DISCUSSION.

The PRESIDENT (Dr. Newsholme) said the Section could congratulate itself on having had two papers which, although highly technical in character, had been very lucidly set forth. He would ask Dr. Ransome, who many years ago had done pioneer work in the subject of Dr. Brownlee's paper, to open the discussion.

Dr. RANSOME said it was embarrassing to be called upon to speak on a subject without having studied it before. But he considered both the papers very interesting. As the President had remarked, he had contributed papers to the old Epidemiological Society on the cause of epidemics and on the cause of the shape of the curve or epidemic wave. In that he had occasion to call attention to Dr. Farr's theory as to the gradual loss of power of transmission of disease after the lapse of a certain time and after it had gone through the bodies of a certain number of susceptible persons. He pointed out, however, that according to Pasteur the poison might, at certain points, be reinvigorated by being passed through the bodies of very young animals. And he thought he was able to show, in the diagrams which he brought before the Society at that meeting, that there were points in which that had actually taken place. When a number of years and the average of those years was taken one crossed out those little irregularities, but those little irregularities, the ripples on the waves, were themselves instructive; and he thought they pointed to the effect not only of that passage of the poison through very susceptible creatures, but also to the influence of external causes, such as the atmospheric changes. He once took the trouble to find out what would be the shape of a curve if one regarded an epidemic as a spark passing through some tinder paper, the tinder being likened to susceptible persons. The shape of the curve depended very much on the shape of the community through which the epidemic was speeding. The ordinary oblong-shaped community and that of towns were much of that shape, and would give a curve almost exactly like that of Dr. Farr—*i.e.*, it would spread at first rapidly and rise to a point, and then come down just as gradually and symmetrically. Dr. Brownlee had shown that in different epidemics there remained a very large population of susceptible persons who had never been attacked, and that therefore the curve could not be accounted for simply in the way which he had just explained. He thought, with Dr. Brownlee, that there must be something inherent in the germ and in the gradual taking away of virulence from it by its passage through the bodies of susceptible persons. That was the theory upon which Dr. Farr framed his formula. He remembered well the cattle-plague epidemic, and Dr. Farr's first contribution took the form of a letter to *The Times*. The curve followed the course which Farr forecasted in that letter. That was one of the first things which directed his (Dr. Ransome's) attention to epidemiology. Dr. Brownlee did not dogmatize on the point, but said there must be something to cause the epidemic to die out.

Dr. BUTLER said Dr. Brownlee's thesis was one which, on the face of it, explained much in the features of epidemics. But he thought something more in the way of definition was wanted of what was meant by "power of infectivity" than the author gave. They were asked to fall back on the variations of infectivity of the organism as explanatory of the course of epidemics. The infectivity of an organism was not anything existing *per se*, but was a correlate between the power of the organism to infect and the reaction in the host. It did not matter what degree of infectivity might be ascribed to an organism as measured by some constant if there were continuous variations in those who were infected. In other words, susceptibility was as much a measure of infectivity on the one hand as infectivity was of susceptibility on the other. He had considerable difficulty in understanding what Dr. Brownlee meant by the words, "The conclusion of an epidemic may be due to the exhaustion of susceptible persons among the population in the first place. In the second and third place, either a loss of infectivity on the part of the organism or of susceptibility on the part of the population is necessary." Loss of susceptibility on the part of the population seemed to be much the same as the exhaustion of susceptible persons. The exhaustion of susceptible persons was the same thing as the acquirement of insusceptibility on the part of the population itself; and that seemed one of the things which had to be borne in mind, because acquired susceptibility was one of the causes of the decline of an epidemic. Constantly, in scarlet fever, diphtheria, influenza, and small-pox, one found many cases presenting mild symptoms of those diseases. One would scarcely be justified in saying that those represented attacks. It was highly probable that they represented an invasion of the particular infection sufficient to create in those who came within range a degree of insusceptibility to an attack of the disease. And constantly in an epidemic one had diffusing themselves and protecting the population those modified attacks which created in the population a number of persons who were then susceptible to the disease. It was called the exhaustion of the soil. There was an acquired immunity keeping pace with the spread of the disease itself. It seemed also that in applying such an abstract mathematical method to the measurement of epidemics one had to make so many estimations, which corresponded to nothing found in the occurrence of epidemics, that the utility of it was most questionable. In order to get such a curve as that, which would represent the exhaustion of all susceptible persons, one had to assume an equally susceptible population exposed equally to an organism of constant infectivity. That was a condition which was never met with. Constant infectivity was obviously a thing which was not met with in practice, and still less so was constant susceptibility, because when such a disease as measles, for instance, was introduced into a family, certain members of the family would fall with it, and others would escape. These variations in incidence of attack might be accounted for by differences as to previous attack, age, constitution, or even hereditary liability to the infection: all those complex factors entered into the matter. Dr. Brownlee particularly excluded the influence of the medical officer of health and the other preventive agencies

which were at work in controlling epidemics; but when one considered the last epidemic of small-pox in London, it would be a large draft on their credulity to believe that that epidemic of small-pox merely represented phases in the infectivity of the small-pox poison. They did know that small-pox was directly controlled as a result of the application of preventive measures which were a limitation purposively placed upon diffusion and upon the susceptibility of exposed persons—factors which seemed to have been quite ignored in Dr. Brownlee's paper.

Dr. E. F. BASHFORD said: The purpose of Dr. Greenwood's paper appears to be to assert that inherited or natural "predisposition" and "resistance" are at least of as much importance in determining the occurrence of tuberculosis as exposure to infection. These conclusions are stated with veiled caution only; since Dr. Greenwood scarcely suggests the unreliability of his primary data, and does not hesitate to push home the practical consequence of manipulating them mathematically as if they were absolutely reliable. If I am correct in the interpretation I put on the paper, then the result of the mathematical treatment of the data contradicts the results of experiments on animals, and those direct observations on man, which have aroused the conviction that tuberculosis is dangerous to the community mainly or solely because it is infectious. This contradiction appears to me to be of such serious import as to call for an emphatic protest from someone engaged in the experimental study of disease. I wish to emphasize that, of course, my criticisms do not refer to the mathematical method, but only to the value of the primary data on which Dr. Greenwood's conclusions are based. I recognize fully the existence of natural resistance and natural predisposition, although I attach more importance to the modifications they undergo under environment. I recognize fully the value and importance of statistical or mathematical treatment of biological data. Dr. Greenwood assumes the validity of his data, and I shall confine my remarks to a criticism of them. A statement as to the occurrence of tuberculosis in the parents of tuberculous patients is taken to be a criterion of inborn liability or resistance. This assumption appears to me to be quite unjustifiable, especially when also no attempt whatever is made to assign a value to environment and to dosage of infective material, both of which experiment has shown are factors of more moment than resistance. Any accurate meaning attaching to the terms "resistance" and "predisposition" has been conferred upon them by experiments made on animals. When applied to the human subject as interpretations of why individuals have acquired or escaped an infective disease, they become merely modern equivalents of the lore of ancient medicine, and for the most part have as much value as old wives' tales of this or that ailment "being" or "not being in the family." The practice of asking patients, before formulating diagnoses, "Has any other member of the family suffered from the same disease?" I regard as a survival of ancient superstition. The question is, as put, justified for the most part as little by knowledge of what constitutes scientific evidence of hereditary taint as it is actuated by accurate knowledge of the laws of infection. Nevertheless, the negative and

affirmative answers of patients play to-day an enormous part in influencing the diagnosis of, *e.g.*, tuberculosis or cancer. Under the experimental conditions applicable to the study of communicable disease "resistance" and "predisposition" have acquired more precise significations, but they have lost correspondingly in importance. They have been shown to apply to purely relative quantities. Their influence is restricted to producing slight variations above and below the average resistance revealed by inoculating large numbers of comparable animals of the same species with known quantities of the infective material. In the beginnings of bacteriology "resistance" and "predisposition" were frequently evoked to explain anomalous results. In the beginnings of experimental cancer research they played an equally prominent part. Sir John MacFadyean emphasized the insignificant part played by "resistance" in the case of bovine tuberculosis during a recent discussion before the Royal Society of Medicine. I may illustrate it for cancer by pointing out that certain carcinomata of mice which Ehrlich found were only able to overcome natural resistance in 1 out of 500 inoculations, we found able to overcome it in 100 per cent. of the inoculations. I need hardly add that little has since been heard of the hypothesis Ehrlich based upon his deductions as to the importance of natural resistance in this particular case. In short, we must recognize to-day that the importance assigned to "resistance" and "predisposition" diminishes in direct proportion with the accuracy and uniformity of the experimental conditions themselves. Under suitable conditions "predisposition" and "resistance" may be allowed for and even neglected. As a matter of fact, they are ignored in the employment of experimental (animal) tests for purposes connected with public hygiene—*e.g.*, in the standardization of antitoxins and in the testing of milk for the presence of the tubercle bacillus. The nature and the dosage of the inoculated materials alone enter into consideration in such determinations. In estimating the importance attaching to natural resistance or predisposition to disease the last word rests of necessity with the experimenter. We all admire the brilliance of the hypothetical mathematical deductions which have been made in physical and chemical science, and which have been subsequently tested and proved to be true by direct observation and experiment; but, be it noted, the final tribunal before which hypothesis, theory, and truth have been respectively established has been the appeal to experiment. I do not suppose that any one of those present to-night is prepared to invert the relative importance of mathematical deduction and experimental proof when applied to problems of biology. In interpreting statements as to the occurrence of tuberculosis in the parents of patients as records of inborn predisposition or resistance, I hold that Dr. Greenwood goes beyond his data. Further, the problem before us to-night is so formulated that an experimental investigation of its validity would require to be made on the human subject, and this is impossible. Although insoluble in the form stated, this problem has been long since restated by pathologists in a way which has rendered the objective investigation of the importance of the "soil" possible. The solution arrived at is devoid of ambiguity, as I have already said. The mere

repetition, by pathologists, of experiments to demonstrate the relative unimportance of the "soil" and the prime importance of dosage of infective agents and of environment would be an unjustifiable undertaking. It rests not with pathologists to prove again that the results of experiment can be applied to man, but with Dr. Greenwood to produce an experimental demonstration of the truth of what he deduces mathematically, or an experimental refutation of the view that, from the standpoint of active measures in the interests of the public health, the germ is emphatically of more importance than the "soil."

Before leaving the pathological basis of the paper I should like to draw attention to the one-sided nature of the literature quoted on what we loosely call congenital tuberculosis. The true state of our knowledge on this subject is not apparent from the narrative given in the paper. The significance of congenital tuberculosis is so fully elucidated that the facts do not justify the use made of the statements quoted.¹ I should like further to draw attention to the relation which obtains as regards fatal and latent or obsolescent tuberculosis in the experience of the post-mortem room. This relation is well shown in the following table given by Harbitz,² based on the post-mortem examinations made in the Rigshospidala, Kristiania. The curve of deaths due to tuberculosis rises for each quinquennial age period up to 25, where the deaths from tuberculosis were 46 per cent. of all deaths; thereafter the curve falls uniformly and rapidly to a minimum between 46 and 50, there being a subsequent rise between 60 and 70. The curve of latent and obsolescent tuberculosis shows exactly the opposite relation: it is lowest at 16 to 25, being then only 23 per cent. of total deaths, rises rapidly to 40 per cent. between 31 and 35, 60 per cent. between 55 and 60, and 77 per cent. between 66 and 70. It seems to me that in Harbitz' investigations and others like them we have additional evidence of the importance attaching to a long latent period in tuberculosis and to the study of *completed* lives. I do not find that this is taken account of by Dr. Greenwood, nor is there any evidence adduced as to the duration of wedded life or the proportion of widows and widowers. I now come to the validity of the actual data utilized. This is the kernel of the whole argument, since it is assumed that other pathological data I consider to be of prime moment in estimating the significance of the incidence of tuberculosis may be entirely overlooked, or, at any rate, need not be allowed for at all. The only data employed are the numbers of parents recorded as suffering from tuberculosis. These data show the most marked fluctuations not only from one institution to another, but as between the parents of the male and the female patients.

Patients in public institutions	Father		Mother	
	Tuberculous	? Tuberculous	Tuberculous	? Tuberculous
Parents of 14,997 males	... 13.2	... 17.2	... 9.1	... 11.7
	per cent.	per cent.	per cent.	per cent.
„ 3,927 females	... 21.0	... 23.8	... 15.9	... 18.2
	per cent.	per cent.	per cent.	per cent.

¹ For a recent discussion of this subject cf. Albien, "Untersuchungen über intra-uterine Tuberkulose Infektion," *Zeitschr. f. Tiermed.*, 1909, xiii, 2.

² Harbitz, "Untersuchungen über die Häufigkeit, Lokalisation und Ausbreitungswege der Tuberkulose," *Videnskabs-Selskabets Skrifter I.*, Math. Naturv. Kl., 1904, 8, Kristiania, 1905.

The excess of tuberculosis in the parents of the female patients means either an over-statement in their case or an under-statement in the case of the males. It is notorious that women generally take more interest in family history than men, and probably this is the explanation here, for we find the same variations in the data referring to the husbands and wives of the patients themselves. (These data are not used by Dr. Greenwood.)

		Tuberculous		? Tuberculous
Wives of 7,701 males	...	3.2 per cent.	...	3.6 per cent.
Husbands of 781 females	...	12.0 "	...	5.7 "

The latter figures are also, no doubt, influenced by the greater care devoted to the health of the husband, and by the fact that at marriage women drop out of the purview of the Krankenkassen. As regards different institutions, there are also variations, especially marked in the case of the husbands and wives of the patients, varying from no statement at all and from 1.5 per cent. to 10.9 per cent. in the case of the data for the certainly tuberculous. It is obvious that the data must be most imperfect. I, however, go further: they are also employed without regard to the restrictions the compiler himself has placed upon their utilization. It is pointed out in the report from which the figures are taken, "The preceding figures show a relatively very low proportion of males suffering from tuberculosis, chest disease, or other illness, as well as of children suffering from tuberculosis, chest complaint, scrofula, or other illness. It is necessary, in the first place, to take into consideration that they only give information as to the health of members of the family, up to a definite point of time—namely, up to the commencement of institutional treatment. Further, they have only the value of lowest estimates, since *they are based entirely upon the statements of patients.*" The italics are in the original. Dr. Greenwood's data have little more than the value of hearsay statements, and it is a pity that he has considered them worthy of the very careful mathematical analysis to which he has subjected them. If the value of the data is so small, Dr. Greenwood has built upon them a mathematical house of cards, and, until he adduces something more cogent against the primordial importance of infection, those of us who have to work experimentally at disease will continue to place our faith in the results of methods requiring only the simplest arithmetic for their interpretation.

Professor KARL PEARSON, F.R.S., said his chief remarks would turn on one point in Dr. Greenwood's paper, about which he hoped to have heard more discussion—namely, the proportion or percentage of those subjects to pulmonary tuberculosis in the population. The President was cited as being the authority for using 1 per cent. He took it that the President meant by that percentage the number existing at any one time in the population. But the proportion really needed was an entirely different one—viz., the number which were recorded as having suffered from tuberculosis in, if possible, the completed lives of a family record. That was a far larger number than were suffering from tuberculosis at any one time. If a person at 25 years of age said that one or

other of his parents had suffered from it, he had probably under consideration a period of some thirty years during which that parent might have suffered from tuberculosis. Granting on the average, as Dr. Newsholme had done, a three-year infectious period, it would be clear that the modal thirty years corresponded to ten sets of cases, or that the value, 10 per cent., adopted by him, was precisely that which flowed from Dr. Newsholme's 1 per cent. of current tuberculosis. The best way of judging of the number of people who during their lifetime suffered from tuberculosis was to judge of the number who died from it. That must be a minimum of the number who during their life had suffered from tuberculosis. In this country, he believed, the proportion was roughly 10 per cent., in America 11 per cent., and in Germany higher still. In a number of records of middle-class families collected by Professor Pearson, where a large proportion of the younger members had not yet passed right through the danger zone, the tuberculous cases amounted to 5.5 per cent. to 6 per cent., showing that in the course of life a much larger number than 1 per cent. not only ran the risk of being tuberculous but did not escape the danger. Thus his percentage did not differ substantially from the President's 1 per cent. if they remembered that it applied to the number who some time during their life had suffered from tuberculosis. Everything which suggested that the records were taken at random really tended to weaken correlation rather than to strengthen it. The relationship between parent and child, even if one took that between father and child, amounted to 0.46 to 0.52 at a minimum; or, if Dr. Newsholme's 1 per cent. were taken, it came to the extraordinarily high value of 0.68. Now, the relationship of husband and wife was much more intimate than that of father and son, yet infection amounted to about 0.33, or, on the view of 1 per cent., to about 0.40. But why should the figures show such a more intimate relationship between father and son than between husband and wife if there were no constitutional factor? Far more figures were doubtless needed before coming to a dogmatic conclusion. Still, he thought something in the inherited constitution was really required to account for the drop between those two classes. There was another striking point, upon which also more light ought to be thrown—viz., if the tuberculous 0.33 was the measure of infection between husband and wife, why should we find almost the same value for the resemblance of husband and wife when studying their relationship in insanity? The figure in this latter case was 0.32. Should they assert that insanity was also an infectious disease? That would be for medical men to settle; but this resemblance was in itself very significant. Elaborate statistics from sanatoria and hospitals for diseases of the chest were very much wanted; they ought to be furnished in as uniform a way as possible. It was surely desirable to ascertain whether, after all, there was not some constitutional influence, as well as infection, at the root of the disease. At present he had more than a hundred new pedigrees of tuberculosis, and this was the sort of material which required to be enormously increased by very careful investigation as to relatives. He could not conceive a more useful expenditure than an expenditure on looking at death certificates of the parents.

and relatives of tuberculous people, if the position was really valid that one cannot accept what anybody says as to the causes of their relatives' deaths. All he suggested at present was that there might, after all, turn out to be something of importance with regard to the soil, and that they could not quietly brush on one side such a great difference as there had been shown to be between the infection factor in husband and wife and father and son. He laid more stress on the relation between father and child than on that between mother and child in this matter, although there was little difference in the values of the statistical constants.

Dr. DUDFIELD said Professor Karl Pearson had raised many points of great interest. During the last few months he (Dr. Dudfield) had had the opportunity of learning something of the value of family histories, in connexion with a dispensary for tuberculosis which had been recently opened in Paddington. On close inquiry being made into the family histories, it was surprising how many people were found to be tuberculous when medically examined, in whom the disease would not otherwise be suspected. He asked what was the criterion of proof in regard to the presence or absence of tuberculosis in husbands and wives. He felt that there was a large proportion of the population who were tuberculous, but did not know it. With regard to death certificates, valuable as they were for statistical purposes, he thought that under present circumstances they were apt to prove somewhat misleading. To those who had business with such certificates it was known that a fair proportion of the deaths of persons, the subjects of phthisis, were recorded under some other heading. Therefore, the search among death certificates, alluded to by Professor Pearson, would not give him all the information he wanted. More information was required as to the family history over lengthened periods, and that could only be got by long-continued observation of the patients and their families—far more than could be given by any sanatoria. In Edinburgh for the past twenty years they had been keeping in close contact with patients, and in the case of one patient who had been attending they traced seventeen tuberculous relatives, not one of whom had any idea he or she was diseased.

Mr. UDNY YULE said he thought he could best occupy his time by putting in simpler form the problem put by Dr. Greenwood, avoiding coefficients of correlation. He would take the table given in the memoir by Dr. Pope and Professor Pearson. The first four lines of that table were said to refer to the general population, by which he thought was only meant the parents of patients in general hospitals. Of the wives of tuberculous husbands, 34 per cent. were tuberculous; of the wives of the non-tuberculous, 13 per cent. Taking the next five lines in the same table, regarding the parents of non-tuberculous offspring, 16 per cent. of the wives of tuberculous husbands were themselves tuberculous, and only 6 per cent. of the wives of non-tuberculous husbands. The corresponding percentages, derived from the remainder of the table, for parents of tuberculous patients were 19 per cent. and 1 per cent. In each case there was a considerable excess in the percentage of the tuberculous amongst the wives of tuberculous husbands. That suggested infection; but, as Professor Pearson

pointed out, similar figures were found where infection could hardly be assumed—for example, in insanity—and the same sort of result was obtained in cases other than disease. That was the puzzle. It was all very well to say that the data might be bad—everybody recognized that they might be bad—but why should there be such similar results in the two cases? Professor Pearson gave another very interesting table in the memoir to which reference had been made, showing the similarity between the stock of the husband and wife. The result was that if the wife's stock was called tuberculous when there was at least one case of phthisis noted amongst any of her near relatives, and the husband's stock tuberculous when there was one case of phthisis amongst his near relatives, there was the same sort of similarity between the stock of the husband and the stock of the wife, when infection could not have come into play as between husband and wife, where there was believed to be infection. In fact, when the husbands' stock was tuberculous 40 per cent. of the wives' stocks was tuberculous; when the husbands' stock was non-tuberculous, only 17 per cent. If there was infection in the one case and not in the other, why were the results so similar? He would like to add that Dr. Greenwood's conclusions did not appear to him so dogmatic as some speakers were inclined to think, but seemed to him a very impartial summary of the data.

Dr. BULLOCH said that as an experimental pathologist he took up a position midway between Dr. Bashford and the statisticians. He wished to remind the Section of the history of tuberculosis doctrines. At the end of the eighteenth century everyone believed that it was highly infective, and that people and their effects ought to be disinfected. In the first quarter of the nineteenth century, and largely through the enormous influence of Laennec, the infectious hypothesis came to be doubted, and in its place was set up the doctrine that consumption depends on some constitutional diathesis, people showing this diathesis or disposition being prone to contract the disease. Within a quarter of a century of Laennec's death so eminent a clinician as Sir Thomas Watson maintained that the disease "cannot be imparted even by one scrofulous individual to another. The disease is not spread by contagion." In 1867, however, that acute and original observer, Dr. William Budd, of North Tawton, brought forward irrefutable evidence that tuberculosis is a zymotic disease of specific nature and "the tuberculous matter is (or includes) the specific morbid matter of the disease and constitutes the material by which phthisis is propagated," a view in complete harmony with the discoveries of Villemin and Koch. As a result of Koch's discovery, inquiries were set afoot on a very large scale in Germany, Austria, England, and America to determine the views of practical medical men on the subject. The results were extraordinarily disappointing. Few could affirm or deny the infectious theory. This is perhaps not surprising when one remembers that the date of the infection cannot be determined. The endemic character of tuberculosis in all civilized countries also renders the tracing of an individual case an impossibility. Lastly, there is the question of predisposition, for in all infections it cannot be doubted that there are two factors—the exogenic virus and the soil on which it lives. It seemed to him

that, so far as man is concerned, the facts with regard to tuberculosis were by no means so certain as Dr. Bashford asserted, and he (Dr. Bulloch) considered that the lines along which Professor Karl Pearson and Dr. Greenwood worked were extremely hopeful in solving a problem on which the pathologist had not been able to shed much light.

Dr. F. M. TURNER pointed out that a symmetrical epidemic curve would result if it is assumed that each case of disease remains permanently infective and the disease were to go on spreading until all the available material were used up. For the number of cases infected would bear a constant ratio to the chances of contact between a diseased and a healthy susceptible person. At the start there might be 10 per cent. affected and 90 per cent. unaffected: the chances of contact would be 10 by 90. The condition would be quite symmetrical at the fall of the outbreak when 90 per cent. had had the disease and 10 per cent. remained to be susceptible: the chances of contact would be 90 by 10. It is certain that such permanent infectivity does not take place in measles or small-pox, but not so certain in the case of scarlet fever. The infectivity rate of patients discharged from London hospitals after scarlet fever was about 3 or 4 per cent. What the rate would be for early cases was not known, but there is much evidence to indicate that it is not very high. Apart from the above special case he was willing to accept Dr. Brownlee's proof from mathematical considerations that ordinary epidemics did not cease for want of material, but from some other cause, such as diminution of infectivity. Many other considerations supported the above hypothesis. For instance, the endemic existence of scarlet fever, with its yearly rise and fall, could not be explained on the former assumption; nor could the number of persons in London susceptible to small-pox possibly be so low as 9,000, the actual number infected in the last epidemic. Nor could Dr. Turner accept Dr. Butler's argument that the vigorous measures taken by medical officers of health and others were the chief or even an important cause in the cessation of an epidemic. For after the first few cases, when perhaps the outbreak takes the authorities by surprise, the vigour of preventive measures is just as great during the period of rise as later during the fall. Some years ago scarlet fever rose heavily every summer, and each summer for several years the isolation hospital failed to accommodate all the cases sent there. According to the theory that it was isolation among other measures which stopped the epidemic, when the hospitals could take no more, the rate should have gone up, but it actually went down every year the same as before.

Dr. HAMER said he sympathized with Dr. Bashford in his desire to know, however it might affect the correlation value, something more about the raw material upon which Dr. Greenwood had worked. He believed the forms which had been employed by the German sanatorium officials, and upon which the particulars had to be written in, allowed of the insertion of only two or three words upon this complicated and difficult question of family history. When one reflected that the information thus supplied was afterwards analysed and tabulated under some thirty different headings in the "Tuberculosis

Arbeiten," one realized that every stroke of the pen of the man compiling the return was, as Carlyle would have said, "significant of much." It was, moreover, not only a question of quantity but of quality also; for the main point which had to be determined was the very difficult one as to whether or no the patient's parents had suffered from a disease, now described and known by a name connoting all sorts of things, which no one dreamed of when the said parents were alive. There was, in fact, so little room for explanation and so much room for doubt as to the significance of the entries made that he (Dr. Hamer) was prompted to ask Dr. Greenwood, concerning his beautiful method of inquiry, the question which Mr. Babbage was asked by a young lady to whom he had exhibited his famous calculating machine—viz.: If you put the question in wrong, will the answer come out right? He (Dr. Hamer) desired to say a word, too, with regard to environment. Dr. Greenwood spoke of the problem as one of infection *versus* predisposition. But were not both alternatives closely bound up with environment? Taking predisposition first, this might, of course, be either inherited or acquired. If it was acquired (and Koch and many others attached little importance to inherited predisposition), the question of environment became all-important. Or, again, take infection. If infection were communicated by food, or by the operation of some unknown environmental agency, such as an intermediate host, environment again became all-important. It was instructive to contrast commonly accepted views concerning tuberculosis with those relating to a disease like typhoid fever. If one took any of the theories current with regard to the spread of typhoid fever, one would have to reckon, in discussing incidence upon husband and wife, with that factor of co-environment. Professor Pearson had told them, moreover, that in such a condition as insanity co-environment played an important part. No one dreamed of attaching much importance to hereditary predisposition or to sputum infection in typhoid fever, but in that disease a high correlation value might easily be brought about by co-environment. If that were true of typhoid fever, why should the statisticians, in dealing with tuberculosis in husband and wife, limit themselves to the questions of hereditary predisposition and direct infection, and take no note at all of what some of those who were present that evening deemed to be wellnigh all-important—namely, co-environment?

The PRESIDENT (Dr. Newsholme) said both papers were extremely interesting and valuable, and he was sure all would join in thanking the authors for them. Dr. Greenwood had stated his case extremely well; he had not been dogmatic nor come to definite conclusions, as he had realized that the weight of the evidence would not bear such conclusions. The real difficulty, as in nearly all similar investigations, was with the data. These were almost hopelessly defective. In order to reach any correct conclusion with regard to marital infection, one would need to know three sets of facts: (1) At what date after marriage one of the partners became tuberculous; (2) for how long a time afterwards the marriage connexion continued; and it was, thirdly, of the utmost importance to know the whole of the subsequent life-history of the surviving

partner. The third factor was, he believed, absent from the figures with which Professor Pearson worked, and it was certainly absent from the figures with which Dr. Greenwood worked. Therefore any conclusions based upon such imperfect data must be tentative; and, in fact, it was doubtful if any conclusion was justified, particularly if it ran counter to pathological and medical knowledge. Such an investigation took a microscopical view of the subject, and it was necessary to take also a macroscopical view in order to arrive at the truth. Dr. Bashford had brought that out in a very forcible manner. Experimental evidence made it clear that one could produce tuberculosis in animals by infection, and that in a given species of animals one rarely found very great variations of susceptibility or resistance, and that the occurrence of the disease was determined chiefly by the dosage of infection and by the duration of those doses. Furthermore—and that was a point which should commend itself to those who, like Dr. Bulloch, seemed somewhat incredulous as to infection being the main factor—they should look at veterinary experience. With certainty tuberculosis could be eliminated from a herd of cattle, and they could be kept free from it, not by diminishing susceptibility or increasing resistance, but by removing the affected members of the herd, by disinfecting the cowsheds, and by preventing the importation of fresh infected animals. He maintained that the history of tuberculosis in this and other countries was quite consistent with the view that as the opportunities for human infection had decreased, so had the total mass of human tuberculosis declined. The diminution of overcrowding, and the better housing of the people, had acted in a large measure by diminishing the opportunities of infection; and, still more, the enormous increase of the institutional treatment of the poorest class of advanced consumptives in infirmaries, &c., had been an important factor in the same direction. In the light of pathological and epidemiological experience, mathematical deductions would require to be based on data which were both complete and accurate before they could successfully controvert the conclusion to which all other lines of evidence converged.

Dr. OWEN PETERS wrote as follows: In the course of his valuable paper Dr. Brownlee has made certain references to a paper read by me at the beginning of this session, and has also touched upon various points there discussed, with regard to which I should like to make some further remarks, after acknowledging the obligation which all interested in epidemiological work must feel they are under to Dr. Brownlee as practically the first to institute a thorough investigation into the principles governing the rise and fall of epidemic curves. It is interesting to see that he has advanced a step further than in his first paper and has admitted an alternative explanation of the behaviour of the typical curves—the possible influence of decrease of susceptibility of the population. It seemed difficult to bind down the whole question of epidemic explosions to one phenomenon alone—that of alteration in the infectivity of the organism. Dr. Brownlee has not, however, given a hint as to the possible nature or cause of the suggested decrease in susceptibility of the population; as to whether, *e.g.*, in diseases with annually recurring epidemics it is due to a

periodic variation in the resistance offered by the general population. But apart from this there still seems to be a considerable part played by complete or partial exhaustion of susceptible persons, at any rate in individual houses or groups of houses, in the case of diseases such as measles and diarrhoea, which so often make a clean sweep of all before them. These limited explosions, although small, must yet have some influence upon the composite curve for a whole town, and to that extent, and perhaps from quite a number of other causes, the curve must be expected to be asymmetrical. Again, if the reader of the paper is rightly understood, he divides the whole population by a sharp line into susceptibles and insusceptibles. But the question arises, Is not immunity largely a comparative matter, the chance of attack depending on the relative highness of infectivity over the highness of the resistance offered, there being, theoretically speaking, few persons who would not succumb to a sufficiently virulent attack? In that case a dividing line is out of the question, or its position depends wholly upon the relative strength of the conflicting forces. When, then, an epidemic outburst occurs in a limited population, it might be supposed that the disease would first advance very rapidly owing to the plentiful material provided by the more susceptible persons. But as these are weeded out the progress amongst the less susceptible becomes progressively slower and more difficult. The outbreak, however, will not collapse here, for with the large amount of infective material now present, and the gentle gradient up to the still more insusceptible cases, it is still able to gather in a few but gradually decreasing number of these, the epidemic curve so produced tailing out to nothing in the gradual fashion of the theoretical curves exhibited. If, however, the virulence of the particular strain were very great, or the susceptibility very low, the whole of a limited population might be attacked, and in that case the curve would certainly have an abrupt fall. Again, where the epidemic potential was only sufficient to bring about the attack of a part of the population, for the strength of that particular strain it would still be legitimate to say that the epidemic in question ceased from the lack of susceptible persons. With regard to the suggestion as to the dual character of the diarrhoea curve, I have for some time been keenly following up the possibility of a double hump in the epidemic curve pointing to the existence of two allied diseases. The question, however, for lack of more particular facts, must be viewed on broad lines, for quite a number of diseases present this tendency. The epidemic curve of measles in England and cholera in Bengal form a curious parallel in this matter, both having a double hump produced apparently by the same cause, the indentation due to the arrival of mid-winter. In both the first hump is sometimes lacking, while at other times the second hump is lacking. It was suggested in the paper that an occurrence something like this in diarrhoea was evidence of the presence of two diseases. But there are other reasons, which I cannot enter into now, for believing that these are not double diseases, any more than plague, scarlet fever and diphtheria, which also exhibit similar appearances. It is true that diarrhoea in hot, long summers abroad does present this appearance in one or two instances, but in British records, of

which I have studied several hundreds from the various large towns, no doubling of any account was found which could not be referred to a second upward movement of the temperature. As regards diarrhoea, there are two things of which I feel perfectly convinced: firstly, that both in small foci and in large towns a constant inherent tendency to an explosive curve of a certain symmetrical form is found; secondly, that the influence of temperature, however, exerts a powerful control over the form of the curve, distorting it greatly or, even if sufficiently low, suppressing the epidemic wave altogether (as in Edinburgh, 1907). The second rise in the London curves for 1895, 1900, and 1892, and that of Glasgow for 1905, were all preceded, the usual fortnight before, by upward movement of the temperature, near to or above 60° F. An allowance must always be made for epidemic exhaustion, which the curve invariably exhibits if, after the acme is reached, the temperature continues high for several weeks more. The presence of these other factors, however, although adding complexity, in no way detracts from the truth of the inherent principles which Dr. Brownlee has demonstrated and which he will no doubt develop to a still more interesting extent.

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Corrigendum—p. 18, line 21, for "abductor" read "adductor."

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Laryngological Section.

November 6, 1908.

Dr. DUNDAS GRANT, President of the Section, in the Chair.

Opening Remarks.

THE PRESIDENT, before commencing the discussion of the cases, thanked the members of the Section for the honour they had done him in electing him President of the Section.

Visitors to this country were generally impressed with the orderliness which pervaded our national life, and yet it had been asserted that we were the least-governed nation in the world. It was his ambition that the Section should be characterized by the most perfect order with the least possible amount of government.

He considered that the greatest benefactors to the Section were those who brought forward cases in which treatment had been unsuccessful, or in which, from some cause, the diagnosis made had been erroneous. They afforded an opportunity of learning how such mistakes might be avoided in the future. Fortunately, that had been a valuable feature of the Section, and the self-sacrifice of the benefactor had, as a rule, received the tactful appreciation which it had so thoroughly deserved. To that rule he hoped there would be no exceptions. In recounting the previous experiences of a patient, reference to practitioners who had previously seen the case should be made with considerable delicacy; he doubted whether it was quite ethical to mention the name without the consent of the practitioner in question, and always to take the word of a patient on the matter might not be doing justice to a brother who might be absent.

Members should always keep in mind that their discussions were not like those of a dialectical society, in which the aim of the speaker was to score off an opponent, but to arrive at the truth, which all desired.

2 Grant: *Epithelioma of Larynx and Hypo-pharynx*

Witticisms were entitled to a place when they helped to reinforce a truth or to impress it on the memory, and he hoped they would always tend to enliven the proceedings; but where they were used to divert attention from a serious argument they were out of place.

Brevity was said to be the soul of wit, but it was not the whole soul; lucidity was much more essential. The endeavour should be to avoid extremes, and his aim would be to guide the Section on those lines, without, as far as possible, enforcing the rules of debate.

Case of Extrinsic Epithelioma of the Larynx and Hypo-pharynx in a Woman aged 41.

By J. DUNDAS GRANT, M.D.

E. M., AGED 41, complained of difficulty in swallowing, of eleven weeks' duration, which had come on acutely with sore throat on starting teaching in a higher grade school. Laryngoscopically there was seen on the posterior wall of the pharynx, just above the level of the ary-epiglottic folds, an irregular shelving projection of a dull pink colour and slightly everted. When the larynx was drawn forward for hypo-pharyngostomy, it was definitely made out to be the upper margin of an ulcer, most probably of epitheliomatous nature, which extended on to the posterior part of the left ary-epiglottic fold. Only a very small œsophageal bougie could be passed in the direction of the left pyriform fossa, and in view of a possible error in diagnosis, iodide of potassium, perchloride of mercury, and opium were prescribed. A swabbing of the secretion from the ulcer revealed no tubercle bacilli, and a fragment removed for microscopical examination was found to consist simply of round-celled tissue; a further fragment, however, was found to consist of typical epithelioma, and thus confirmed the original diagnosis. After a fortnight of the specific treatment, it was certainly possible to pass a larger œsophageal bougie, but the diseased condition continued. Among the palliatives which have given her the most relief have been the inhalation of anæsthesin and the application of menthol and guaiacol. At first she could swallow only liquids; now she is able to take sops, and occasionally, after the anæsthesin powder, she can take a little boiled fish or a lightly boiled egg.

DISCUSSION.

Sir FELIX SEMON said that these cases afforded corroboration of the fact to which he drew attention in 1894, and more recently, that whilst cancer of the larynx was very rare in women, the few cases which did occur in them were almost always of the extrinsic variety.

Dr. WILLIAM HILL called attention to the unfortunate title which Dr. Grant had applied to this case. Dr. Scanes Spicer had called attention at Exeter last year to the unsatisfactory and ambiguous employment of the terms "intrinsic" and "extrinsic" in reference to laryngeal cancer. Here was a carcinoma originating in the pharynx, and mainly a pharyngeal carcinoma, which, because it in due course invaded the larynx, was scheduled as an extrinsic laryngeal carcinoma; its most prominent mark, namely "pharyngeal," was, in accordance with a regrettable practice, not even mentioned. The case would be more accurately described as a pharyngo-laryngeal party-wall cancer; it was a primary cancer of the pharynx and merely an invasion cancer of the larynx. Sir Felix Semon, following more or less on Krishaber's classification and terminology, divided laryngeal carcinomata into two groups, viz., those which he described as arising from the cavity of the larynx, and which, like Krishaber, he called intrinsic, and those which he thought arose from other parts of the laryngeal mucosa, which he called extrinsic. Now, no cancer arising in the larynx could anatomically be described as extrinsic in origin, though it might become at a later stage extrinsic by extension; all primary laryngeal carcinomata were endo-laryngeal in origin and also at first in situation, that is to say, they were intrinsic whether they arose from the glottic region and its adjacent parts or from the region immediately below the margin of the larynx, and they only became extrinsic by extension to the pharynx, &c. There was much misconception in certain high quarters as to what actually constituted the laryngeal mucosa. The margin of the laryngeal tube was represented by the free border of the epiglottis and ary-epiglottic folds, and then passed over the ary-tænoids and along the upper border of the cricoid plate—the inter-ary-tænoid region; all mucosa internal to this margin was laryngeal—endo-laryngeal, of course—but the larynx possessed no true exo-laryngeal mucosa. The mucous covering of the pharyngeal aspects of the ary-epiglottic folds, ary-tænoids, and posterior surface of the cricoid plate and pyriform fossa was unquestionably pharyngeal, not laryngeal, mucosa, and tumours arising from that mucosa were primarily pharyngeal cancers, though admittedly they speedily assumed the added character of invasion cancers of the larynx. Although Sir Felix Semon habitually employed what he (the speaker) regarded as the confusing terms "intrinsic" and "extrinsic" in reference to cancers which were sometimes truly laryngeal in origin and sometimes not, Sir Felix for the most part carefully avoided the expression "*extrinsic in origin*," and he (the speaker) thought that perhaps Sir Felix only meant to convey the undoubted fact that the tumours arising from Krishaber's so-called intrinsic and extrinsic areas were merely

intrinsic and extrinsic in tendency only. Others, however, were less cautious, and definitely spoke, to give an example, of a primary endo-laryngeal carcinoma, *e.g.*, from the laryngeal side of the ary-epiglottic fold, as extrinsic in origin—an anatomical absurdity. Mr. De Santi, for instance, in his excellent little book on "Cancer of the Larynx," repeatedly used the expression "extrinsic in origin" in reference to carcinoma of admittedly endo-laryngeal origin. That work was entitled to be regarded as authoritative, not only on account of the special circumstances attending its compilation, but more especially on account of its general freedom from inaccuracies. Mr. De Santi's book was, however, concerned only with primary cancer of the larynx; pharyngo-laryngeal—*i.e.*, invasion—cancer of the larynx from the pharynx was not discussed at all. Mr. De Santi was too good an anatomist to describe cancer originating on the pharyngeal mucosa of the ary-epiglottic folds, the ary-tænoids, the pyriform fossæ, and posterior surface of the cricoid plate as in any sense primary laryngeal cancers, and by inference his statistics excluded them. But Mr. De Santi, though naturally enough employing as far as he could Krishaber's regional classification, had, unfortunately, also adopted the latter's misleading terminology, so that following authority we found him writing of carcinomata having their origin in the endo-laryngeal mucosa covering the epiglottis, the ary-epiglottic folds, ary-tænoids, and anterior surface of the cricoid plate as "extrinsic in origin," when, as a matter of fact, they were truly intrinsic in origin and extrinsic only in tendency. Many other writers following Semon included in the term "extrinsic laryngeal carcinoma" growths arising on the pharyngeal as well as those arising on the laryngeal mucosa of the laryngo-pharyngeal party wall, including such a largely pharyngeal structure as the pyriform fossa. He (the speaker) fully recognized the practical advantages and clinical necessity of the more extended grouping adopted by Sir Felix Semon, and he (Dr. Hill) provisionally proposed to allude to these two groups as (1) essential or circumglottic area laryngeal cancers, and (2) party-wall cancers. The former were wholly endo-laryngeal in origin, with a tendency to remain long intrinsic, whereas the party-wall cancers were rapidly invading growths and included both those of intralaryngeal, *i.e.*, intrinsic, origin and those of extralaryngeal, *i.e.*, extrinsic, origin—invasion cancers; and the habit of speaking of all these party-wall cancers as "*extrinsic laryngeal carcinomata*" was generally misleading, and, as applied to the early stage of their growth, often absolutely wrong, whatever their tendency might be. With a view to elucidating his remarks, he begged to hand round two copies of his (the speaker's) comprehensive "Classification of Primary and Invasion Laryngeal Cancers," and he hoped Sir Felix Semon would do him the honour of carefully scrutinizing this scheme at leisure in the event of his remarks not proving sufficiently clear and convincing.

Dr. JOHNSON HORNE thought Dr. Hill had raised a very important point, which was well worth further consideration at some future time. If the statements of Dr. William Hill and Sir Felix Semon were accepted, it practically meant that *primary* cancer of the larynx in a woman would become an unknown disease.

Sir FELIX SEMON said he certainly had no confusion of mind on the subject. As in the second part of "Faust," people put a mystery into his words which had never been meant, and that was at the root of the remarks of Dr. Spicer at Exeter and of what Dr. William Hill had just said. He (Sir Felix) did not mean anything different from Krishaber; Butlin and he had followed that authority in calling intrinsic cancers those situated in the cavity of the larynx proper—in other words, those affecting the vocal cords, the ventricular bands, and the subglottic part of the larynx; that was surely a very simple definition. Extrinsic cancers were those originating from the epiglottis, the ary-epiglottic folds, and the inter-arytænoid fold. Whether that was strictly scientifically a good or a bad definition might be open to argument; but from the practical point of view it certainly would be a pity if that convenient division were disturbed, because there was a most important difference in the extrinsic as compared with the intrinsic variety. The latter remained for a long time limited to the larynx itself, and therefore the results of thyrotomy were excellent. The extrinsic cancers, on the other hand, led to early infection of the glands in the neighbourhood, and therefore were not suited for that operation. Dr. Jobson Hodge's remark that real cancer of the larynx in women was practically an unknown disease was an exaggeration. Cancer of the larynx generally was a very rare disease in women, and if it occurred it was almost always of the extrinsic kind, but he (the speaker) had himself seen and described a few cases of cancer of the vocal cords in women.

Dr. SCANES SPICER said the current statistics of cancer of the larynx appeared to be based on vague ideas associated with the terms "extrinsic" and "intrinsic." Sir Felix Semon had just spoken of intrinsic cancer as cancer *arising* in the cavity of the larynx, and of extrinsic as those growths *situated on* the epiglottis, epiglottic folds, and inter-arytænoid folds. Therefore the distinction seemed sometimes to be based on where the morbid appearances were most marked and sometimes on the assumed site of origin. That was ambiguous. The statistics founded on that needed to be sifted and analysed. At Exeter, in 1907, he had called attention to the fallacies surrounding the subject, and the clinical importance thereof; for in his experience cancer of the posterior wall part of the "extrinsic" area was much more common than that of the vocal cord ("intrinsic") area.

The PRESIDENT (Dr. Dundas Grant) said the case he brought forward would be better described as epithelioma of the pharynx extending to the larynx. He attributed great practical value to the distinction between intrinsic and extrinsic as proposed by Krishaber.

Case of Thyro-lingual Sinus in a Boy aged 14.

By J. DUNDAS GRANT, M.D.

THE fistula opened about $\frac{3}{4}$ in. above the sternal notch, and was surrounded by an area of cicatricial tissue of the size of a sixpence. The cord could be felt extending up to the hyoid bone, behind which it disappeared. The finest possible probe could only pass for the distance of $\frac{3}{4}$ in. It was freely dissected out on October 9, and above the impermeable spot it was slit up so that a very fine galvano-cautery point could be inserted as far as its termination behind the hyoid bone, so as to destroy the secreting surface. The dissection was then finished, and the removal of the tube appeared to be complete. The wound was closed up by means of a subcutaneous suture, but so much tissue was removed at the lowest part in order to dissect with the cicatrix that primary union did not take place at that part, although in the upper part the union was complete.

Dr. GRANT said he would watch the case and bring it forward at a later period, whether it continued well or recurred. He was not very sanguine, as he could feel a suspiciously firm cord under the cicatrix.

**Epithelioma of Left Vocal Cord; Thyrotomy; Recurrence;
Extirpation of Left Half of Larynx; Lasting Cure.**

By Sir FELIX SEMON, K.C.V.O., M.D.

THE patient, a clergyman, aged 46 at time of first operation (aged 48 years and 10 months at present), sent by Dr. Lewis and Mr. Marsh, of Birmingham, had been treated elsewhere for some length of time for tuberculosis of the lungs and larynx without effect, until ultimately in December, 1905, the diagnosis of malignant disease of the larynx was definitely established. At that time the left vocal cord had quite perished in a mass of diffuse red infiltration, from which white projections stood out. The left half of the larynx, except the arytaenoid cartilage, was perfectly immobile. Thyrotomy was performed on December 11, 1905, when a tough growth, occupying the greater part of the left vocal cord, superficially ulcerated, and on section of a

yellowish white colour, was removed. There were no enlarged glands in the neck. The growth was found by Mr. Shattock to be a typical squamous-celled carcinoma. The patient made a good recovery from the operation itself, but the wound granulated internally and externally much more extensively than is usually seen in these cases. On February 13, 1906, a good deal of irregular swelling was seen on the left side of the larynx, below where the left cord had been. This swelling appeared mammillated, greyish, and succulent. This tissue quickly increased in size, and on March 14, 1906, Mr. Shattock reported that the structure of a fragment removed intralaryngeally was that of a typical granuloma. The growth was then entirely removed with Lõri's cutting catheter, with the exception of one small nodule about the size of a pin's head in the middle of the scar. This spot did not disappear spontaneously, as had been hoped, but on the contrary, according to Mr. Marsh's report, gradually increased until, at the beginning of October, 1906, there could be no doubt that a genuine recurrence had taken place. On October 16 the larynx was reopened, when a growth was seen to occupy the middle of the scar a little more to the posterior than to the anterior part. The whole of the left wing of the thyroid except its uppermost part was removed. There was free bleeding, which was finally arrested by the application of the thermo-cautery. The soft parts of the left side of the larynx were, by suitable suturing, stitched to the big muscles, so as to leave as little chance as possible for the development of subglottic stenosis, whilst the small remaining part of the left half of the thyroid cartilage was fixed in proper apposition to the right half. The Hahn's tube was replaced by an ordinary tracheal tube, and the external wound then stitched together. He made such a good recovery that on the eighth day after the operation he was able to go out, the external wound having almost entirely closed. The voice, almost immediately after the operation, was a well-audible whisper, and has since then wonderfully improved, although a very slight amount of stridor is occasionally heard after exertion.

On July 3, 1908, the following note was made: "Very satisfactory condition, voice surprisingly good; left half of larynx replaced by an immobile red ridge, the margin of which stands just in the median line, somewhat above the level of the right vocal cord, underneath which a thin cicatricial ridge can be seen extending along the right lateral wall to the posterior wall. Not the least trace of any recurrence."

DISCUSSION.

Dr. STCLAIR THOMSON asked whether Sir Felix kept in the tracheotomy tube and stuffed the wound for some time, or whether he treated it as he did an ordinary laryngo-fissure—without a tracheotomy tube and without stuffing.

Sir FELIX SEMON, in reply, said the Hahn's tube was replaced after the operation for a few days by the ordinary tracheal tube. He particularly recommended, where half the larynx was being extirpated, that the soft parts remaining should be stitched to the big muscles on the same side. That prevented stenosis in a remarkable way, and stenosis was the great risk after hemilaryngectomy.

Case for Diagnosis. (?) Continuous Fibroma of Neck and Larynx or Malignant Disease of the Larynx with Enlargement of Glands in the Neck.

By Sir FELIX SEMON, K.C.V.O., M.D.

THE patient, aged 45, had, eight years ago, a tumour removed by Sir William Watson Cheyne from the floor of the mouth on the left side. Several enlarged cervical lymphatic glands were removed at the same time from the left side of the neck, and microscopic examination of the parts is said to have shown undoubted epithelioma. There was freedom from trouble for nearly eight years until recently, when the patient complained of huskiness and discomfort in the throat.

On October 19, Dr. Daniel brought him to me for a consultation, when a large swelling was found occupying the region of the left arytaenoid cartilage and left ary-epiglottic fold. This so completely prevents an inspection of the cavity of the larynx proper that only occasionally a glimpse of the anterior third of the right vocal cord can be obtained. In consideration of the size of the tumour it is surprising that the voice is perfectly normal and that there is practically no stridor. It is red, very slightly mammillated, looks very dense and elastic, and is nowhere ulcerated. On phonation the whole swelling makes a distinct movement in the direction from within to without.

On laryngoscopic examination a rounded swelling of about the size of a Tangerine orange, but rather flat, is felt behind the sterno-mastoid. It is somewhat movable, not tender to the touch, not adherent to the skin. There are several enlarged but not tender glands to be felt in

both sides of the neck. There is only very slight discomfort in swallowing, but the patient can easily take solid food. There is no dyspnoea and no stridor. The aspect of the laryngeal swelling so much resembles that of the case of soft continuous fibroma of the larynx and neck shown repeatedly by Sir Felix Semon to the Laryngological Society of London that one certainly would think in the first place of similar conditions obtaining in the present one, were it not for the patient's history.

Sir William Watson Cheyne has kindly completed the notes of the case as follows:—"October 30, 1908: Captain T. was sent to me by Greville MacDonald on December 19, 1900. I found an ulcer, clinically evidently epitheliomatous, in the floor of the mouth at the front part, and some enlarged glands under the chin and in the submaxillary region. On December 21 I excised the ulcer and the glands. I have not a note of the microscopic appearance, but I am sure it was examined and found to be epithelioma. He came back in the beginning of March, 1901, with a small recurrence in the floor of the mouth, and a mass of enlarged glands in the anterior triangle, and on March 8 I operated again. I saw him again in June, 1901, when he was all right, and heard nothing more of him till the other day, when Dr. Daniel brought him to me again. The swelling of the larynx does not have any direct connexion with the old scar, in fact is far from it, and may quite well be of a different nature altogether. On that matter I will not express an opinion. The glands are hard, adherent, and directly a continuation of the chains of glands formerly removed. I should doubt if they have anything to do with the laryngeal trouble, and I strongly suspect that they are a delayed recurrence of the epitheliomatous gland trouble. It would be quite easy to take out one for microscopical examination, but if they are epitheliomatous I doubt if it would be possible to do a really radical operation. But if you want a gland out I am quite ready to do it."

When the patient was seen this morning (November 6), *i.e.*, three weeks after the first examination, an ominous spot of ulceration had made its appearance on the œsophageal aspect of the growth, thereby rendering the diagnosis of malignant disease almost certain. Under the use of iodide of potassium the internal aspect of the tumour had slightly decreased, and a little more could now be seen of the right half of the larynx and of the chink of the glottis.

DISCUSSION

Sir FELIX SEMON wished to explain that he would not have brought forward the case as a doubtful one with regard to diagnosis if the present condition had existed at the time he sent the note. There was now a patch of ulceration on that part of the growth occupying the posterior wall of the larynx. All the same, he would be very glad to hear opinions as to the nature of the case and recommendations as to procedure. He had asked Sir W. Watson Cheyne to give him full particulars as to the character of the growth removed eight years ago. From his reply it seemed uncertain whether the growth had been microscopically examined or whether, at any rate, the notes of such examination had been preserved. But clinically Sir William was quite certain it was epithelioma.

Dr. STCLAIR THOMSON said the growth appeared to be on the posterior surface of the party wall, between the larynx and œsophagus. It had the white ulceration which so strongly suggested malignant disease. He did not counsel anything being done for it, though probably Professor Gluck might think the case operable.

Mr. HERBERT TILLEY shared Dr. StClair Thomson's opinion concerning this case. The right arytænoid cartilage and right side of the larynx were now involved as well as the left. The only chance for the patient would be laryngectomy, with removal of the large mass of glands in the neck. Three weeks ago he saw a patient in whom the disease was equally extensive, and a very thorough and extensive operation had been performed, and the patient, who was aged 68, had done well. Since in the present case it was eight years since the original operation was performed it was possible the disease was not so malignant as in those cases where the primary growth was small, but rapidly invaded surrounding structures.

The PRESIDENT (Dr. Dundas Grant) said that the ultimate decision must rest with the patient, who could only have a miserable time in either event. He thought it possible that it could be removed, but palpation would help in that decision very much. Though he did not think the operation should be urged, if the patient cared to accept the risk he should be allowed to do so.

Inter-arytænoid and Subglottic Infiltration of Fifteen Months' Duration, causing Difficulty of Breathing which necessitated Tracheotomy.

By HERBERT TILLEY, F.R.C.S.

W. A., MALE, aged 57, had tracheotomy performed on July 1, 1907, to relieve increasing difficulty in breathing. When seen by exhibitor on June 4, 1907, the glottis was encroached upon by a pyramidal-shaped,

non-ulcerated swelling springing from the anterior surface of right arytenoid. Below the anterior commissure a smooth, rounded tumefaction could also be seen. Both vocal cords moved freely, but the right was congested, and its edge irregular and swollen. Increasing doses of iodide of potash, given until 30 gr. were taken three times daily, and for four weeks at a time, had little apparent effect in reducing the swellings.

July 5, 1908: The swelling had diminished in size, and, as breathing through the glottis was free, the tracheotomy tube was removed. September 14: Stridor was increasing again, and, by means of direct vision, a portion of the larger swelling was removed. The following day reactionary swelling necessitated reinsertion of tracheotomy tube, which has remained in situ till the present.

The larynx and trachea were demonstrated by means of Brüning's apparatus for direct examination of the œsophagus and lower air passages.

DISCUSSION.

Mr. STUART-LOW said he admired Mr. Tilley's perseverance in the case, and perceived his difficulties. The first difficulty arose if the seat of the patient in such cases was not a low one. Killian had a seat only 14 in. high, with a tall straight back, and the patient was seated with the back erect and the head thrown back. The second difficulty occurred when the patient was not cocainized thoroughly. Killian's assistant cocainized for half an hour before any attempt was made to pass the instruments.

Mr. HERBERT TILLEY, in reply, thanked Mr. Low for his suggestions. It might be an advantage if a smaller chair were in the room, but the chair did not constitute a real difficulty, especially with a fairly tall operator. Neither had he experienced any difficulty owing to deficient cocainization of this patient; he thought the patient stood the examination extremely well—sufficiently long for those who wished to do so to see the larynx. There were several small points to learn in the examination of such cases, but after a week or two one got over them instinctively. He regretted he could not show the bifurcation of the trachea in the second case. It was three months since he last examined him, and there was now a large growth ulcerating into the trachea. Only those who had looked at the larynx by the direct method could have any idea how well defined everything looked; compared with Killian's former method there was 50 per cent. more light and much more room. He thought it would revolutionize the older methods of intralaryngeal interference, especially with regard to the removal of simple growths and the treatment of local ulcerations.

**Case of Malignant Disease of the Right Maxillary Antrum,
involving the Outer Wall of the Nose and the Cheek, in
a Male aged 68.**

By CHARLES A. PARKER, F.R.C.S.Ed.

THE patient's attention was first attracted to his nose fourteen months ago by recurring attacks of epistaxis with sanious discharge between the attacks. Some nasal obstruction was subsequently noticed, but this has never been very marked. Three months ago the patient first noticed a swelling on the cheek near the inner canthus of the eye, for which he sought advice at the Royal Ophthalmic Hospital, where he was admitted on September 23. Mr. Lawson incised the swelling and let out a very little pus, and on examination found exposed bone at the bottom of the wound. Mr. Lawson thought the condition secondary nasal disease with implication of the sinuses, and transferred the patient to Mr. Parker at the Throat Hospital on October 16.

On examination a firm, though friable, growth, attached to the outer wall of the nose, was seen filling the middle meatus. On posterior rhinoscopy the growth could just be seen round the upper and outer margin of the choana. On transillumination the right side was quite dark, whilst the left was quite clear. A small opening was discovered in the mouth to the outer side of the alveolar border, through which a probe passed into the antrum. Examination with the probe revealed that, though the antrum was by no means filled, growth could be detected on its upper and outer walls. Externally there was a hard infiltration of the soft tissues of the cheek immediately below the inner third of the infra-orbital margin. The incision wound discharged some thin serous fluid, and exposed bone could be felt on examination with a probe. A section of a portion of the growth removed from the middle meatus shows blood-clot chiefly, with areas of granulation tissue, also areas of large and small mono- and poly-nuclear cells, suspicious of malignant disease. Examination of the antrum with the probe and inspection of the nasal cavity show that neither of these regions is as extensively involved as is often the case, and it seems unusual under these circumstances to find an infiltration of the soft structures of the cheek.

Opinions as to the possibility or advisability of operative interference are invited, though in the exhibitor's opinion the distribution of the growth renders the possibility of 'complete removal extremely doubtful, and the patient's age and general condition render the attempt inadvisable.

DISCUSSION.

The PRESIDENT (Dr. Dundas Grant) said the case showed the importance of the examination of the nose in cases of disease in the neighbourhood of the inner canthus.

Mr. STUART-LOW said he had had two cases almost similar, and thought the present case had been allowed to go on too long unoperated upon. Had it been operated upon two months ago the result might have been satisfactory. A man engaged in the meat market received a blow, and came some time afterwards with a swelling in the cheek which looked like a gumma, but it was not of that nature. The skin, as in the present case, was not implicated though inflamed. The man was put under antisiphilitic treatment and watched for a time, and then a radical antral operation was performed. A tumour was found occupying the antrum and was freely removed. He did it by the open method and the whole condition disappeared. Dr. Wingrave believed it was a granuloma of doubtful nature. The other case was a girl, aged 22, who had a swelling above the eye and over the nose; the eye could not be opened to its full extent. After three weeks' antisppecific treatment and watching, the maxillary antrum was opened, and an almost similar condition to the last case was found. Dr. Wingrave was in doubt about the true nature of the growth. These were both taken early and operated upon. For such cases he advised that a radical antral operation should be undertaken at once. He suggested that in the first place the inflammation should be got rid of by the use of the appropriate serum. Dr. Spicer showed last year a case of malignant disease in the back of the throat in which Professor Wright had employed a serum, after which the inflammatory symptoms subsided. After this serum treatment a radical antral operation might be performed or the upper jaw excised.

Mr. PARKER, in reply, said that the existence of a fistulous opening from the antrum into the mouth was of some interest, for through this opening it was possible to define the limitation of the growth within the antrum. It was attacking the upper and outer walls, through which it had spread both into the nose and into the cheek. He disagreed with Mr. Low, as he thought there was definite malignant infiltration of the soft tissues of the cheek. He did not consider it possible to remove the whole disease, and he did not contemplate attempting any operation.

Case of Suppurative Cervical Cellulitis and Epithelioma of the Epiglottis.

By PETER H. ABERCROMBIE, M.D.

THE patient was shown at the January, 1907, meeting of the Laryngological Society, his progress was reported on at the February, 1907, meeting of the Society, and he was again shown at the November,

1907, meeting of the Laryngological Section of the Royal Society of Medicine.¹ On February 5, 1908, I removed enlarged glands from the right side of the neck, and Dr. Wyatt Wingrave reported that he could find no evidences of malignancy in them. This was consistent with the fact that the swelling had been larger before, and was subsiding at the time he was last shown at the meeting in November, 1907. On February 19 last I again operated and removed a mass of matted glands, which Dr. Wingrave found to be epitheliomatous. I did not see him after this for several months. Then he called at the hospital in May, 1908, with a large swelling on the right side of the neck. Further operation was refused. The skin over the mass soon gave way, and he had repeated and severe hæmorrhages during the following few months. He died soon after one of these losses of blood on October 12, 1908. I saw him a week before, when his anæmic state was extreme, and examination with the mirror, on that occasion, showed that there was no recurrence of the disease in the larynx.

Perforation of Soft Palate following a Severe Attack of Scarlet Fever in Childhood.

By PETER H. ABERCROMBIE, M.D.

THE patient, a man aged 35, a bricklayer by occupation, was sent to me by his doctor on account of a discharging right ear, which also was a consequence of the scarlet fever attack. No reference was made by the patient to his throat condition, which was noticed in the course of the usual routine examination. The following history was obtained:— Until he was aged $4\frac{1}{2}$ he had always been perfectly healthy in every way, and his speech at that time was quite normal for a child of that age, according to his mother's statement. When aged $4\frac{1}{2}$ he had a most severe attack of scarlet fever, and very nearly died from it. "Abscesses" developed in the throat and the right ear. A swelling formed behind the right ear, which broke and discharged for several months, after which a piece of bone came away through the opening, and the wound then slowly healed up. The discharge from the right ear has continued more or less ever since. The speech defect was noticed after the scarlet fever, and the hole in the palate was discovered then, too. The palatal

¹ *Proc. Roy. Soc. Med.*, 1908, i, Laryng. Sec., p. 10.

perforation has not caused him any inconvenience, with the single exception of the defective speech. About eighteen years ago he had an operation performed at Golden Square Hospital by Mr. Norris Wolfenden with the object of closing the perforation, but this was unsuccessful.

This appears to be an undoubted case of perforation of the palate due to destructive processes in the course of severe scarlet fever. There are other evidences of old ulceration in the throat. The perforation is unilateral, and there are no signs of malformation present. As a rule, in



Perforation of soft palate due to scarlet fever.

these cases the anterior pillars of the fauces are the parts affected, but here it is the right posterior pillar and soft palate. No operation is proposed; indeed, the patient would not consent to such. His ear is improving under antiseptic treatment.

DISCUSSION.

Sir FELIX SEMON confessed to having had in former days a sort of old-fashioned general practitioner's idea that where there were perforations in the soft palate the cause was always syphilis. Not many years ago it was a very

generally accepted axiom that the mere discovery of a perforation in the palate was sufficient to justify the diagnosis of syphilis. But he had recently observed perforation in the soft palate after an attack of what probably was pneumococcal angina. Later, he saw a sharp-cut ulcer in another case of the same kind in the epiglottis, and that originated practically under his eyes. In cases of obscure inflammatory—particularly in septic—affections of the pharynx and larynx in which suddenly perforation occurred, he would therefore ask members of the Section to have a bacteriological examination made in order to find the micro-organism which caused it. The matter was well worth further study.

Dr. PATERSON asked whether matters would be helped if the Wassermann serum diagnosis for syphilis was carried out. He understood that the reaction also took place in the congenital variety, so that it would include the whole range of syphilis.

Dr. McDUGALL (Liverpool) thought the possibility of a congenital defect should be borne in mind where such an opening was seen. The conclusion should not be hastily formed that it was the result of a destructive process.

Dr. SCHOLEFIELD said it was not so rare as had been supposed for perforation of the palate to occur in scarlet fever. During a couple of years he saw four or five cases in 3,000 patients of clean perforation, which healed and left very little scar tissue; it looked like natural tissue round the opening which was left. Another deformity producing an appearance which might be attributed to syphilis was, in some cases, the punching out of a piece of the palate when removing adenoids. In a relative of his, evidently the edge of the palate had been taken hold of by the forceps and a clean piece punched out. He advised him to always confess the origin of it when he saw a doctor.

Mr. CLAYTON FOX said he thought the absence of cicatrices militated against the condition being the result of a specific fever. The cases he had seen due to the latter had been associated with obvious cicatrization. He saw no reason for not regarding the present case as congenital.

Dr. DAN MCKENZIE said he could not agree with the previous speaker that there were no cicatrices in the pharynx in this case. In his opinion the presence of scars was particularly well marked.

Dr. ABERCROMBIE, in reply, said there was no history of syphilis in the case. The mother said she was quite certain there was no perforation before the scarlet fever and that it followed immediately afterwards.

Laryngeal Vertigo in a Case of Early Tabes.

By STCLAIR THOMSON, M.D.

THE patient came complaining of choking fits, with a sense of suffocation, from which he fell down insensible. Patient has had twelve of these attacks in the last eighteen months. They begin with a suffocating

feeling in the throat, a sense of choking, and he then falls down and loses consciousness. On one occasion he was taken up by the police, and as his breath smelt of whisky (he had taken a little just before the attack) he was taken to the police station. Fortunately a brother was able to explain his malady satisfactorily. He has noticed that his walk is unsteady in the dark and that his legs get into a tangle when he runs. He has had shooting pains in his legs. He has not noticed any change in his voice; although a musical amateur, he has not been able to sing for the last three years. Had specific disease seventeen to eighteen years ago, and was well treated for twelve months. Patient walks well, but it is noticeable that he spreads his feet at rather a wide base, especially when standing with feet together and eyes closed. Suffers from cold feet and shooting pains in the legs. The pupils are irregular, and the right is larger than left; Argyll-Robertson phenomenon. Pulse equal; no signs of aneurysm on auscultation or radiography. The left vocal cord moves very little on adduction and does not abduct at all. It is fixed in the cadaveric position. The voice is clear and apparently unchanged. Knee-jerks absent.

DISCUSSION.

Dr. LAMBERT LACK asked whether other members had met with many such cases. Recently he had had three patients under his care with symptoms corresponding with those in Dr. Thomson's case. One was caused by disseminated sclerosis or disseminated syphilis of the nervous system, but in the other two he could not be sure of the cause, although one was said to have followed influenza. In both the latter cases there was a sudden sense of choking, sometimes preceded by a slight cough, and then the patient became unconscious. He wondered what one should tell the patient, and what was the best form of treatment. Ought such a case to have tracheotomy performed, and wear a tube fitted with a cork which could be taken out in an emergency? There was no sign of paralysis of the larynx, and he wondered whether the attacks were dangerous.

Sir FELIX SEMON asked why Dr. Thomson had called the condition laryngeal "vertigo" in a case of tabes. The description led one to think it was simply a violent laryngeal "crisis"—a feeling of suffocation in the larynx, loss of consciousness, and then a fall. In regard to Dr. Lack's question as to whether the attacks were dangerous, he had read of one case which terminated fatally, but in the majority they were much more frightening than dangerous. The general experience was that the attacks were most violent at the onset of the disease, and gradually decreased as the disease progressed, and that while in many of the cases paralysis later appeared in one abductor or both, the crises disappeared.

Dr. FITZGERALD POWELL said he had seen a similar case in an officer who had been in China and had had malaria. He suffered from some form of paralysis. He did not know if it was tabes, but eventually he lost the power of locomotion. There were spasmodic attacks of the vocal cords, when he got black in the face and fell down unconscious. When he was unconscious the spasm relaxed. His death was due to the paralysis, not to any accident in connexion with his laryngeal spasm.

Mr. PARKER said that many years ago he had brought a patient before the Laryngological Society¹ who had similar symptoms to those present in Dr. Thomson's case. He had looked upon these symptoms as due to laryngeal crises, and it was interesting to note that the first attack had preceded any other symptom of tabes by about three years.

Mr. HORSFORD said that last year he had a case similar to one of Dr. Lack's, which he decided was a neurosis. The patient was a young girl of 19, who had frequent attacks of choking during sleep, as well as one or two occasionally during the day. She was often cyanosed in them, but never lost consciousness. He found she had double abductor paresis. He had never seen perfect abduction of the cords. She had had a good deal of nasal obstruction from adenoids, which he removed without an anæsthetic because of the attacks, and when she had had some months rest in addition the attacks disappeared. It was probably a case of abductor spasm.

The PRESIDENT (Dr. Dundas Grant) said he thought the discussion was straying from what was recognized as laryngeal vertigo, in the typical instances of which the patient gave a cough and dropped down, afterwards recovering his senses instantly. Such cases he regarded as very rare. One of the few he had had was in an elderly man of very gouty habit. He advised the man's doctor to treat his gout, and when that was done the attacks ceased. With regard to the name "vertigo," it was inappropriate because there was no sensation of turning, and in such cases as he had just described he thought the proper name was laryngeal syncope.

Dr. STCLAIR THOMSON, in reply, said he brought the case up chiefly to excite discussion, because he thought it an opportunity to clarify the terms. He only used the term "vertigo" because Charcot did so. Why not use the term "spasm of the larynx"?

Sir FELIX SEMON, in further comment, said that the spasm of the larynx which often occurred in connexion with tabes was generally known under the name of "crisis." One spoke of gastric crises, and so also of laryngeal crises.

Dr. STCLAIR THOMSON, in further reply, said "crisis" was used where the subject was certainly tabetic, and when the attacks occurred in such cases as Dr. Lack's they might be called "spasm." He was willing to withdraw the term "laryngeal vertigo" in the present case. Charcot's original paper mixed up spasm of the larynx, spasmodic laryngitis, and other conditions. Dr. Thomson had a young woman under his care who apparently had double

¹ *Proc. Laryngol. Soc., Lond., 1895-6, iii, p. 46.*

abductor paralysis and attacks of spasm, and she carried about a tracheotomy tube ready for insertion. She was a morphinomaniac, and he was puzzled, but he believed Sir Felix Semon saw her afterwards and, with more deftness, managed to see her cords give a momentary gape. He also saw a private case which used to get those spasms and unconsciousness. Sometimes he would spring out of bed. He died five years afterwards from mediastinal tumour. Perhaps it would be well to reserve the term "spasm" for cases like Dr. Lack's, and "crisis" for cases of tabes. He was sorry that time had not permitted reference to a learned paper by Dr. McBride in the *Archiv für Laryngologie* on the subject.

Case of Extensive Fracture of the Walls of the Frontal Sinus.

By J. GAY FRENCH, M.S.

THE patient, a man, was aged 64 at the time of his accident, which took place in September, 1903. He was struck on the forehead and left forearm by the step of a passing engine. On admission to the Great Northern Central Hospital he was found to have a fracture of both bones of the forearm and a cut 3 in. long on his forehead, with a marked depression over the frontal bones. The pupils were unequal, the left smaller than the right. There was bleeding from the nose and vomiting of a considerable quantity of altered blood. He was conscious at the time of admission, but blindness and incoherency of speech were rapidly coming on. Under an anæsthetic the wound in the forehead was enlarged, and it was found that the walls of both frontal sinuses—which were large—had been extensively fractured and driven in. There was also fracture of the posterior wall of the left frontal sinus, with exposure and tearing of the dura mater. The septum between the two sinuses was completely broken down. The parts were cleansed, all the loose comminuted bone removed, the dura sutured, and the skin wound closed except for a drain.

The patient made an uninterrupted recovery, being discharged on November 9. He refused to have any plastic operation done. (X-ray photographs were shown.)

Mr. GAY FRENCH said that it was fortunate for the patient that he possessed a pair of large and well-developed frontal sinuses, as no doubt a large part of the force which was expended in fracturing and comminuting the anterior walls and septum would have—in the case of a man with small frontal sinuses—expended itself in extensive damage to the brain and vital centres, and the patient would in all probability have been killed outright.

Case of Congenital Occlusion of the Right Posterior Naris.

By HAROLD BARWELL, F.R.C.S.

THE patient, a girl aged about 25, has suffered all her life from complete obstruction of the right nostril. A deflection of the septum prevents adequate inspection of the deep parts of the nose from the front. With the rhinoscope mirror the right choana is seen to be occluded by a partition, in the upper part of which a crescentic projection covers a small depression, or possibly a minute perforation.

DISCUSSION.

Mr. CRESSWELL BABER thought there was no doubt it was a congenital occlusion of the posterior naris. At one time such conditions were considered to be rare, but several cases had been shown at the Society, of which his own was the first, and was followed by that of the President and of Dr. Ball. In his own case the obstruction was partly membranous and partly bony, and the bone, when operated upon, was found to be $\frac{1}{2}$ in. thick. At the upper part of the obstruction a depression was seen by posterior rhinoscopy in the same position as the small perforation in the present case. In the present case he did not notice any difference in the size of the two cheeks. In his own case the affected side was larger than the other. That showed that the atrophy was not due to the occlusion, as might have been supposed. A case had, moreover, been recorded in which both posterior nares were congenitally closed and one side alone was atrophied.

Dr. BALL asked what Mr. Barwell intended to do. In the case which he showed himself, Mr. Charters Symonds suggested the line of treatment which he (Dr. Ball) had since followed. It was to remove about $\frac{1}{4}$ in. of the posterior edge of the septum together with as much of the occluding membrane as possible. That made a permanent opening at one sitting, and there was no subsequent trouble. In the present case it would be difficult to do that because there was such a marked deflection of the septum to the right side in front. He would be inclined to operate on the septum as far back as seemed necessary, let that get well, and then remove the posterior end of the septum. The choana on the occluded side was always the smaller in these cases.

Dr. SCANES SPICER said it would be interesting if the case were treated on the principle of submucous resection of the septum, so as to settle what was the nature of the mass behind by approaching it along the septum. He thought the bony growth came in this case from the septum. Freyer had previously remarked that many of such growths were septal in origin. He (Dr. Spicer) thought the whole of the obstructing pyramidal mass arose from

the septum, and by submucous resection carried right back to the posterior margin of the vomer it might be got away. He thought there probably was organic adhesion between the opposed mucous membrane surfaces, so that it would be necessary to divide this and insert a rubber of lint for a few days.

Dr. McDOUGALL said he had never had a similar case, but he thought that if the operator put a metal guard on his finger and introduced it to the back of the choana the obstruction might be cleared out and the nasal passage kept open without difficulty.

Dr. PATERSON said the case was of interest from the point of view of the origin of such conditions, and it appeared to confirm the views of Hochstetter as to the development of the nasal fossæ. Hochstetter contended in contradistinction to this that the primitive nasal cavity was formed as a blind sac which pushed backwards and became separated from the mouth cavity only by the membrana bucco-nasalis, which ultimately ruptured and formed the primitive choana. In the case shown the closed posterior choana was well defined, and it seemed to him best explained by regarding it as a persistent bucco-nasal membrane.

Mr. CLAYTON FOX asked whether members would express their opinions as to the origin of those cases. He thought the idea of its being a party wall was put out of court, and the question was whether the deformity was a remnant of the posterior part of the oral pit or whether it was due to the fusion of the elements which entered into the normal formation of the choana. The oral plate was further back and could not form the immediate closure of the choana.

Mr. BARWELL, in reply, said he was rather afraid of the tendency to contract. He proposed to do a submucous resection of the nasal septum first, so as to get a good view backwards, and then remove freely with the chisel, remembering Dr. Ball's suggestion as to the back part of the nasal septum.

Epithelioma ; primary in Posterior Wall of Pharynx, now invading Œsophagus and Larynx.

By WILLIAM HILL, M.D.

THE patient was a woman, aged 39. Tracheotomy and gastrostomy were performed six and seven weeks ago respectively. When the case was first seen at the end of August the tumour was as large as a pigeon's egg and projected into the vestibule of the larynx, yet there was no marked cervical glandular enlargement at that date, and it is not a prominent feature even now. Portions were removed through Mosher's spatula.

Case of Tuberculous Disease of the Larynx.

By DAN MCKENZIE, M.D.

THE patient, a man aged 51, was shown at the June meeting of the Section as a case of malignant disease. On that occasion the suspicion was expressed by Sir Felix Semon that the case was one of tuberculosis rather than malignancy. In consequence of this expression of opinion the case has been carefully watched with the view of arriving at a definite decision, and, as time has gone on, the clinical appearances have gradually come to assume the aspect of tuberculosis. During the period of observation portions of the diseased tissue were from time to time removed and handed to Dr. Wyatt Wingrave for pathological examination, but it was not until several examinations had been made that the definite opinion was pronounced that the disease was tuberculosis. Dr. Wingrave's specimen is on exhibition. In the meantime evidence of tubercle in the lung was afforded by the discovery of crepitant *râles* at the right apex. Tubercle bacilli have not yet been found in the sputum. Thus Sir Felix Semon's suspicions have been thoroughly justified.

The exhibitor has been struck with the simulation to the text-book descriptions of cancer of the larynx presented by this case. The patient, when first seen, was aged 51, and had been suffering from hoarseness for four months. The left side of the larynx was red, immobile, and so greatly swollen that the ulceration at the left side of the base of the epiglottis could only with difficulty be descried. The ulcer looked malignant. The right side of the larynx seemed to be quite healthy.

DISCUSSION.

Mr. GAY FRENCH said there seemed to have been a good deal of difficulty in the early diagnosis of the case. It was the type of case in which the determination of the opsonic index for tubercle and the administration of tuberculin might have helped matters forward when the case was first seen.

Dr. McDUGALL asked how long infiltration of the left glosso-epiglottic fold had been present. It was characteristic, and he thought the case was one of chronic pulmonary tuberculosis. When one thought the patient was going on well he was apt to have a sudden acute exacerbation.

The PRESIDENT (Dr. Dundas Grant) said that those who saw the case early would have the greatest difficulty in calling it tuberculosis. The great rule, which was excellently formulated by Dr. Jobson Horne in the discussion at Exeter, was

first to exclude tubercle before thinking of syphilis or cancer. But in that case the unilaterality of the condition, the infiltration of the side of the epiglottis corresponding to the ulceration beneath, and the swelling, were so great on the affected side and so conspicuously absent on the other that tuberculosis scarcely seemed likely; it looked more like epithelioma. And to those who watched it slowly evolve the difficulty was still present. The thanks of the Section were due to Dr. McKenzie for bringing the case forward.

Dr. DAN MCKENZIE, in reply, said there had seemed to be no difficulty in the early diagnosis, and had there been any idea that the case was tuberculosis, the opsonic index would have been determined. After a doubt was raised, every possible means of coming to a conclusion was tried. The ophthalmo-reaction was tried, but the man had bleary eyes and one could not be sure of the effect. The glosso-epiglottic folds had been infiltrated for some considerable time.

Case of Recurring Epistaxis (Alveolar Epithelioma).

By W. STUART-LOW, F.R.C.S.

THE patient was a man aged 65, a stoker in a gasworks, sent to the hospital for recurring epistaxis from the right nostril, tumefaction of the base of the nose extending to the right orbit, and a spongy protuberance in the right middle turbinated region.

Pathological Report by Dr. Wingrave.—The growth consists of a well-marked stroma forming alveoli containing epithelial cells. The stroma consists for the most part of closely packed fusiform cells with some lymphocytic infiltration. Its meshes vary in thickness and arrangement, showing great irregularity, consisting of only a few cells or fibres in one part and forming the bulk of the tumour in another. The alveoli also vary considerably in size and shape. Each contains loosely packed epithelial cells, for the most part spheroidal, but varying in size and shape from mutual pressure. The nuclei also exhibit great variation; they stain deeply and some are fragmented. The cytoplasm is granular, and selects acid fuchsin and eosin. These cells are, from their morphology and tinctorial characters, more like gland epithelium than endothelium, but the growth at first sight is strongly suggestive of an endothelioma. It probably started in a glandular structure, some sections exhibiting an apparent transition. From its general arrangement and the polymorphic character of its elements the growth is doubtless malignant in its nature.

Mr. STUART-LOW said that the patient first noticed the bleeding from the right nostril four months ago. There had been no pain until the last few weeks, when the orbit had been invaded. The maxillary antrum on the same side was quite dark when the man was first sent to the Central London Nose, Throat, and Ear Hospital, and it had remained so. * During the past fortnight he had felt increasing and nearly constant giddiness. There could be little doubt, therefore, that the tumour had extended through the cribriform plate into the cranium, causing brain pressure, and downwards and outwards into the maxillary antrum. He had, during the last few weeks, treated four cases of malignant disease in the mouth, all arising in men who were engaged in occupations necessitating the inhalation of hot dry air and dust, and he considered this a contributory cause of cancer in susceptible and vulnerable subjects, viz., in people with thin and deficiently secreting mucous membranes—the hypomyxomatous. In his opinion there was no likelihood of any operative measures being of any use, as all the growth could not be removed.

Case of a Woman with Rhinoscleroma.

By W. STUART-LOW, F.R.C.S.

THE exhibitor said that as there was a history of the duration only being eight months this was doubtless an early stage of rhinoscleroma, but of this stage it was quite typical. Since the cutaneous structures had become invaded he had had the great advantage of Mr. Hartigan's assistance, and he was now trying to cultivate the bacillus with the object of preparing a vaccine for the treatment of the case. In view of the futility of all treatment hitherto employed, he desired opinions as to the feasibility of using vaccines.

Pathological Report by Dr. Wingrave.—On May 25, 1908, a small fragment was cut and examined by direct method, when it was seen to consist principally of typical granulomatous tissue undergoing well-marked sclerosis. This element was formed of lymphocytes containing strands of epithelioid cells with a scanty stroma of fibres, staining deeply with thionin. There were no giant-cells, and the epithelioid elements showed a tendency to fibroplastic changes, staining with eosin. Minute blood-vessels were well marked, with endothelial activity in some points, while in others there was well-marked arteriosclerosis with blocking of the lumen. The surface epithelium was very irregular and distinctly thickened, with a distinct tendency to collective but not individual invasion. No tubercle bacilli could be seen in the substance of the tissue, either inter- or intro-cellular. Hyaloid bodies were not developed.

The bacillus of rhinoscleroma had been isolated and took the staining well, but it soon faded. A film or smear was taken by a curette from the ulcerated surface and was found to contain various bacteria, chief of which was a Gram + diplo-bacillus and many Gram -- diplococci. No tubercle or acid-fast bacilli. The capsule of the diplo-bacillus was doubtful. Agar inoculation by sweep afforded a polymicrobial growth showing both long and short forms of a Gram + diplo-bacillus without a capsule. *Nature*: It is a granuloma in which the tendency is toward sclerosis, and probably an early stage of rhinoscleroma before hyaloid bodies are formed. Neither tubercle bacilli nor spirochætæ were seen.

DISCUSSION.

Dr. JOBSON HORNE asked what were the facts on which the diagnosis was based. Unless it certainly was rhinoscleroma it should not be indexed in the *Proceedings* as such. Such errors tended to detract from the scientific value of the *Proceedings*. His own impression was that it was an ordinary specific lesion.

Dr. FITZGERALD POWELL said he thought the case resembled what was known as syphilitic lupus, which might clear up under antisyphilitic treatment, but they had not heard any report of the microscopic examination.

Dr. STCLAIR THOMSON said he did not see why they should wander beyond lupus or syphilis in the case, as the woman had been thirteen years in this country. If she was inoculated abroad it had remained latent a long time. The few cases he had seen in Vienna and England of rhinoscleroma had never had a weeping surface. There was sclerosis without breaking down, and he would be glad to learn what the pathological report was. Frisch's bacillus was now given up as the cause of it, and it was recognized that the organism so named was not different from Friedländer's pneumococcus.

Mr. STUART-LOW, in reply, said the diagnosis was a pathological one, as the bacillus had been found and a vaccine was being prepared. Syphilis was quite excluded, as she had had antisppecific remedies pushed. The surface was moist because he had been obliging dermatologists with specimens cut from it. The patient said she was now better in every way. Glycerine-boraledehyde had also been applied locally, and it seemed to have relieved the stuffy sensation. He had shown slides of the growth under the microscope, and anyone doubtful of its nature should study them.

Dr. JOBSON HORNE pointed out that there was no pathological report before the Section, and as he gathered from the remarks made by Mr. Stuart-Low that there was no dearth of material, suggested that some of it might be submitted to the Morbid Growths Committee, who, if in doubt, would confer with their referee.

This suggestion was agreed to.

Case of Deflected Septum causing Complete Occlusion of Right Side of Nose.

By G. C. CATHCART, M.B.

THE patient is an actor by profession. Although there is complete occlusion of the right side of the nose he says it does not interfere either with his breathing or his voice. He does not desire any operation on the septum unless the shape of the nose can be changed externally.

Case of Lupus erythematosus with Affection of Mucous Membrane of Palate and Mouth.

By H. LAMBERT LACK, M.D.

THE patient, a woman aged about 40, has suffered from lupus erythematosus of the face for some ten or twelve years. She has the typical butterfly-shaped distribution of the disease on both cheeks, and has been under treatment at various hospitals. There is a small patch about 1 in. long and $\frac{1}{2}$ in. broad on the mucous membrane at the junction of the hard and soft palates, and a similar patch on the inner side of the right cheek opposite the molar teeth. These patches somewhat resemble lupus vulgaris; they are very chronic and give rise to no symptoms. The case is shown because of its supposed rarity; I have never met with a similar case.

Dr. Sequeira, who sent me the case from the Skin Department at the London Hospital, states that mucous membrane affections in this disease are comparatively common, apparently occurring in about 28 per cent. Reference to his investigations will be found in a paper published in the *British Journal of Dermatology*, 1906, xviii, p. 59, and Dr. Sequeira tells me that he has seen many similar cases since.

DISCUSSION.

Dr. BALL said that, six years ago, the girl was attending the skin department of a hospital with which he was connected, and he was asked to see her palate. He saw there a fairly typical patch of common lupus. She was shown at a society by the dermatologist who had charge of her as a case of lupus erythematosus of the face and common lupus in the mucous membrane. His own diagnosis at that meeting was not disputed. Dr. Lack did not say what

he considered to be the nature of the patch on the mucous membrane of the palate.

Dr. LAMBERT LACK, in reply, said that he had brought forward the case as several members said they did not know that lupus erythematosus affected the mucous membranes. He did not think the patch on the inside of the cheek was like ordinary lupus. He was told that patches on the mucous membrane of the cheek were more common than on the palate.

Case of Laryngeal Neoplasm exhibited, with Microscopic Specimen of same, prior to Operative Treatment.

By W. JOBSON HORNE, M.D.

THE patient, a woman aged 64, had had cough and impairment of voice, and at times aphonia, since last April. The symptoms were attributed to influenza. The growth occupied the posterior third of the right half of the larynx; it was situated above the vocal cord and between the ventricular band and the arytaenoid region. It hid two-thirds of the right vocal cord from view, and appeared to spring from the ventricle or the ventricular band. It was moriform in appearance. The right cord moved freely, and so did the left. There was no other lesion in the larynx. A piece of the growth projecting beyond the edge of the cord had been removed, and a section had been cut at right angles to the growth for microscopic diagnosis. This section was exhibited. The growth, in the opinion of Dr. Horne, was an innocent one and its nature was that of a sessile papilloma. Dr. Horne intended to eradicate it by direct laryngoscopy and to cauterize the base.

Angio-neurotic Œdema in a Male aged 48.

By CHARLES A. PARKER, F.R.C.S.Ed.

THE patient complained of transient swellings distributed all over the surface of the body and the mucous membranes of the upper respiratory passages. The first attack occurred two years ago, and since then he has had almost daily attacks. The swellings are most marked in the early morning, and gradually subside as the day goes on, disappearing somewhere between 5 p.m. and 8 p.m. The tongue, cheeks, eyelids, soft palate, larynx, and nose are frequently involved, and there is a distinct

history of laryngeal obstruction with noisy and difficult respiration. Externally, the soles of the feet, the buttocks, the arms, and thighs, and the back of the neck are the parts most usually affected, whilst the scalp is also occasionally involved. The swellings on their first appearance are distinctly pink in colour. On examination at 2.30 p.m. on November 6, the face and cheeks showed distinct swelling, the eyelids were puffy, and the uvula, left ventricular band, and the arytaenoids were oedematous. Swollen areas were also observed on the scalp, the flexor and inner surfaces of both arms, and on the flexor and inner surfaces of both thighs. At 5.30 p.m., the time of the meeting of the Section, all these swellings had almost disappeared.

The case was shown on account of the rarity of the condition in the male, the wide distribution of the oedematous patches, and the regularity of the daily attack.

Excision of Half the Larynx for Large Carcinomatous Tumour of the Arytaenoid.

By ARTHUR EVANS, M.S.

OCTOBER, 1906: I was asked to see a patient in the out-patient department who was suffering with intense dyspnoea. On making a laryngoscopic examination a large tumour was seen, almost completely filling the upper aperture of the larynx; its upper surface was so large that it was impossible to make out anything of its lower connexions, but one judged it to be right-sided, as the right side of the larynx was more crowded than the left. The dyspnoea was so marked that operative interference was imperative.

October 8: Operation. An incision was made from just above the thyroid notch to the upper border of the sternum; the cricoid cartilage was not more than 1 in. above the sternum. The trachea was opened and a Hahn's cannula inserted. In about ten minutes the thyroid cartilage was slit up, or rather cracked, with a bone-cutting forceps. The right cord was now seen to be reddish and thickened, and this thickening extended up the right side of the larynx. On putting one's finger up, a large rounded mass could be felt in the region of the right arytaenoid cartilage, and on pulling up the hyoid bone with a blunt hook there was exposed to view the yellowish

tumour which had been seen with the laryngoscope; an incision was now made from the upper end of the median incision outwards on the right side close to the hyoid bone, giving an excellent view of the tumour. It was decided to remove the right half of the larynx; this was accomplished with very little difficulty; all the soft structures were peeled off the right half of the thyroid cartilage, and its posterior, upper, and lower attachments cut through, and finally an incision was made through the soft structures uniting the arytaenoid cartilages; not more than two vessels needed ligaturing. The transverse incision was now sewn up, and the median incision down to the middle of the thyroid cartilage; the remainder of the wound was left open, and through it the cavity of the larynx was filled with iodoform plugging. The Hahn's tube was not working perfectly at the beginning of the operation, so I packed a piece of marine sponge into the trachea above the tube; this sponge I removed at the end of the operation, leaving in the Hahn's tube.

October 9: The patient is sitting up in bed and looking wonderfully well. October 10: The patient is out of bed and is sitting in a chair by the fire. October 12: The patient is decidedly weaker. October 13: The patient has ceased to retain the enemata, and is being fed through an œsophageal tube. October 14: The patient resents the passage of the œsophageal tube and struggles to prevent its introduction into his mouth; the temperature has gone up, he has developed a cough, and the chest reveals signs of pneumonia. I opened up the wound, and through it could then pass the œsophageal tube without difficulty, and with no discomfort to the patient, but the stomach resented the introduction of food, and returned the feed. The patient got rapidly weaker and died the next day.

The patient stood the operation so well, and was so well for the first few days following the operation, that I believe he would have made a good recovery had it but been possible to maintain his strength by regular feeding, and to this end I wish I had left the whole of the large wound entirely open, and from the beginning fed him by means of the œsophageal tube passed through the wound directly into the œsophagus.

The specimen is now in the Westminster Hospital Museum.

Case for Diagnosis.

By G. NIXON BIGGS, M.B.

ON September 3 the patient presented himself to me at the Seamen's Hospital, complaining of hoarseness of eight weeks' duration; no other symptoms. Previous attack one and a half years ago, which got well with treatment. History of syphilis thirteen years ago.

On examination, there is a large swelling situated in the region of the right ventricular band; the cord cannot be seen; the swelling is smooth, and no ulceration can be seen; the left cord is reddened, and the ventricular band on this side is slightly swollen. Both arytaenoid regions are swollen, and there is swelling of the inter-arytaenoid region (irregular). The epiglottis is thickened; no enlargement of lymphatic glands.

Patient was admitted on September 17 and placed on mercury and potassium iodide. His lungs have been examined on several occasions, but no evidence of tubercular disease could be detected. Sputum: No tubercle bacilli (several examinations). Weight has remained the same—9 st. 4 lb.; no elevation of evening temperature; silence has been enforced.

Unfortunately the patient showed signs of mercurial poisoning, and the treatment had to be suspended for twenty-one days. The patient is now again on potassium iodide, taking 20 gr. three times a day; this dose is being increased rapidly. At first there was slight improvement, but for the last four or five weeks the condition has remained quite stationary.

It is proposed to remove a portion of the growth for microscopical examination. Opinions are invited as to the nature of the tumour.

The present enlargement of the lymphatic glands is due to the fact that the patient is just recovering from an attack of acute lacunar tonsillitis.

Laryngological Section.

December 4, 1908.

Dr. WATSON WILLIAMS, Vice-President of the Section, in the Chair.

Case of Immobility of the Left Vocal Cord in a Male Patient, aged 16.

By J. DUNDAS GRANT, M.D.

THE voice has been weak and hoarse for about two years. When seen a month ago, the left vocal cord was found to be immobile in the cadaveric position. A chain of enlarged glands was pressing along the anterior border of the sterno-mastoid. Clinical examination was negative, and there has not yet been an opportunity of making a radio-scopic examination.

DISCUSSION.

Dr. JOBSON HORNE said the larynx did not present the usual appearances met with in recurrent laryngeal paralysis. The complete paralysis of the left half of the larynx reminded him of a case in which the condition had been brought about by direct injury. Enlarged cervical glands were palpable on both sides of the neck; possibly those on the left were exercising pressure. The larynx did not suggest to his mind tuberculosis of the lungs themselves.

Dr. BALL thought it was a case of complete paralysis of the left cord. The enlarged cervical glands might be associated with enlarged glands in the chest, or lower part of the neck, which might press on the left recurrent laryngeal nerve.

Mr. F. J. STEWARD said the question which arose in the case was whether the swelling in the neck was an enlarged lymphatic gland. He had recently seen a case in which a woman was admitted into hospital for a supposed enlarged and painful gland on the left side of the neck, very much in the same position. That was removed, and nothing particular was noticed about the gland at the time; but it was realized that her voice was altered, and, on examination, complete paralysis of her left cord was found. The tumour turned out to be a neurofibroma, which must have been attached to either the recurrent laryngeal or the vagus. It was evident that the lump in the present case extended deeply down towards the carotid sheath, and possibly it might be of the same nature as the case he had mentioned.

The CHAIRMAN (Dr. Watson Williams) said he regarded it as an interesting case illustrating the difficulty in accounting for such paralyses of a vocal cord, especially in so young a patient. It seemed to him suggestive of the existence of some enlarged gland pressing on the corresponding recurrent laryngeal nerve.

**Case of Extensive Intrinsic Epithelioma of the Larynx in a
Male Voice-user, aged 62.**

By J. DUNDAS GRANT, M.D.

COMMENCED with hoarseness, gradual increase during two years; occasional pain left side of neck. Pale, papillated ulcer occupying the greater part of an area of infiltration involving the left vocal cord and ventricular band. Edges slightly everted; areola of congestion. Left half of larynx immobile; right half practically normal. No spreading of the thyroid cartilage, no glandular enlargement. Diagnosis founded on inspection only. No removal of fragment for microscopy has been made. Progress probably very slow.

DISCUSSION.

Mr. DE SANTI said there was no doubt about the diagnosis. With regard to treatment, he regarded the man as a very suitable case for hemilaryngectomy. The whole extent of the disease could not with certainty be known; but it seemed to be limited to one half. Before doing any operation he thought that the patient should be told that it might be necessary to do more, that the whole larynx might have to be removed. But the absence of any considerable glandular infiltration, and the man's general condition and the mobility of the opposite side, pointed to such an operation being effective.

The CHAIRMAN said his impression was much the same as Mr. de Santi's. But until the operation took place it was impossible to be positive that even hemilaryngectomy would suffice to get rid of all the disease, and when the interior of the larynx was exposed it might be found that the disease was too extensive for removal.

**Cure of extensive Web uniting Middle Two-fourths of the
Vocal Cord by Division and Wearing of an Intra-
laryngeal Splint.**

By WILLIAM HILL, M.D.

THE patient was a woman, aged 44. The web had resulted from traumatism (surgical removal of a pachydermia laryngis). The case was complicated by complete fixation of the right cord of twenty years'

duration following an operation for goitre. To avoid an external operation if possible the web had first of all been merely removed through a Killian's laryngoscopic tube, by the aid of Horne's forceps, by Mr. Tilley; the patient would not tolerate the passage of Schrötter's tubes or intubation, and re-adhesion rapidly took place. Dr. Hill then performed thyro-fissure, removed the web, and, by means of the splint shown, kept the fissure open and the cords separated for three weeks until cicatrization had taken place, after which the wound was allowed to heal. The cure was a perfect one.

Stenosis of Larynx in a Child.

By T. JEFFERSON FAULDER, F.R.C.S.

PATIENT, a male child aged $2\frac{1}{2}$, had in October, 1907, a severe attack of diphtheria of fauces, larynx and bronchi. At the end of October, 1907, tracheotomy was performed. It was found subsequently that the child could not breathe without the tube. Three curetting operations were done for adenoids, but some very tough (? fibrous) portions of the growth could not be removed. Thyrotomy was also performed later. Admitted to the Throat Hospital, March, 1908, wearing a tracheotomy tube, the child was found to be unable to breathe without the tube and with the opening in the neck closed. No air passed through the larynx, and there was no phonation. On June 27, 1908, the larynx was examined through a bronchoscope. The lumen of the larynx appeared to be obliterated, and no passage could be found for a fine probe. On August 1, and again on November 12, the child had severe attacks of bronchitis, and still occasionally coughs out thick viscid sputum. At the present time, if the tube be stopped, the child can utter two or three words in a gruff pharyngeal voice, but is still unable to breathe through the larynx.

DISCUSSION.

Mr. BABER thought if it was impossible to find any passage through the larynx, and if anything were done it should be laryngo-fissure, clearing out the cicatrix inside the larynx.

Mr. HERBERT TILLEY thought the treatment should be based on the lines of a paper which recently appeared in the *Journal of Laryngology*,¹ by Drs. Sargnon and Barlatier. Such cases were about the most trying in their department. He had at present a case in hospital in which he adopted their method of treatment. The stenosis was so extreme that only a fine probe could be passed upwards from the tracheotomy opening into the larynx. He first dilated up the

¹ Vol. xxiii, pp. 365, 411, 475, 649.

stricture by Lister's steel urethral bougies, then split the thyroid cartilage and trachea down to the opening of the tracheotomy wound, and, holding the parts aside, sutured the skin to the mucous membrane of the larynx and trachea. The condition originated in diphtheria, when a tracheotomy tube had been inserted through the side of the thyroid cartilage. Some method was necessary to keep the narrow air-way open, and the procedure adopted by Dr. Sargnon and Dr. Barlatier was to suture the skin to the mucous membrane of the trachea, and to place in the trachea a rubber tube plugged with some gauze (to prevent the accidental entry of food) and keeping it for an indefinite time until there was absorption of scar tissue. Dressings had to be changed every day for the first week, and the wound carefully cleansed. The tube had to be kept in from three months to twelve months to get a cure. In his own case the tube had now been in about two months already. The child breathed clearly through the larynx, and in a week the whole thing cicatrized down, and had to be opened up again. Therefore in the present case he suggested that the larynx and trachea should be split and the mucous membrane of these parts sutured to the skin, a rubber tube placed above the tracheotomy tube and kept there until the air-way was sufficiently and permanently dilated. Then the edges of the wound could be freshened, and the air-way closed, and the tracheotomy tube finally removed.

Dr. WILLIAM HILL said the case he showed might be conveniently discussed with the present one. The arrangement he recommended was a simple one. Any nurse or dresser could use the dilating instrument, which was fashioned out of two of Semon's nasal splints bent L-shaped and cut to fit the glottis. In his case the web was divided in the first instance by Killian's intralaryngeal method, and the condition recurred worse than ever. His device could be taken out, washed, and replaced daily, and there was nothing irritating in its use, and he could guarantee that it would keep the glottis open throughout. He remembered many years ago seeing a child suffering from stenosis following ulceration in typhoid, under Chiari, in Vienna; the patient was under treatment fifteen months with an open laryngo-fissure, which was packed. That operator finally turned pieces of skin in from the neck into the larynx and grafted them on each side. He had no information of the ultimate result.

Dr. H. J. DAVIS said he remembered a case at St. Thomas's Hospital in the diphtheria ward who had had tracheotomy done a year before, but could not do without the tube. It was eventually cured by a small celluloid catheter passed upwards through the larynx, the two ends being fixed by the side of the ear. He had to look after the child for six months; it eventually was able to breathe without the tube.

Dr. LAMBERT LACK said he had had many such cases under his care, and he thought all of them were due to the original operation having been a laryngotomy; the tube had been put in through the thyroid cartilage, or through the cricoid. He had never yet seen stenosis follow a proper tracheotomy. He believed the trouble was due to the irritation of the cartilages of the larynx by the pressure of the tube, and now Dr. Hill and Mr. Tilley

suggested putting something else in and keeping it there. He (Dr. Lack) was sure that the right treatment was immediately to perform a low tracheotomy, and leave the larynx quite alone. That might not cure the worst cases, but it would cure many of them. If it did fail the larynx could be operated upon later. The last case of the kind he had was a man aged 20, whose "tracheotomy" was done when he was aged 4, so he had worn his tube sixteen years. The tube had been inserted through the right ala of the thyroid. Dr. Lack opened the larynx and cut away as much of the scar as possible, and did a low tracheotomy. In four months he was able to remove the tube, and the patient was now well and back at his work without any tube. He had originally tried dilating the larynx with metal plugs, but he found afterwards that the cases did better with the treatment he had just described.

Mr. F. J. STEWARD agreed that such cases were mostly due to tracheotomy having been done too high up. Apparently laryngotomy was successful, but before Dr. Faulder submitted the child to that he urged him to give a proper trial to dilatation. By that means he had succeeded in getting five cases well, and it was therefore worth while giving up time and attention to this plan. He believed dilatation was not as a rule given a fair trial. It was not reasonable to expect cure if the dilator was taken out after a few days or weeks and then left out entirely once for all. Such a plan would not be expected to succeed in urethral stricture. The treatment consisted in dilating the stricture, under an anæsthetic, first from the tracheotomy wound, with silver catheters, until the smallest size intubation tube could be got into the larynx. That was left in for two or three days, until the next size could be got in, and so on until the largest size necessary had been introduced. This tube was left in for a few days, then taken out for five minutes the first day, then for ten minutes the second day, then fifteen the third day, and so on, the period during which the tube was left out being constantly but very slowly increased, until eventually it was only in position for one hour in the twenty-four, then one hour in two days, then four days, and so on again till the interval had been increased to weeks and finally to months.

Dr. FURNESS POTTER said another point of interest was the date and cause of the formation of the web. Some time ago the case was shown at the Laryngological Society of London¹ on account of the aphonia, and was examined by most of the members present, when various opinions were expressed. At this time there was no difficulty of breathing; her one complaint was of aphonia, which was thought to be functional. He had examined the patient a number of times, but had seen no sign of a web. There was paralysis of the left cord which dated from the time of an operation performed fifteen years ago for the removal of an enlarged thyroid.

Mr. FRENCH said whatever might be the method of treatment, if Dr. Faulder would give injections of fibrolysin once a week, as well as apply it locally to the fibro-cicatricial tissue in the larynx, he would find that the benefits would be much enhanced. He believed it would answer very well if used in conjunction with dilatation.

¹ *Proc. Laryng. Soc. Lond.*, (1906-7) 1907, xiv, p. 41.

Dr. DAN MCKENZIE said a case of stenosis in which dilatation could not be practised had been under the President's treatment at the Central London Throat and Ear Hospital, and the operation described by Sargnon and Barlatier was performed. Pneumonia developed two or three days after the operation, and the child died, an event which was obviously prone to happen in such cases. So that before proceeding to do such a large operation dilatation should be tried if the nature of the case permitted.

The CHAIRMAN said that, speaking generally, in cases of chronic laryngeal stenosis his own inclination would be in favour of a trial of intubation wherever possible. In the majority of cases, if the stricture was first dilated under an anæsthetic, an intubation tube could be put in, even if only a small one to begin with, a larger one being introduced after waiting for a time, and allowing the patient to wear an instrument, much in the way which Mr. Steward had found so successful. But no hard and fast rule could be laid down, and different methods had to be employed in some cases. He congratulated Dr. William Hill on the successful result of his original and ingenious device. He was glad to hear favourable opinions of fibrolysin, as he had heard very unfavourable views about it, and had not tried it.

Dr. FAULDER, in reply, said he examined the patient through the direct bronchoscope tube, so as to discover the condition of the larynx internally. It was impossible to examine the patient in the ordinary way with the laryngoscope mirror. The epiglottis and aryepiglottic folds were normal, but nothing was normal beyond that. The trouble dated from diphtheria in 1907. Tracheotomy was said to have been done as a matter of urgency, and later thyrotomy, it having been found impossible to remove the tracheotomy tube. Recently the child had been able to make sounds of a sort, but he did not think there was now passage of air through the larynx. He regretted he did not attempt to pass a tube through the tracheotomy wound upwards. He would like to carry out Mr. Tilley's suggestion, but he saw no objection to removing the scar tissue at the time. The skin-flaps would partly keep the wound open, and they would partly line the cavity. In a case of apparent attempt at suicide skin-flaps were turned in, but there was a growth of hair in them. He asked whether Thiersch's skin-graft could be carried out in such a case as the present. The objection to such a drastic operation was that this year the child had had three very severe attacks of bronchitis, in the last of which death nearly ensued. It was probably correct that the supposed tracheotomy in that case was a laryngotomy. Obliteration of the larynx would prevent his dilating in the ordinary way; he must use a cutting instrument first.

Dr. WILLIAM HILL, in reply, said the point of interest to him was not the origin of the web as suggested by Dr. Potter, but how the condition had been absolutely cured. As to the history of the case, there had been alarming bleeding during a previous thyro-fissure for the removal of a growth, and Spencer Wells forceps were put on the vocal cords to stop that bleeding, and perhaps the cords had been abraded by retractors; anyway, a web formed. His celluloid intralaryngeal splint did not irritate the larynx; there were, of course, some

granulations at the fissure at the end of three weeks; these were then removed and the wound allowed to heal. There was no re-formation of the web or bother of any kind. The pressure could be regulated in his device—a point of importance. Though he had only one case to show, he believed the method had a future in these cases where either the patient would not submit to intubation or where house surgeons or practitioners were not prepared to carry out intubation. The cure of the web by this method was both easy and certain, and he thought it would prove useful in certain of those more extensive and intractable cases of traumatic stenosis which had been under discussion, where long-continued intubation was impracticable or undesirable for any reason.

Epithelioma of Palate in a Boy, aged 16.

By J. W. BOND, M.D.

F. W. HAD noticed a swelling on right side of hard palate for five years. The tumour was hard, fixed chiefly to right hard palate, but extended across middle line and also over part of soft palate. It was removed with overlying mucous membrane and the wound cauterized in May, 1908.

DISCUSSION.

Dr. P. MCBRIDE said that when he came to look at the patient he failed to see the epithelioma. There was much cicatricial tissue, so he presumed the epithelioma had been removed. If so, it should have been stated.

Dr. PEGLER said Dr. Bond exhibited a slide, and it was, in his opinion, a section of a typical epithelioma.

Dr. JOBSON HORNE suggested the case should be referred to the Morbid Growths Committee, and that they should be allowed to cut their own section.

Dr. BOND replied that it was difficult to decide whether it was epithelioma or endothelioma. The balance of expert opinion favoured the former. Clinically, it was a very hard fixed tumour, fixed chiefly to the bony part of palate. When it was removed all the parts around were cauterized owing to its suspicious appearance.

Fibroma of Nasopharynx in a Boy, aged 17.

By J. W. BOND, M.D.

B. U. HAD suffered from nasal obstruction for six months. A large mass, extremely hard, was found to occupy the front of naso-pharynx and the right side of nose at the back. The left nose was a mere chink

from pressure on and consequent displacement of the septum. Patient had had no hæmorrhages.

DISCUSSION.

Mr. HERBERT TILLEY said that he noticed the ordinary incision as for excision of the upper jaw had been made. That might have been necessary in this case, but in a patient he showed some years ago in which he did several operations for a recurrent naso-pharyngeal fibroma, a laryngotomy tube was put in, the soft parts of the face were turned up, and the ascending process of the superior maxilla was removed. It was surprising how much room could be obtained by that method, and since the incision was made in the gingivolabial fold, from one malar process to the other, no deformity was produced.

Dr. BOND replied that he thought the operation was easier than it proved to be. He commenced with a "preliminary laryngotomy," and then expected, by splitting the soft and removing part of the hard palate, to remove the whole growth without difficulty, but he found the right antrum crammed with growth, the nasal wall of antrum gone, and the growth protruding more into the nose than was customary. He then made a flap in the face over the upper jaw, and tried to remove the growth through an opening in front of the face, after taking away the ascending process of the superior maxilla and the front of the antrum, whilst leaving the alveolar process and orbital plate untouched. He had finally to take away nearly all the alveolus on the right side to get sufficient access. He found the front of the sphenoid body was largely open. It would have been impossible to get the growth away by simply lifting up the lip and making incisions from below, in manner suggested by Dr. Tilley. The operation had taught him that such growths could not always be removed through the mouth.

A Case of Lupus of the Epiglottis.

By W. JOBSON HORNE, M.D.

THE patient, a girl aged 17, was brought before the Section on May 1, 1908. On that occasion there was a difference of opinions expressed by the members who took part in the discussion as to the nature of the disease. The case was then shown prior to any operative treatment. Since then the gross part of the disease had been removed, and in accordance with the wish expressed on the previous occasion a microscopic section of the part removed was now exhibited, together with the patient, with a view of eliciting further expressions of opinion as to the nature of the disease. The patient had expressed herself as decidedly benefited by the operative treatment.

**Primary Tuberculous Granuloma of the Triangular Cartilage
of the Septum.**

By L. H. PEGLER, M.D.

THE patient, a widow, aged 50, a newly arrived German native, presented herself at the Metropolitan Throat Hospital complaining of nasal obstruction; complexion fresh; well nourished; no signs of tuber-



FIG. 1.

A giant-cell system, close to the necrotic margin of a lobule; numerous giant-cells are seen. About thirty such systems were to be counted in the section ($\times 73$).

culosis elsewhere. Growth formed a lobulated mass attached to and occupying a depression in the left side of the cartilage anteriorly, not only deflecting, but perforating it, so as to be just visible in the right

fossa. The mass was light red, soft, and slightly œdematous, not ulcerated; when blanched by cocaine the lowermost lobule appeared in part almost as blue as a nævus. There was no true resemblance to either lupus, gumma, sarcoma, or malignant growth. The majority of the divisions were snared off *en masse*; the deep soft parts of the base were curetted. Hæmorrhage was very free. Pure phenol was rubbed in, and a gauze dressing applied. A clean healed surface appeared in about three weeks, with a small perforation in the centre. The microscopic sections present a typical series of giant-cell systems, one bordering upon another (figs. 1 and 2). There is no true epithelial covering in the

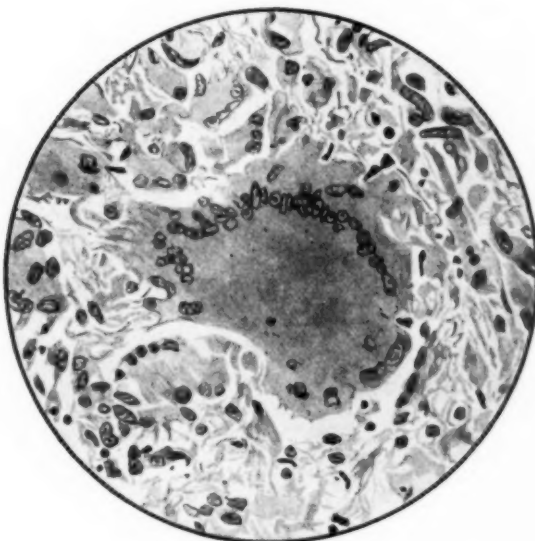


FIG. 2.

More highly magnified view of a giant-cell from a neighbouring system ($\times 365$).

sections that have been made, but the surface is ulcerated in places. Staining for tubercle has given negative results. In this latter, and many other respects, the case is parallel with those described by himself in Steward's paper on "Tuberculosis of the Nasal Mucous Membrane."¹

¹ *Guy's Hosp. Rep.*, 1897, liv, p. 149

DISCUSSION.

Dr. JOBSON HORNE, speaking quite generally and from his own clinical and pathological experience of the subject, pointed out that serious fallacies would creep into their work if they allowed the diagnosis of primary tuberculous disease of the nose to be based upon the presence of giant-cells alone in a microscopic section of the growth. When he had the time formerly to pursue personally his clinical observations to their pathological end, it had been his practice to reserve for animal experiment a portion of the growth which he had removed. When the microscopic sections presented giant-cells but no tubercle bacilli, as in the beautiful section exhibited that evening by Dr. Hemington Pegler, then he (Dr. Jobson Horne) had resorted to animal experiment, and with negative results. In view of the fact that the cure of a disease at times could be so largely determined by the nature of the diagnosis, Dr. Horne contended that the point which he had submitted was of no little scientific value. It amounted to this: that giant-cells were met with in the microscopic examination of nasal growths other than those occasioned by the tubercle bacillus. To the general public the word tuberculosis conveyed a great deal more at the present time than it did formerly. Primary tuberculosis of the nose was admitted to be a rare disease, and in the opinion of Dr. Horne it was not desirable that the prevalence of the disease should be increased by the diagnosis being based upon insufficient scientific facts. As regards the case communicated to them that evening, together with the microscopic section, he considered that neither the clinical facts as printed on the agenda paper nor the histological appearances presented by the microscopic section were in themselves sufficient upon which to base a diagnosis of primary tuberculous granuloma of the nose. The *Proceedings* of the Section were largely appealed to for statistical facts owing to the scientific accuracy imparted to them by the discussions. He therefore hoped that the title of the communication would be amended before being indexed in the *Proceedings* of the Society.

Dr. PEGLER, in reply, said he regretted the patient could not be shown. If that had been possible, he would have been glad to demonstrate the slight residual perforation. Should there be recurrence, and the patient reported herself, he would show her. He had hoped to raise a discussion on the relationship between nasal tuberculosis and lupus, a point which was still debatable, and he would like to add a note which he found in Dr. Watson Williams's last edition of his book, p. 146: "It cannot be said that their pathological identity has been conclusively demonstrated, while their very different clinical manifestations favour the view that lupus and tuberculosis are pathological as well as clinical entities." With that statement he identified himself. He willingly referred the specimen to the Morbid Growths Committee.

The CHAIRMAN, referring to the quotation, said he had since wavered in the opinions he had formerly held on the matter, but he could not discuss the

point, as it would open up two large and vexed questions which they had not time to enter on.

Note.—Owing to pressure of work to be got through at the meeting in very short time Dr. Pegler omitted to make the following additional reply on the subject of diagnosis. This had been established, not upon the presence of a few giant-cells, but a series of typical giant-cell systems, and although tubercle bacilli, for which a further extensive search had been carried out, had not hitherto been discovered, this negative result was unfortunately a familiar experience in similar cases. The clinical features and behaviour, and the microscopical picture, left practically no doubt of the diagnosis, and inspection through a $\frac{1}{2}$ oil immersion showed an absence of any mycelium, streptothrix or other, in sections stained by methods that would have demonstrated such a structure had it been present. With this view Dr. Lazarus-Barlow and Mr. Shattock had concurred. The term "primary" had been employed in the case in the belief that in the absence of any demonstrable signs of tubercle elsewhere in the patient, topical infection had been conveyed to the nose by a finger or similar means.

Case of Spasmodic Cough.

By L. H. PEGLER, M.D.

PATIENT is an unmarried woman, aged 25; no occupation. After having long complained about her throat, she had last January an attack of influenza lasting six days and complicated by a bad cough. On getting about again she noticed that the cough was followed by an involuntary spasmodic noise, which can best be described as a croak frequently reiterated. The symptom has sometimes lasted two hours at a time, or a considerable part of the night, and then been followed by great nervous prostration. If desired to cough, the involuntary croaking is set up, and the mechanism of the act in the larynx can be easily watched by aid of the laryngoscope. Globus and other hysterical indications are present.

DISCUSSION.

Dr. DONELAN said that in many of the cases of spasmodic cough after influenza it seemed to be a symptom of true "grippe" in which only the larynx was involved. How much the globus hystericus counted for he could not say, but many cases of persistent cough among children in influenza epidemics were influenzal, though they were often thought to be whooping-cough. The influenza in the present case required treatment as well as the hysterical condition.

Dr. DAVIS said he would treat such a case by giving quinine and applying a strong faradic current.

Case of Osteo-fibroma of the Maxilla.

By ANDREW WYLIE, M.D.

THE patient, a man, aged 53, consulted me on January 9, complaining of a growth in the palate of twenty-five years' duration. He suffered no pain and very little discomfort; in fact, he stated that beyond a certain amount of anxiety it gave him no trouble, and having grown slowly and gradually he had got quite used to it. Mastication and deglutition were performed without any difficulty, and with the exception of a slight "thickness" or impaired resonance his voice and articulation were normal. He had always enjoyed perfect health, and his teeth were sound. On examination a large smooth oval swelling was seen occupying the whole of the hard palate (see fig.). It was the



Photograph of growth taken immediately after removal, showing the surface where it was attached to the palate.

same colour as the surrounding mucous membrane; it was not tender to the touch, very firm in consistence, and slightly movable on steady pressure. It was apparently attached by a thick pedicle to the left alveolus behind the second molar, and had moulded itself to the hard palate. Without employing any anæsthetic, a stout wire was passed round the tumour by means of the ordinary Wilde's polypus snare, and with the exercise of moderate force the pedicle was cut through with a grating noise suggestive of bone crushing. There was practically no bleeding at the time of the operation, or afterwards, from the stump, which was of about the size of a sixpence, and healed rapidly without any complication.

The exhibitor has postponed showing the case for eleven months to see if there was any recurrence.

Pathological Report by Dr. Wyatt Wingrave.—The tumour has the appearance of a new potato; it measures 5 cm. by 2·8 cm., and weighs 13 grm. The cortical part for a depth of about 5 mm. is firm and tough, and encloses a hard stony core, which reaches the surface at its point of attachment. In structure the cortex is composed of densely-packed white fibres mingled with elongated fusiform cells (fibroblasts). The stone-like core consists of compact bone with relatively small cancellous spaces approaching the character of ivory or petrous bone. In nature it is evidently an osteoma growing from the periosteum of the maxilla to which it was attached. There was no sign of any sarcomatous tendency.

The CHAIRMAN congratulated Dr. Wylie on his interesting case and on the result.

Case of Left Abductor Paralysis in a Woman, aged 38.

By H. J. DAVIS, M.B.

THE cords show no signs of inflammation, but the left cord is fixed in the middle line. The voice is hardly affected. There are physical signs of phthisis at the left apex. The case is either one of early tubercular laryngitis, or the paralysis is due to involvement of the recurrent laryngeal in the thorax. The exhibitor is inclined to the former view.

DISCUSSION.

Dr. DONELAN said the case might be of centric origin, and reminded him of one, five years ago, in a patient who suddenly lost her voice during parturition, as this woman seemed to have done. In his case he was told by the patient's medical man that she had a very severe labour, lasting about twelve hours, and when she had reached that stage when patients are encouraged to cry out, she had no voice with which to do so, and it remained permanently in abeyance from paralysis of a vocal cord. On the other hand it might be associated with tuberculous glands in the mediastinum, though as the patient definitely connected the onset of her symptoms with her last confinement, the other theory might be worth consideration.

Dr. FURNESS POTTER suggested that the case looked more like one of fixation of the crico-arytenoid articulation than one of paralysis, because there was

obvious sign of infiltration on that side, which was very marked. He thought it probable that there was a tuberculous deposition in that neighbourhood.

Dr. STCLAIR THOMSON said he thought the same as Dr. Potter. The cord was not fixed in the mid line nor in the cadaveric position. When the patient phonated there was a gap left. The loss of voice antedated the appearance of phthisis. It would be wiser not to commit themselves about it being abductor paralysis until they were sure of some such cause as pressure on the recurrent laryngeal. It would be better to say simply "fixation of the cord."

The CHAIRMAN said he thought it was probably ankylosis of the crico-arytenoid joint, as there was obvious thickening, especially on the outer side of the cricoid cartilage on that side.

Dr. DAVIS, in reply, said when he first saw the case on October 16th, the cord was fixed in the mid line. As Dr. Ball pointed out, the cord was now in the position of extreme abduction. The swelling on the left ventricular band he believed to be due to chondritis. The voice at that time was scarcely affected, but as she had been coughing a good deal lately, her voice was now much hoarser. He believed it to be commencing tubercular laryngitis.

Case of Malignant Disease of the Tonsil.

By H. J. DAVIS, M.B.

THE patient is only aged 40. Symptoms of two months' duration consist of earache and some dysphagia. The condition at first sight appeared to be one of ordinary enlargement, and the tonsil was guillotined. The piece removed was very hard, and ulcerated posteriorly. Microscopic examination showed "typical squamous-celled epithelioma." With a laryngeal mirror fungation may be observed extending on to the lateral pharyngeal wall. Tongue free; larynx not involved; no glands. Patient has been advised to have entire growth removed by external operation.

DISCUSSION.

Mr. DE SANTI regarded the case as very suitable for the external operation, the external carotid being tied. Cases of the kind shown before the Section, especially those by Dr. Lack, had done very well by that method.

The CHAIRMAN said he took the same view as Mr. de Santi.

Case of Carcinoma and Syphilis of the Larynx.

By H. J. DAVIS, M.B.

THE patient is aged 41. He had syphilis fifteen years ago. The only symptom of which he complains is cough, but he has suffered with his throat for a long time. The interior of the larynx shows evidence of old syphilitic disease, but the right ventricular band and aryteno-epiglottidean fold are invaded by a growth different in character from the tissues in the larynx. A section of the growth shows carcinoma. The patient is willing to have the whole larynx extirpated if this is thought advisable. He is exhibited with this object in view.

DISCUSSION.

Mr. STEWARD asked whether there was any chance that the sections had got mixed up. It might be advisable to have another portion excised and examined.

Dr. W. MILLIGAN said that he would be disposed to procrastinate and to use mercurial inunction and iodipin internally. If after a fortnight's vigorous treatment there was no improvement, the question of total extirpation of the larynx might then be reconsidered. On the whole, he would be in favour of recommending a tracheotomy rather than total extirpation of the larynx.

Dr. LIEVEN (Aix-la-Chapelle) said that at Aix he had seen two such cases, aged 43 and 44 respectively. The second case he saw was a swelling of the right arytenoid bone. He got better, and six months later he came back with the same condition, somewhat ulcerated. In spite of treatment it got worse, and he called Dr. Moritz Schmidt into consultation. An excised piece was found to be epithelioma. No doubt it had been syphilis at first, and had yielded to treatment. Schmidt refused to operate, and the man died six months later. He believed the present case was epithelioma. He did not advise the use of iodipin, the action of which was very mild and too slow in a case like this. He would use iodide of potassium and keep clear of everything else.

Mr. DE SANTI said he would leave the patient alone for the present and watch the effect of iodide of potassium, &c. Should it progress and turn out to be malignant, he would do tracheotomy, because if the whole larynx were removed he could not again earn his living.

The CHAIRMAN thought that after further trial of antisyphilitic treatment, and when the diagnosis of malignant disease was proved certain, the question of operation should be considered, as it seemed a case in which total extirpation of

the larynx afforded some chance of success. Under such circumstances he felt that the position of a patient whose larynx had been removed should be fairly placed before the patient, and if he preferred to accept the risk of operation and the discomforts and disadvantages that were inevitable results, the final decision should lie with him and not with the surgeon.

Dr. DAVIS, in reply, said the case would be different if the man were aged 65 or 70, but he was so very young. With regard to the pathological findings, it was stated by an expert to be "typical carcinoma." The remainder of the larynx was being invaded by the growth, and he did not think less than complete extirpation would be of use.

**Additional Notes and a Letter from Professor Chiari with
reference to the Case of a Lady Baritone shown in
February last.**

By CYRIL HORSFORD, F.R.C.S.

Dr. HORSFORD recapitulated the main points in the case which was brought up at the former meeting,¹ and regretted that there were no satisfactory answers given to the three questions he put to explain the cause of the voice. He reported that, as a result of the improved condition of the upper resonators, which followed the removal of adenoids and the treatment of chronic rhinitis, and also the diminution of the density of vocal cords by the treatment of the laryngitis, the voice gained three or four notes at the upper end of the compass and became lighter in quality, so that she was now described by vocal experts as a tenor. He then read a portion of a letter from Professor Chiari, of Vienna, who described the larynx as follows: "The interior of the larynx was remarkably large; the epiglottis ary-cartilage and ary-epiglottic folds are thicker and more massive than in female larynges. What is more striking is the fact that the lips of the glottis are much broader, though their length did not appear to me abnormal. The inspection of the contours of the throat shows a distinct, though not very pronounced, beak-shaped projection at the incisura thyroidea. . . . This is without doubt a case of a clearly male type." He (Dr. Horsford) did not agree with the view that the voice was caused by a male type of larynx. He

¹ *Proc. Roy. Soc. Med.*, 1908, i, *Laryng. Sec.*, p. 61

had recently obtained the opinion of Dr. Lydia Leney (gynæcologist), who made a complete examination of her genital organs and found them, both externally and internally, normally feminine. In his opinion it was the voice of a permanent laryngitis due to the forcing downwards of the "chest notes" of a contralto. This result is the loss of the middle and upper tones of the voice owing to the strain on the vocal cords. There had arisen permanent congestion and thickening, so that, with increased density, the pitch became lower. He had known contraltos who could at will imitate the male quality.

DISCUSSION.

Dr. DAN MCKENZIE thought it unnecessary to invoke chronic laryngitis as the cause of a baritone voice in a woman. They had just heard that Professor Chiari looked upon the subject's larynx as of the male type. The subsidiary, or secondary, sexual qualities were not infrequently abnormal in this way. Thus there were masculine women and effeminate men; women with hirsute faces and men with roomy pelves. In like manner, women with baritone or manly voices produced by larynges of the male type were by no means uncommon, and now that prominence had been given to the subject the speaker expected that many more cases would be brought to light. It was, further, worthy of notice that the contralto voice in the woman was an approximation to the male type just as the tenor voice in the man was an approximation to the female. The etiology of these abnormalities had aroused considerable interest, and the theory which seemed most plausible might be stated as follows: In the mature sexual gland of the one sex there were vestiges of embryonic structures which represented the sexual glands of the opposite sex; when these vestiges developed sufficiently to supply an internal secretion qualities normally characteristic of the other sex made their appearance. It was, of course, true that the person now under discussion was female, but, obviously, no gynæcological examination could exclude the presence of such elements as he had just mentioned.

Dr. JOBSON HORNE was glad that the case had been brought forward again, and he was particularly interested in hearing the views expressed in the letter read from Professor Chiari. When the case was exhibited to the Section in February, Dr. Horne had ventured to sum up the larynx as one *sui generis*.¹ That observation had provoked some criticism, and he was pleased to find that his views were in accordance with those expressed by Professor Chiari. In view of the two cases of baritone voices in female singers exhibited that evening, he (Dr. Horne) was tempted to ask the question what constituted a baritone voice.

¹ *Proc. Roy. Soc. Med.*, 1908, i, *Laryng. Sec.*, p. 61.

Dr. P. McBRIDE asked whether Dr. Horsford had seen an article in German which dealt with the question of sex as shown by X-rays applied to the larynx and trachea. The method was used especially in a case of hermaphroditism, and the author of that communication thought much of that method of determining the sex.

Dr. HORSFORD, in reply to Dr. Jobson Horne, said he could not easily define baritone in words, for it was determined by a certain pitch or compass, and especially a definite characteristic quality only known to the well-trained ear. He had not read the paper referred to by Dr. McBride. With reference to Dr. Frederick Spicer's case, he had had the privilege of examining her throat and voice before, and he did not consider her quite a baritone, neither in quality nor compass, but that she was an early stage of the condition in his case. It was a large contralto larynx, the evidence of organized changes, the result of forcing being an early singer's nodule on the left vocal cord and rounding of the edges of the cords.

Skiagram of a Case of Empyema of the Frontal Sinus.

By A. L. WHITEHEAD, B.S.

In this case large masses of polypi had been removed, the ethmoids curetted and radical operations on both antra performed; pus continuing to be discharged the frontal sinuses were washed out. With the canula first used some difficulty was experienced in efficient irrigation; the skiagram showed clearly that an unsuitable curvature caused the instrument to impinge upon the posterior wall. A knowledge of the dimensions of the sinuses as indicated by skiagraphy was found to be of the greatest assistance when they were subsequently operated upon by Killian's method.

Dr. STCLAIR THOMSON said he was glad to see the case, as he had shown two similar radiographs at the June meeting of the Society, when they were received with scant consideration. The majority of members said that skiagraphy was quite unnecessary, half of them because their own tactus eruditus told them when a frontal sinus canula had entered the cavity, and the other half because they could rely on their patients' own sensations to determine the position of the instrument. Dr. StClair Thomson confessed that he could not put complete reliance in either of these criteria; and Mr. Whitehead's skiagraph, like his own, explained how invaluable the X-rays were. Killian, Mosher, and all who had had much experience of sinus work were agreed that complete justice could not be done to a patient if, in treating the frontal sinus, use was not made of skiagraphy.

Specimens of Septa in Animals, showing possible Explanation of Abnormalities in Man.

By G. SECCOMBE HETT.

MR. HETT showed specimens of the septa in various animals, including those of the dog, cat, sheep, and rabbit. In these animals bilateral projections from the septum occur, and this is especially well marked in the case of the dog. In this animal there is a ridge on the septum near the floor of the nose, which approximates to the inferior turbinate. This ridge is exactly comparable to the spur found as a variation in man in the same situation. This ridge in the dog extends backwards along the cartilaginous and bony septum, and at the latter it is met by a bony projection from the palate bone in the line of the inferior turbinal, with which it meets and fuses. The inferior meatus of the nose in this animal then is roofed over by the approximation of the ridge of the septum with the inferior turbinal, and in front of this with a ridge which is prolonged from the inferior turbinate anteriorly, while behind the spongy portion of the inferior turbinate, which occupies the middle third of the anteroposterior length of the nasal fossa, there is a complete roof to the inferior meatus formed by the fusion of bony processes from the inner and outer walls of the nasal fossa. Spurs, unilateral or bilateral, occur in man as a common abnormality quite apart from any septal deviation, and may cause no symptoms unless combined with the latter, or when owing to undue narrowness of the nares, or hypertrophy of the inferior turbinal, nasal obstruction is produced. It has occurred to me that cases of obstruction of the posterior nares such as that recently shown by Mr. Harold Barwell may possibly be due to a condition occurring as a variety in man, similar to that found in the posterior part of the dog's nasal fossa, and this is borne out by the fact that in Mr. Barwell's case, and also I believe in others, there is an adhesion which is sometimes partly bony, between the septum and the posterior end of the inferior turbinate.

DISCUSSION.

Dr. DAN MCKENZIE said Mr. Hett had suggested that septal spines were, possibly, vestigial structures, normally present in certain of the lower animals. But it was generally reported that septal deviations and spines were commoner in the white civilized races than in the coloured and uncivilized races; and this, if correct, militated against Mr. Hett's views.

Mr. HETT, in reply, said that he was sorry that he had had no opportunity of comparing the frequency of septal spurs in the coloured and white races, but that he believed that Dr. McKenzie was right when he said that septal deviations were less common in the former. With regard to spurs, however, we know that in white races, especially in individuals with broad noses, they often cause no symptoms, and that in the negroid races with their broad nasal cavities the presence of spurs would be less likely to cause symptoms, and so not to be brought under observation.

Frontal Sinus in a Female Patient 14 days after a Killian Operation.

By STCLAIR THOMSON, M.D.

THE patient suffered from suppuration in the left frontal, ethmoidal and maxillary sinuses. The maxillary cavity was drained from a tooth socket; the ethmoidal region was well cleared away at several sittings; the frontal sinus was repeatedly washed out. The sounding of the sinus was controlled by the Röntgen screen. The sphenoidal sinus was explored and found to be healthy. Owing to the persistence of headache the patient begged for a radical operation on her left frontal sinus. This was carried out on Thursday, November 19. The radiograph proved most useful by showing that the left frontal sinus crossed the middle line, and that there was an orbito-ethmoidal gallery running outwards behind the bridge. The wound healed by first intention. There was no diplopia; all headache has ceased, and there has been no discharge since the gauze drain was removed. As the maxillary suppuration persisted the alveolar plug was abandoned a week ago, and an opening made into the cavity from the nose. The case is shown to illustrate the rapid and complete relief of symptoms which is obtained in successful cases without any disfigurement.

The CHAIRMAN congratulated Dr. Thomson on the result of the operation, which had apparently cured the disease without leaving any cosmetic defect.

A Woman, aged 18, with a Baritone Voice.

By FREDERICK SPICER, M.D.

PATIENT is a healthy, well-made young woman. She commenced to menstruate when aged 12 and has been regular ever since. She started

to sing, when aged 14, in private; her voice has "always been the same," and at school, when aged 10, she was classed as second alto. In the larynx there is nothing abnormal except a slight thickening on the left cord, there are adenoids in the post-nasal septum, and in the left nostril there is some obstruction due to deviation of the septum to that side.

Case of Tubercular Laryngitis.

By WILLIAM HILL, M.D.

THE patient was a man, aged 49, with well-marked tubercular tumefaction and ulceration of the larynx, with insignificant physical signs in the chest. Dr. Hill demonstrated the laryngeal lesion through Brunnings's new gutter-shaped spatula by *direct vision* laryngoscopy, and nearly forty members were enabled to rapidly inspect the lesion without the delay and discomfort to a tubercular patient inevitable when examined by indirect laryngoscopy.

Case for Diagnosis.

By WILLIAM HILL, M.D.

THE patient is a woman, aged 40. Ulceration of the epiglottis and general laryngeal tumefaction. No evidence of tubercle in lungs and sputum.

Sarcoma of Tonsil and Soft Palate.

By P. R. W. DE SANTI, F.R.C.S.

THE patient is a man, aged 50, with a history of sore throat of two and a half years' duration. He has now been under my care some two months with a large, hard, fairly painless swelling of the right tonsillar and palatal regions. When first seen the swelling resembled a large

gumma or inflammatory mass, but the history and consistency of the tumour pointed to a diagnosis of a fibro-sarcoma. The patient was losing flesh, had pain and difficulty in swallowing, and was anxious to undergo any treatment that was likely to rid him of the pain. He appears to have been shown before the Society by Dr. Jones in June, but no diagnosis was given by Dr. Jones, nor was any opinion expressed by members. My own opinion is that the growth is a slowly growing fibro-sarcoma and inoperable. I have had him in the hospital under me for one month, and injections of Coley's fluid, $\frac{1}{2}$ m to $1\frac{1}{2}$ m, have been made over the upper part of the neck just at the level of the angle of the jaw. He has generally reacted strongly to these injections, and much glandular swelling has followed, but pain has been relieved; swallowing of solids has become possible, and slight increase of weight noticed. The growth, however, is no smaller.

Epithelioma of the Fauces.

By W. JOBSON HORNE, M.D.

THE patient, a man aged 64, a fitter by trade, had had good health, with the exception of recent attacks of bronchitis, up to October, 1907. In the following March he experienced pricking sensations in the right side of the throat with pain on deglutition and a sensation of choking; recently deglutition had been associated with regurgitation through the nose. He was greatly troubled by the collection of phlegm in the throat. There had been no hæmorrhage. Syphilis was contracted thirty-six years ago. The uvula was ulcerated, its surface red and granular. The faucial pillars on the right side had been destroyed, and occupying their site there was a chasm in which the posterior wall of the pharynx on the extreme right participated; the base of this cavity was yellow and sloughy. The right side of the hard and soft palate was occupied by a smooth swelling, the mucosa of which was red and intact. The lymphatic glands at the angle of the mandible on the right side were large and very hard. The case in the opinion of the exhibitor was inoperable. Patient had derived benefit from the administration of iodide of potassium.

Papilloma of the Fauces.

By W. JOBSON HORNE, M.D.

THE patient, a man aged 35, had experienced a bad throat for about six weeks. He was a painter by trade. When first seen some nine days previously the fauces presented a general pharyngo-tonsillitis. Attached to the anterior pillar of the left side was a pedunculated papilloma. The case was exhibited as illustrating a true papilloma of the fauces in contradistinction to the excrescences of tonsillar tissue at times described as papillomata.

Laryngological Section.

January 8, 1909.

Dr. DUNDAS GRANT, President of the Section, in the Chair.

A Discussion on the Modern Treatment of Syphilis, especially in regard to the Upper Respiratory Passages.

Opened by W. A. LIEVEN, M.D. (Aix-la-Chapelle).

WITHIN the last few years considerable progress has been made in the diagnosis of syphilis. The microbe of syphilis, the *Spirochæta pallida*, has been found, and we now know from the investigations of Wassermann, Bruck and Neisser that the serum of a syphilitic contains bodies which are only to be found in people infected with syphilis. On the other hand we have not progressed very far as regards the means of fighting against this disease. Mercury and iodide are still the most important, if not the only reliable, remedies. The experiments in serum-therapy have unfortunately all failed, and the preparations of arsenic which have been employed during the last two years are neither reliable in their results nor are we acquainted with the exact indications for their use. I will speak first of the various antisyphilitic drugs, and in the second place give an outline of the methods of their employment.

Mercury still remains the most important remedy. Neisser in Batavia studied the influence of all the different drugs which are employed in the treatment of syphilis upon a large number of anthropoid apes and monkeys, and the results showed (1) that mercury not only assists the organism to defend itself against the spirochæta, but also kills the microbe; (2) that inoculations made from the internal organs of a syphilitic monkey which has been sufficiently treated with mercury

are incapable of transferring the virus to another animal; (3) that a syphilitic monkey which has been brought sufficiently under the influence of mercury to effect a cure can be reinfected a second time with syphilis. I should also like to remark that similar results can be obtained by the use of atoxyl. Unfortunately the human race cannot tolerate such large doses of atoxyl without danger to the optic nerve. All these facts afford theoretical confirmation of the experiences of daily practice.

There are different methods of administering mercury to the organism. It may be given by the mouth, when absorption is carried out by the gastro-intestinal tract, or injected by the subcutaneous or muscular tissue, whilst in the case of inunctions the skin and the lungs participate in the absorption of the metal into the system.

As regards the treatment of syphilis by the mouth, in Germany we are all agreed that this method is insufficient to obtain a satisfactory result. I have not only seen a great many relapses of a secondary and early tertiary character, occurring during the first two years of the course of the disease whilst mercury was constantly being taken, but during seventeen years' practice in Aix-la-Chapelle I have noticed that after no other form of treatment were there so many severe symptoms in connexion with the central nervous system as after a pill cure, as it is still carried out in England. In addition to its inefficiency in combating the disease it has the great drawback of very easily producing a chronic enteritis, and when taken for any length of time a chronic stomatitis of a most distressing character. The stomatitis due to rubbings or injections is generally shown by a dark red mucous membrane, whilst the gums, during treatment by the mouth, often present a pale and dirty appearance, and exhibit a tendency to considerable retraction. At the same time this form of stomatitis is not by any means a sign that there is an energetic action upon the syphilis, because it is not a rare occurrence to find recent mucous patches on the mucosa of a patient who is suffering from this particular variety of stomatitis. The theoretical investigations made by Bürgi and others furnish an explanation for this. He found that the mercury is rapidly eliminated and disappears from the urine within a few days after the administration of it is stopped.

The second method of treatment of which we must speak is that of hypodermic injections. We inject either soluble preparations of mercury or insoluble ones suspended in an oily medium. The soluble ones were introduced by Lewin, of Berlin, in 1867. His custom was to inject a

1 per cent. solution of corrosive sublimate. Since then a great many different preparations have been employed which it is impossible to describe here. I am in the habit of using the French "Bijodure de Mercure" (2 per cent. solution), because I find it practically painless and its effect as good as any of the others. An injection of 1 c.c. or 15 μ is given daily for twenty to thirty days. There are two drawbacks to the administration of soluble salts of mercury: (1) The necessity for seeing the doctor every day, and (2) the fact that the mercury, though effective at the time, very soon disappears from the system. Relapses occur more frequently than after the insoluble preparations.

Of the latter the following are in general use: Calomel, grey oil and salicylate of mercury. The two first are injected in a 40 per cent. strength. One employs a special syringe (Barthélémy's), and in the case of calomel injects up to ten divisions once a week, or if *huile grise* is used up to fourteen divisions once a week. One division of this syringe is measured to contain one centigramme of mercury of a 40 per cent. preparation. The syringe is indispensable for calomel and grey oil, as it is impossible to give exact doses with the usual Pravaz-syringe. Six to eight injections of calomel, ten to twelve of grey oil, and eight to ten full Pravaz-syringes (1 c.c. or 15 μ) of salicylate, are considered sufficient for a full course. There is no doubt that these injections have a great many advantages. We are certain of the quantity the patient is getting, and the treatment can be carried out without giving rise to any suspicion in the patient's surroundings. The method also is convenient, as the patient requires to see his doctor only once or twice a week. Last, but not least, there is the fact that the results of this kind of treatment are very satisfactory.

The most effective of the three preparations is calomel, whilst the grey oil has a much slower, but generally sufficient, effect. Between these two stands the salicylate of mercury, which it is right to say is used with excellent results by many physicians in Germany. Its curative effect is sufficient, it is generally painless, and there seems to be hardly any danger in its administration.

This leads me to speak of the disadvantages of the insoluble preparations. Calomel is extremely painful, although this drawback has to a certain extent been overcome in Levy-Bings and Neisser's new suspensions. Aseptic abscesses are common, and one sometimes observes that two or three days after such an injection the patient has an attack of shivering followed by some fever. This attack is called by the French "la grippe mercurielle." Against these injections is to be urged

the fact that when once an injection is made the medical man is no longer in a position to influence the degree of rapidity of the absorption. When this takes place too rapidly severe symptoms may appear. It is possible that a dose of an insoluble salt of mercury may lie dormant for months, and then pass rapidly into the circulation, giving rise to urgent symptoms. Even fatal results have occurred from this cause. The salicylate seems to be practically without danger, and I therefore consider it the only preparation fit for the routine treatment of syphilis, whilst I reserve calomel for the malignant forms, in which a rapid result is requisite, and everything else has failed.

Whichever insoluble salt we employ, it will always be most important that the "technique" should be carried out most carefully. First we should introduce the needle, in order to ascertain whether a vein has been penetrated. For the same reason I always inject into the upper part of the buttock, above a horizontal line joining the tops of the trochanter major, and preferably in the upper and outer quadrant. Under no circumstances whatever should injections be given in cases of diabetes or Bright's disease.

In our country, owing to the many disadvantages, if not dangers, associated with injections, the majority of the profession prefer treatment by inunction. The teachings of Sigmund have made it clear that inunctions can be carried out without necessarily producing mercurialism. The evolution of the present method of inunction is due to these principles. At Aix-la-Chapelle the routine of rubbings has especially been brought to perfection, and it is due to that place that this treatment has regained its reputation on the Continent of being the method which combines the greatest therapeutical effect with no danger whatsoever to the patient, because one can at any time stop the absorption of mercury by simply washing off the ointment. I admit that the treatment by inunctions is difficult to conceal from the patient's surroundings, and that the cure deserves the name of being dirty and disagreeable, but in spite of this its advantages are so great that whenever I have asked a medical man in our country what kind of treatment he would prescribe for himself, there was no one who did not reply, "Inunction."

Inunctions, however, ought to be carried out very carefully. The process is as follows: With the bare hand the requisite quantity—*e.g.*, 4 grm. to 5 grm. of 33 per cent. grey ointment—is evenly rubbed into the skin by the patient himself for a period of at least twenty minutes, and after this time, if the inunction has been properly carried out, the

skin should appear dry and not shiny. On the first day both calves are rubbed; on the second, the left; on the third day, the right thigh; on the fourth day, the abdomen and flanks; on the fifth day, both arms. During these five days no bath is taken, and there should be no change of underwear. On the sixth day the whole body is thoroughly washed with soap in a bath. These courses of five days are repeated again and again until the requisite number of rubbings has been reached. If there is a reliable masseur at hand, both thighs should be rubbed on the second day, and the back on the fifth day. This is the best way of carrying out the cure at home or at the hospital.

At Aix, however, we enable the patient, by the use of the thermal waters, to absorb and to tolerate much larger quantities of mercurial ointment than those mentioned above. The daily rubbing is preceded by a sulphur bath of about 95° F., in which the part which is to be rubbed later is carefully washed with soap. The alkaline water softens and removes the superficial layers of the epidermis, thus opening the pores for the absorption of the ointment. The temperature of the bath is not high enough to remove the ointment in the bath unless it is washed off with soap. The mercury does not penetrate the normal skin, but is rubbed into the pores, where it is converted by the secretion of the sebaceous and the sweat glands into combinations capable of being absorbed by the system, and when absorbed circulates in the body as an albuminate. It is also absorbed by inhalation. The baths and drinking waters promote metabolism, thus giving the mercury an opportunity to enter into new combinations with fresh albumin, and consequently to be distributed to its fullest extent throughout the system.

The two most unpleasant symptoms of mercurialism—namely, stomatitis and colitis—may be observed with every form of treatment, but they can be almost certainly prevented by careful attention being bestowed on the buccal cavity and on the regulation of the bowels. During the cure the teeth must be cleansed after each meal by means of a soft tooth-brush with a tooth-paste of salol and chlorate of potash. The mouth also must be rinsed every hour with a solution of aluminium acetico-tartaricum. Mercurial ulcerations of the gums are cured in a few days by being touched with a concentrated solution of chromic acid. It is essential to remove all debris from the space between the gums and teeth before painting. Naturally, careful rinsing of the mouth after the application of so strong a drug is essential.

The intestinal tract should be attended to, and care should be taken that the bowels are thoroughly opened every day in order to eliminate the mercury secreted into the intestines. The sulphur water at Aix is often sufficient to ensure a daily motion, and the hydrogen sulphide in it acts upon the mercury so as to produce the comparatively innocuous black sulphide, thus reducing the irritation of the gastro-intestinal mucosa to a minimum.

In cases where diarrhoea occurs, even if associated with blood in the stools, a dose of 25 drops of laudanum, if necessary repeated again in eight to ten hours, and the removal of the ointment by means of a soap bath, will certainly stop the diarrhoea.

The second remedy—namely, iodide—is more useful in tertiary manifestations. Its chief action consists in promoting absorption of specific neoplasms which characterize this stage. One, however, obtains good results with iodide in certain secondary symptoms, such as vegetating patches, which are chiefly found at the entrance of the nasal passages and on the floor of the mouth. The preparation which works most quickly is iodide of potassium, and no other preparation of iodine equals it in effect, so that it should invariably be used in cases of imminent danger to life or important functions, or in those cases in which a rapid diagnosis is required, as, for instance, in the case of a growth of doubtful character. Unfortunately, very often this drug or other preparations of iodine cannot be given on account of the occurrence of severe iodism. In these cases a daily prescription of 15 gr. of sulphanilic acid in 7 oz. of water will be found most successful in preventing this unpleasant symptom.

If the patient should be quite unable to take iodide of potash by the mouth, I prescribe sajodine, 3 to 8 tablets of $\frac{1}{2}$ grm. per day. It has a mild and prolonged effect, and is eliminated more slowly than iodide of potash. This is still more the case with subcutaneous injections of iodipin, which I inject in doses of 20 grm. to 30 grm. three times a week. The total course is 250 grm. This quantity is sufficient to put the system under the continuous mild effect of iodine for about six months. This method, therefore, is of great prophylactic value, and very useful when the patient cannot be trusted to take iodide by the mouth as regularly as he ought.

As you all know preparations of arsenic have recently been tried in the treatment of syphilis. The sodium amyl-arsenate (atoxyl) has been given up in our country on account of the repeated occurrence of

unpleasant symptoms; several cases may be mentioned in which total atrophy of the optic nerve resulted. Researches undertaken with the object of finding another less poisonous preparation of arsenic have led in England to experiments with soamin (sodium para-aminophenyl-arsenate), and in Germany to experiments with arsacetin (acetyl-anilarsenate), which is similar to the orsudan of Burroughs and Wellcome.

The number of cases treated with these two preparations is not sufficient to induce me to recommend the use of these drugs in general practice. I think Neisser is right when he says that only perhaps after twenty years of experience with an enormous number of cases shall we be entitled to say anything about the real therapeutic value of arsenic.

After this scheme of general treatment I should now like to speak about the time when treatment should be commenced, and the duration of the course requisite for a complete cure. I am of opinion that in the case of a sore which is suspected to be of a primary nature the diagnosis ought to be certain before treatment is commenced. This is especially necessary, because chancres of the upper respiratory tract are sometimes extremely difficult to diagnose. If the spirochæta cannot be found one must wait a few weeks for the appearance of the roseola or examine the case by the serum test, which, however, is unfortunately only to be obtained several weeks after the appearance of the chancre. An exception should be made in those cases where the doubtful sore is in a prominent position, or where the patient comes into close contact with other people.

For the first course I generally prescribe 40 to 50 rubbings of 5 grm. each. At Aix, if the patient stands mercury well, I let him have two rubbings per day of 4 grm., rising to 5 grm. during the last fortnight. If he is anæmic I give him three to four injections of sodium arsenate three to four times a week. Though the primary sore and secondary eruptions generally disappear within a fortnight after commencement of the cure, it ought always to be carried out to its full extent. The care of the mouth should be continued for a fortnight after each course. Three further courses of rubbings should follow at intervals of six months. If there has not been a relapse during the first year of treatment the fourth course may be postponed until the end of the second year. I wish to lay stress upon the necessity of carrying out these treatments even if a relapse should not have occurred.

Should it be impossible to give inunctions on account of social reasons or owing to the irritability of the skin, I substitute eight to ten injections of salicylate of mercury in vasenol. When rubbings cannot be given and intramuscular injections are not tolerated, then and then only do I decide to give internal treatment. I am in the habit of prescribing the tannate of mercury in doses of 1 decigramme three times a day in pills. These are taken for six weeks, followed by an interval of two months' rest, and so on for a period of two years.

Before speaking about the local treatment of the different forms of syphilis in the nose, mouth and throat I should like to say that in most cases general treatment suffices, and local methods can be dispensed with. If the chancre is situated on the lips or on the outside of the nose it ought, for cosmetic and prophylactic reasons, to be covered by a mercury plaster (Beyersdorff No. 15). For chancre inside the nose an indifferent spray, followed by the insufflation of nosophen, is sufficient in every case. Chancres in the mouth or pharynx ought not to be cauterized, as they heal very quickly under general treatment, but they may early require the dusting on of some orthoform, as they are extremely painful.

Relapses of a secondary type on the mucous membranes are generally more resistant to treatment than lesions occurring at the commencement of the disease, which as a rule promptly yield to general treatment. The most useful remedy for patches of the *erosif* or "ulcerated" type (Fournier) is the application of a concentrated solution of chromic acid; the mucosa ought to be carefully dried before the drug is administered in order to prevent it from spreading. If a very adhesive scab is required one should paint over the chromic acid 10 per cent. solution of silver nitrate. The scab will then become bright red in colour, owing to the formation of silver chromate. Some patients with ulcerated patches on the edges of the tongue suffer considerably from profuse salivation, which easily loosens the scabs produced by cauterization. The giving of belladonna will in many cases minimize this. Mucous plaques of the introitus narium or cracks at the angles of the mouth should, after cauterization, be covered with 5 per cent. white precipitate ointment.

If shortly after an energetic course of treatment a relapse occurs with secondary symptoms on the mucosa, it is not wise to at once give another full course of treatment. It is often possible to overcome these manifestations simply by local treatment. Should this not be followed by success another mild treatment may be advisable, such as wearing a

mercolint flannel on the chest or taking mercury tannate pills for three to seven weeks. By these measures it is practically always possible to keep the patient going until the time arrives for beginning the next full course of rubbings, as specified in our scheme. Smoking is very often the reason of the constant relapses, and you will often find that from the moment the patient abstains from tobacco the patches cease to appear.

In any case one should try to keep as near as possible to the chief courses of treatment as prescribed in the scheme. It stands to reason that extremely severe cases may make it necessary to give the patient more frequent treatments. At the end of the first year my patients begin to take iodide. I give 10 gr. three times daily for three to four weeks, after the third and fourth cure. Or I give a course of injections of iodipin up to a total of 250 grm. during the inunction treatment itself.

The laryngologist does not often have an opportunity of observing a case of syphilis from the beginning. He sees more cases of a tertiary nature, where iodide is the sovereign remedy for a quick result. Thirty grains daily ought to be given at least. If one of the substitutes of KI is to be given one must bear in mind that a favourable result cannot be expected so quickly. In ulcerated *tertiaries* I always give iodide of potash *alone* for four or five days, and only when, from its appearance, the ulcer shows a tendency to healing, do I start inunctions. After the tertiary ulcers are healed iodide is stopped and iodipin injections are employed, whilst the rubbings are continued. We expect the inunctions and iodipin together, on account of their prolonged effect, to prevent relapses, which iodide of potash alone often fails to do.

In addition to tertiary cases of a regular type, there are others that from the very beginning show a malignant character. They sometimes show themselves a few months after the primary sore; sometimes they even develop from persistent secondary eruptions. The malignant form of syphilis is often not cured by ordinary courses of mercury and iodide, but gets worse and worse. I have seen cases of ulcerations in the upper air-passages which after each dose of iodide showed a sudden breaking down of the infiltrated tissue, and when the iodide was stopped the reaction ceased.

In these desperate cases it is advisable to try injections of calomel, which should not be given at the same time as the iodide. After a few injections of calomel I have seen a marvellous improvement take place, and this was followed by the extraordinary fact that

afterwards these cases could be treated in a similar manner to ordinary tertiary cases.

For local treatment I recommend mercurial plaster for all ulcers of the lip and the outside of the nose; for those in the nose itself tampons, covered with a 10 per cent. ointment of eucrophen, have a cleansing and healing effect. The crusts are also easily removed by these tampons. If the granulations of tertiary sores are not sufficient, they ought to be painted with tincture of iodine. This is often required on the posterior wall of the pharynx, as there is very little tendency to healing owing to the very poor vascularization of this part. Sequestra ought not to be dealt with until they become loose. It may be necessary to cut them into several pieces by nasal forceps if they are too large to be pulled out in one piece. In two cases I made an incision in the naso-labial fold in order to remove a large piece of bone. The extraction, even of large sequestra, is generally comparatively easy, because the septum is, as a rule, partially destroyed before the other parts of the nose become involved, and thus more room is afforded for handling the necrosed bone. I will not refer here either to the treatment of perforations of the hard and soft palate or to the plastic operations or paraffin injections which are employed in the treatment of deformities of the nose.

Adhesions of the soft palate to the posterior wall of the pharynx ought to be separated if there is enough of the muscular tissue left to ensure the closure of the naso-pharynx during deglutition. If the patient is unable to do this after the operation he is worse off than before, for his voice retains an abnormal character, and in addition to this all food passes into the naso-pharynx and the nose. Several forms of apparatus have been introduced to keep the soft palate separated from the posterior wall of the pharynx after operation; a vulcanite plate fixed by a spring to the teeth, or an indiarubber ball lying in the naso-pharynx, which is filled with air by means of a tube passing through the nose. A case exhibiting a good result was shown at this Society, the apparatus used being a lead plate fixed by strings which passed through the nose and mouth.

Tertiary processes in the larynx require energetic general treatment, as there is a marked tendency to cicatricial narrowing of the larynx in these cases. We must bear in mind that fibroid metamorphosis may result from infiltration of the larynx without any sign of ulceration. The methods of dilating these contractions are the same as those employed in the treatment of narrowing of the larynx from other causes.

I wish to draw your attention to the rare tertiary cases, first described by Sir Felix Semon, in which warty excrescences appear in the larynx, and if localized on the vocal cords may cause considerable dyspnœa. These new formations ought not to be treated by local measures or by operation, as they always yield to general treatment, though I do not wish to hide the fact that an energetic and prolonged treatment is necessary.

Last but not least I should like to draw your attention to the necessity of treating the swellings of the local glands, which nearly always persist to a certain degree after the general treatment. I advise the patient to massage these swellings with $\frac{1}{2}$ grm. of blue ointment before going to bed for a period of a fortnight. This should be repeated from time to time.

DISCUSSION.

Colonel LAMBKIN, R.A.M.C., said that nearly thirty years ago he had an introduction to Dr. Brandis, whose memory was now as green at Aachen as it was then; he was looked on as the father of the kind of treatment now being carried out there. He (Colonel Lambkin) was converted to the idea that the true channel for the introduction of mercury in syphilis was through the skin. He came away an enthusiast for inunction, as he had seen most extraordinary results, and he had since endeavoured to carry it out. But he found it very uphill work, and, indeed, to carry out the Aachen method in England was almost impossible. In the Army, although the non-commissioned officers were trained to do it, and were instructed to see it done, unless one superintended it personally it was shirked. The actual rubbing lasted for about twenty minutes, and was very hard work. But at Aachen the rubber's bread and butter depended on the thoroughness with which he did his work. The treatment was also very difficult in civil life in this country. Recently he was at Bath, and medical men there told him it was almost impossible to get the masseurs to do it; they had an idea that the process would give them syphilis. It was undoubtedly dirty, it took much time, and it could not be kept secret. Otherwise, he had nothing to say against the method, provided it was always carried out in the same way as at Aix. In England a man was often given a drachm of blue ointment and told to rub it into his groin once a day. That was simply playing at the treatment. At the Rochester Row Military Hospital

it could be seen carried out in certain cases, particularly where there was induration at the site of infection which could not be got rid of by other means. He had always regarded it as most important to get rid of indurations at site of original sore, and now that the *Spirochæta pallida* had been discovered, the reason of that would be obvious. He did not think the method could be carried out as a routine procedure. He had long since abandoned internal medication for syphilis, and his own regular method was now intramuscular injections. With creasote and camphor, calomel could now be used fairly easily and without much pain, and especially if combined with palmitine. But he found that syphilis returned more frequently after the use of calomel than after using metallic mercury. The patient had injected $\frac{3}{4}$ gr. of calomel once a week for four weeks, when in most cases all symptoms would have disappeared at the end of the fourth week. The patient was then put on metallic mercury. Probably metallic mercury was much more slowly absorbed, but, as with calomel, once it was injected one lost control of it. Still, he had never yet had an abscess from the treatment, nor seen embolism in the many years during which he had used the preparation. It was injected into the upper third of the buttock, on one side once a week, and on the other the following. The needle he used was much shorter than that shown by Dr. Lieven, and the shorter needle caused less pain, and pain was the one drawback of the intramuscular method. He agreed with Fournier that "the fear of" the needle was the thing which drove most patients away. It had been said by many that the injection operation should be done in two stages: first the needle should be put in to see if any blood came out; if so, then it should be withdrawn and put in at another spot. Though theoretically this is correct, he did not think it ought to be carried out, because he had never seen any bad effects from not adopting that precaution, and because every separate puncture, of course, increased the pain, and a patient to whom it had been done three or four times would probably not return. With regard to lesions in the upper air-passages, and their local treatment, he thought enough use had not been made of the curette. In lesions of the pharynx, tongue, and other mucous surfaces of the mouth, he used a 20 gr. to the ounce solution of chromic acid, and if they did not soon respond to that, he at once curetted. In treatment and preventing recurrences, he had found very great improvement from the amyl-arsenate salt, even more than from calomel. During the one and a half years it had been used at Rochester Row it was noticeable how few cases of sore throat there had been. Even now, when the hospital was stocked with new patients, about seventy, the chief feature was the absence of lesions of the upper respiratory passages, especially the throat. He had abandoned the use of atoxyl itself; he had not had bad effects from its use, but when he was in Uganda recently he had seen some cases which he did not quite like following its use. He therefore used soamin or amyl-arsenate preparation which was made by Burroughs and Wellcome. Latterly, on the advice of Professor Neisser, whom he met at Sheffield, he had used arsacetin with even better results,

manufactured by Ehrlich, of Frankfort. It had the advantage over soamin that its solutions did not decompose. The solutions of soamin decomposed in twenty-four hours.

Sir FELIX SEMON said he was very pleased to see that Dr. Lieven's very large experience corroborated his own—viz., that the treatment of syphilis by small doses of mercury given by the mouth was not protective against secondary and tertiary syphilis. That was against the doctrine of Sir Jonathan Hutchinson, and against the view which was still prevalent in this country. Almost every patient he had seen with severe tertiary syphilis of the upper air-passages had had mercurial treatment in the early stages of the disease by the mouth, and not a few for the two years which Sir Jonathan Hutchinson laid down. He had also been pleased that Dr. Lieven said that in syphilis of the upper air-passages, as a rule no local treatment was needed, as that had been his (Sir Felix's) own experience. He had been surprised to hear from Colonel Lambkin that even in early secondary lesions he had recourse to the curette, as he had never found such energetic interference necessary, although he did, of course, not deny that in many cases local treatment was indispensable. He was glad that Dr. Lieven had referred to early malignant syphilis. His personal experience was that enough was not known on that point. If in cases of that kind, sometimes as early as ten months after infection, violent symptoms of stomatitis, gingivitis, deep ulceration, &c., were met with in the mouth and throat, one was inclined to think that they must be due to the administration of mercury, and accordingly had recourse to iodide. Then it might be found that iodide preparations were borne equally badly, and finally it was not to be wondered at that sometimes the practitioner threw up the sponge altogether. In reality these were very bad cases of early malignant syphilis, such as he had recently described in the *British Medical Journal*, 1907, ii. p. 952. They were often advantageously treated with sarsaparilla preparations. He trusted that in his reply Dr. Lieven would say more about the serum diagnosis, which was very little known yet in this country.

Major FRENCH desired to speak in favour of the inunction treatment of syphilis, which he had used extensively for in-patients at Woolwich for three years and twelve years previously. He agreed that unless the inunctions were thoroughly done, it was worse than useless. The patient received a course of forty inunctions of a drachm of unguentum hydrargyri and a drachm of unguentum paraffini flavi and had a bath every day, when the old ointment was washed off. That method was in routine use in the French, German, Bavarian, Italian, Hungarian, Swedish and Danish armies, and in many large civil clinics on the Continent, notably Unna's, Pontoppidan's, Wiesbaden, and Aachen, but not in this country, except at a few places. He admitted that in private practice it was not a convenient method. Local lesions about the mouth arose, in his experience, almost solely from smoking, and when the smoking was stopped they commonly disappeared. Early malignant syphilis was occasionally seen in the Army in men who came from foreign service,

as in Burmah. But such cases were now rare, which he thought was due to the more systematic treatment now carried out. He commonly treated his out-patients, as a Service convenience, on the lines mentioned by Colonel Lambkin; he gave them grey oil, one injection per week in defined courses. Relapse was less frequent after inunction as contrasted with injections, and there was a more rapid reduction of lymphatic glands, while the increase in weight was greater in early syphilis. That agreed with the observations of Cabot, who found that the hæmoglobin and the red blood-cells increased for the first three weeks of the mercury administration, and after that time the red cells and the hæmoglobin were less. He frequently temporarily stopped the course at the twenty-fifth inunction because of the patient then beginning to lose weight, but subsequently finished the course. In the treatment of syphilis in the Army, the patients were taken into hospital, where they were generously dieted, and, of course, they had not poor half-starved patients to treat there. He quite agreed with leaving sequestra in the nose until they became loose. Sometimes they came away of themselves, but, if not, when loose they could be removed surgically. He reserved iodide for the later lesions, and gave it between courses of inunction. He believed that the iodide assisted in the elimination of the mercury. The experience in mild cases of treating syphilitic out-patients in the Army by the intramuscular method was commonly good, but he thought inunction should have a fair trial, in view of the conclusive testimony on the Continent in favour of the latter. The convenience of injections did not justify the disuse of other methods. The reduction of syphilis in the Army in India since 1898 was due to the control of diseased men and women and other factors, and not to injections of insoluble salts of mercury, as claimed by Colonel Lambkin, and believed in by other persons in ignorance of the real facts.¹

Mr. T. P. BEDDOES expressed the opinion that for most cases continuous treatment by the mouth was best. If the state of the gums and teeth is attended to before mercurial treatment is commenced the system is soon brought under the influence of the drug without it being necessary to give opium; symptoms rapidly disappear and do not relapse or recur. Though there are severe cases, some due to alcohol, some to pre-existing disease, others where, for no evident reason, the disease assumes a malignant form, in all these no form of mercury by the mouth is absorbed, and ingestion treatment requires supplementing by more intensive methods. These cases come under the notice of specialists, laryngologists, otologists and neurologists, who are apt to over-estimate the proportion of cases where pills are insufficient. In England, with, and it is submitted because of, continuous treatment, ataxy and general paralysis seem less common than where intermittent treatment is more frequent. When cases were found unsuited for ingestion treatment then inunction is of service, combining absorption of mercury and the advantage of massage, which of itself without inunction benefits syphilitic cachexia.

¹ *Journ. Roy. Army Med. Corps*, May, July, 1908.

Inunction and change of scene is of benefit to those who, for domestic or other reasons, are unable to continue their ordinary pursuits, and for whom a break in their ordinary life is desirable. Such cases were benefited by travelling with a suitable attendant, or resorting to Aix to secure the advantages of suitable thermal waters. Sulphur, sarsaparilla, guaiacum, and sweating, though old remedies, are of distinct advantage, both in early and late stages; thus Zittmann's treatment and pilocarpine injections have their advocates both in London and America. When patients do not respond to treatment by the mouth, intramuscular injections should be commenced as soon as possible, for when patients are made to understand that they are the best form of treatment they then submit to them. The soluble mercurial salts act quickest, and of these salicyl-arsenate is best, as it combines the specific action of mercury with the tonic effect of arsenic, but soluble injections have to be made at least twice a week. When it is impossible to give bi-weekly injections, then calomel is the next best form for injections. Many preparations are procurable, but none is less painful or more efficient than a basis of lanolin and olive oil. This can be put up in bulk by any competent chemist; preserving the injections in bulk has the advantage over having each injection separate that the calomel is less liable to deposit. Injections of metallic mercury act more slowly and have to be repeated less often than calomel, but with previously robust patients, as soldiers, it is easily understood that they are sufficiently intense. Carbolized mercurial cream has been extensively used in England since its introduction by Dr. Althaus, though other ingredients have been added to the cream. Those hitherto employed are not only useless, but disadvantageous. The 10 per cent. cream, with the mercury by weight and the other ingredients by measure, has the advantage over those of higher percentage, with all the ingredients by weight, that each injection contains an easily calculated amount of mercury; whereas the 50 per cent. and 40 per cent. injections have a specific gravity of 1.50 and 1.40, so that 2 ℥ of the one contain $1\frac{1}{2}$ gr. of mercury and $2\frac{1}{2}$ ℥ of the other contain $1\frac{1}{2}$ ℥ of mercury; besides, they require special syringes. Though injections are usually made into the buttock, patients taking active exercise often prefer them in the spinal muscles of the loin. It is suggested that the mercurial plaster for chancres should be spread on white leather. The two most important complications of syphilis of the upper respiratory passages are adhesions and laryngitis; œdema of the larynx, which is liable to arise if œdema of the lips and tongue are present. To avoid this opium should not be given and the atmosphere should be kept moist, especially when patients were subjected to the heat of the Zittmann treatment. Adhesions are apt to occur in the pharynx, where they are more common, but are also met with in the external ear; adhesion of the lips is known. The sodium and ammonium salts are less liable than iodide of potash to cause debility, rashes, or iodism.

Mr. HAROLD BARWELL remarked that the first two or three speakers had been treating syphilis under special circumstances. Treatment at Aix and

in the Army was different in point of management from that in private practice. In the latter, inunction was not easy to carry out, and he did not think it had been proved that ingestion treatment was not satisfactory in ordinary cases if properly carried out and in sufficiently large doses. Sir Felix Semon said nearly all the severe tertiary cases seen by him had been treated earlier by the mouth, but inasmuch as nearly every case was so treated in England it was not surprising to find that to be so. But members of that Section were particularly anxious to investigate the rapid treatment of severe lesions of the throat, such as those which threatened perforation of the palate, or where a gumma of the larynx was causing dyspnoea and dysphagia. Little had been said about intramuscular injections of the soluble preparations of mercury; his experience was that they were of great use in getting the patient rapidly under the influence of the drug. Some of these salts were very painful, but he found that the benzoate of mercury caused little discomfort. He used a 1 per cent. solution combined with $2\frac{1}{2}$ per cent. of sodium benzoate, $\frac{1}{2}$ per cent. of sodium chloride in distilled water; $\frac{1}{2}$ gr. to $\frac{1}{2}$ gr. of the mercuric salt was thus given every other day, or every day, and the cases which did not react to this treatment were very few.

Dr. DONELAN was glad to learn from the paper that the Aachen methods were as sound and thorough as they were eighteen years ago, when, as translator, he introduced the writings of the Aachen surgeons to English readers. He would be sorry to appear to detract from the value of those methods either as a means of cure or as an impressive ritual. Unfortunately they were only applicable in the case of the well-to-do and were quite beyond the reach of the mass of those who contracted syphilis. At the same time he was many years ago converted to the view that in the vast majority of cases inunction was the only method of real value in all the earlier stages of syphilis. Both at the Italian Hospital and in private practice he had carried it out for the last eighteen years in a modified way in from thirty to fifty cases yearly. The essential features of the method were the alternation of mercurial inunctions with the ingestion of iodides and the perseverance in this and other suitable treatment for two years. Whilst avoiding absolute routine the method usually followed was the inunction of mercurial ointment, preferably made with lanolin, into the groins and axilla. He found no case in which such athletic efforts as those described in the paper were required to produce physiological effects. On the contrary he had found it a necessary routine to warn out-patients to report themselves to the house surgeon in the event of any signs of salivation occurring. He remembered no case in which physiological effects had not been produced in from ten days to a fortnight. A fortnight of inunctions was followed by a fortnight's treatment with iodides in various forms, and so on for a varying period, generally about six months, every case being, of course, treated on its merits. After this time a mixed ingestion course of mercury and iodides with iron or arsenic was continued for the remainder of the period, and the patients were further advised to submit themselves to a short course each spring for five

or six years whether there were apparent symptoms or not. He was still in touch with a number of these cases, some of whom had begun treatment twenty years ago, and there were no recurrences amongst them. On the other hand he had seen at the hospital a large number of waiters and others who had been treated by injections of soluble salts of mercury in Italy, France, South America and other countries where this method was in favour and in whom the treatment had been continued for from six to twelve months. These people had come to the hospital suffering from recurrence, often of severe character, in a year or two from the cessation of treatment. He thought it was quite practicable with a little forethought and certain obvious precautions to carry out inunction treatment in private practice even amongst persons following their usual business. He was somewhat disappointed that the paper dealt so largely with the general pharmacology of the subject and so little with the matters of special interest to the Section foreshadowed in the title. In this connexion he might mention the great value of sprays of corrosive sublimate in naso-pharyngeal and laryngeal syphilis. In view of what had been said of general paralysis he would like to direct the attention of members who had not seen them to the results of the researches of Ford Robertson as to the cause of this disease. They appeared to show that while syphilis predisposed to general paralysis by lowering the resistance the actual cause was certain microbes infecting the nasal mucous membrane. They were found in nearly all cases and produced symptoms of general paralysis in animals. Some benefit had resulted from treatment by hydroxyl and other oxygen preparations.

Mr. STUART-LOW said that he had recently seen in consultation cases which proved the importance of the variation and alternation of remedies in the treatment of secondary and tertiary syphilis. One case, that of an adult male with advanced secondary ulceration of the fauces and pharynx and numerous patches of a rupial-like nature on the skin, had been treated with injections of calomel and soamin for a month with no improvement, but on changing this for inunctions of mercury the beneficial results were made evident in a week by the clearing up of the throat ulceration. Another case was that of a male patient who had been having mercury internally for a month until his gums were very sore. His throat was very deeply ulcerated and in places quite sloughy; it was exceedingly painful, preventing even fluids being swallowed without much pain and difficulty. Orthoform powder was used locally to the throat with marked relief to the pain and inunctions of mercury employed. In a few days the throat had improved but remained deeply ulcerated and painful. The inunction was then suddenly stopped, and at once a marvellous alteration for the better took place. That the sudden cessation of mercurial inunction had sometimes this effect was a fact not sufficiently recognized and practised. In another case treated in the same way as the last, the improvement on the sudden stoppage of the mercurial inunctions was still much more marked on giving soamin. It seemed that in certain instances arsenical preparations given after mercurial inunctions had

a marked remedial effect. Another instance was that of a female suffering from tertiary trouble in the nose. Iodide of potass had been given for months with only very indifferent action, but on changing the treatment and substituting iodipin locally and generally, rapid improvement ensued. These and many other cases of severe and intractable syphilitic conditions of the nose and throat that had been seen and treated by Mr. Stuart-Low clearly established the great value of varying and alternating the treatment. He wished to emphasize the great utility of Donovan's solution, combining in itself mercury, arsenic and iodine, in the therapeutics of syphilis, and to say how often he had proved the value of local applications in the throat implications and complications of syphilis, especially mercurial fumigation and calomel inflation.

Dr. STCLAIR THOMSON suggested that syphilis would often get well without any treatment. When Dr. Lieven said more about the serum diagnosis in his reply, would he also tell the meeting about the *Spirochæta pallida*, when, where, and how to find it? Mercury preparations through the skin he had used for many years, having, like Sir Felix Semon, seen disastrous results from treatment merely by the mouth. Sir Jonathan Hutchinson, in one of his last writings, in the "Syphilis Number" of the *Practitioner*, 1904, lxxiii, p. 145, said he did not recommend treatment by the mouth because it was the best treatment, but simply because it was the most convenient. It was important to remember that patients objected to exposing that part of their anatomy which it was not correct to show either to friend or foe, and private patients very much objected to being "stabbed." Colonel Lambkin and Major French mentioned the importance of having patients in bed. At the Throat Hospital, Golden Square, they learned a long time ago the importance of putting patients to bed. When rubbed as out-patients they did not seem to get on, but on being put to bed and fed up they improved rapidly. Among the points which Dr. Lieven had not had time to treat of was that of tracheotomy. That operation could be avoided in many cases if the patient were put to bed and rubbed; he had avoided it in that way many times. But sometimes tracheotomy was inevitable. Then if the mercury through the skin were thoroughly kept up the larynx cleared up much more quickly than was the case before the trachea was opened. He also asked Dr. Lieven whether in hereditary syphilis inunctions were as satisfactory as in the acquired disease. He asked because in this country, unfortunately, it was the habit to treat those children only with hydrargyri cum creta by the mouth. That morning he saw a child with extensive keratitis and threatened total deafness, yet she had never had anything but a little hydrargyri cum creta, though she was now 10 years old.

Dr. SCANES SPICER said the Section was much indebted to Dr. Lieven and Colonel Lambkin for having brought before it the papers they had presented. Just now there was considerable stir in medical circles in connexion with the treatment of syphilis, especially as to the injection of the new salts, atoxyl,

soamin, arsacetin, &c. Practitioners naturally felt interested in the question whether they must abandon the classical routine of oral administration of mercurial preparations in favour of these newer injection methods or of the more rigorous inunction system of Aix. He did not think, at present, the British public would stand, as routine measures, either systematic gluteal injections or the systematic inunctions and restraints of Aix; there was a popular idea that in this affection medicines should be given by the mouth, and it was supported by the bulk of professional opinion in this country, whether rightly or wrongly. Was it now necessary for the profession to get converted? He was not referring to exceptionally severe cases or exceptional circumstances, but to the ordinary average case. As compared with twenty-five years ago the results of the treatment of syphilis in England had been highly satisfactory, for it was comparatively seldom now the bad rupial cases and necrotic and phagedænic developments were met with, and the symptoms generally did not develop to such a severe degree. He felt confident that this improvement had been due to the methods customary in England of mercurial administration by the mouth. A very valuable means of applying mercury to the specific ulcerative lesions of the nose and throat, if used systematically, was the calomel fumigator, which now in view of the spirochæta becomes a rational instead of an empirical measure. It was important to see that the upper respiratory passages were clear when specific lesions of the mouth and throat were present. It did not do to ignore nasal obstruction because the patient had syphilis. He had seen with Mr. Hastings Stewart a remarkable case illustrating this. The patient had a severe deep naso-pharyngeal ulcer on the posterior wall extending down to the vertebral column, and it would not heal in spite of the most approved systemic treatment by several of the best experts; it had lasted about three years and caused the patient much distress. The patient had a high degree of nasal obstruction from septal deformity, was an aggravated and life-long mouth breather, and had great thickening and irritation of lips, tongue and pharynx. Operative clearance of the nasal obstruction made all the difference to that patient, for the improved ventilation and drainage of the post-nasal space allowed him forthwith to get well (as far as the naso-pharynx was concerned) under the very same systemic treatment which was previously unsuccessful; and Dr. Hastings Stewart had given him an opportunity of conforming the permanence of the result two years afterwards in that patient. Hence, although the specific affection may be the main matter in any case, local new specific conditions should not be ignored, and may even make all the difference in the result of our other measures.

Dr. DAN MCKENZIE drew attention to the difficulty of coming to a decision upon the question of the efficacy of any particular method of treating syphilis. Syphilis was a disease the course of which was spun out over long years, and the medical man who treated the "tertiaries" was but seldom the same individual as he who had treated the "secondaries." Thus the patient's word

as to what treatment has been adopted, how long it had been continued, and what the immediate result had been, constituted in many cases the sole guide to the nature and results of that treatment. And our condemnation of the older methods rested too frequently upon that slender evidence. It was, in his opinion, questionable whether any method of treating syphilis could be confidently relied upon to prevent severe tertiary symptoms in every case. For this reason, he held that judgment on the comparative merits of the old and new therapeutics should be suspended until it was possible, from the continued observation of a large series of experiments, to determine with some degree of precision the best remedial agent and the best remedial method. They should avoid being swayed to either side by impressions and opinions. Turning to syphilis as it affects the upper air-passages, he, like previous speakers, had frequently obtained rapid benefit from calomel injections in cases in which mercury by the mouth had proved unavailing. He had employed calomel suspended in parolein without causing pain and without producing abscess. In those obstinate secondary pharyngeal ulcers which approximated to the tertiary in character, the mouth and teeth should be attended to in order to exclude sepsis from the ulcer. He condemned the routine administration of mercury in pill form, for it was impossible to make sure that pills were always digested and absorbed. In leukoplakia of the tongue and cheeks he had obtained a surprising amount of benefit, in some cases, from hydrogen peroxide used as a mouth-wash.

Dr. LIEVEN, in reply, said that a sero-diagnosis of syphilis is possible, in that the blood of a syphilitic contains bodies which are capable of combining with a complement in the presence of an extract from the liver of a syphilitic fœtus. With a positive reaction this mixture (extract + complement + the decomplemented serum to be tested) is not capable of producing hæmolysis in a so-called hæmolytic system (red corpuscles of a sheep + decomplemented serum of a rabbit immunized against sheep blood) because of the absence of complement which has been used up. In the absence of antibodies the complement is not used up, so that the corpuscles of the sheep are promptly decomposed by the same and the immunized rabbit serum. In Bauer's test the red corpuscles are directly added to extract + complement + the decomplemented serum. Bauer thus does away with the sheep-immunized serum, because according to his experience the human blood possesses enough hæmolytic power in regard to the sheep's red corpuscles to decompose them. Bauer's test has proved reliable, and has the advantage of not requiring the prepared serum of the rabbit. Dr. Lieven referred to an article of Bering's, published in December last, which gave a full account of the practical importance of sero-diagnosis in syphilis. Bering's own experiences are founded on nearly 900 examinations of blood. He also, like other German investigators, came to the conclusion that a positive reaction is only present in cases where there has been an infection with syphilis at one time during the patient's life. The test is absolutely reliable. It is therefore of the

greatest importance in the diagnosis of cases of repeated miscarriage, doubtful growths and ulcerations, and also in diseases of the eye when there is no history of infection. A very important fact has been demonstrated by Bering—namely, that a positive reaction is much less frequent in such patients who have been undergoing an energetic, chronic, intermittent treatment, whilst, on the other hand, in patients who have been sufficiently treated the positive reaction is often more absent. Dr. Lieven believed that the question as to whether internal treatment had the good therapeutic effects which were claimed for it by Dr. Beddoes was likely to be settled before long by the serum test. As to the method employed by Colonel Lambkin, Dr. Lieven was of opinion that the introduction of hypodermic injections in England meant a blessing to the thousands suffering from syphilis. He quite agreed with Colonel Lambkin that the inunction treatment could only be carried out if the patient could be trusted to do it properly. Dr. Lieven said that those long needles which he had shown were absolutely necessary for women on account of the thick layer of fat on their buttocks. He maintained that it was better to inject deep into the muscle. Hypodermic injections were more painful and apt to leave long persisting nodosities. He admitted that embolism was practically impossible with the thick cream used by Colonel Lambkin and the 40 per cent. grey oil and calomel mixture as now used on the Continent, but careful attention had to be paid to all preparations made with a more liquid medium. He paid special attention to suspension of salicylate in paraffin or even in vasenol. Dr. Lieven thought that the good effects of arsenates were to a great extent due to their tonic qualities and to their particular effect on the skin. He quite agreed with the suggestion that smoking had a great deal to do with the frequent relapses of mucous patches in the mouth. He had always found that his German, English, and French lady patients comparatively rarely showed secondary symptoms in the mouth because they did not smoke, whilst Russian ladies, who smoke a great quantity of cigarettes, suffer in the same manner as men. As regards the elimination of mercury by the use of iodide, he stated that there existed no definite proof that this was really the case. The fact that this statement had passed from one text-book to another did not make it more true. Then Dr. Lieven said that he had not only expressed his own opinion about the inefficiency of the internal treatment, but the general opinion of Continental physicians. He also stated that, as far as he knew, no reliable statistics existed on the frequency of locomotor-ataxy in Germany, but he admitted that in France there seemed to be more ataxy than anywhere else. He could only state that locomotor-ataxy is mostly found in cases which in the beginnings are very slight, and therefore insufficiently treated. As to Dr. StClair Thomson's question about the practical use of examination for the spirochæte, Dr. Lieven said that in general practice only the examination of primary sores gave a quick and reliable result. The method consisted in squeezing the sore in order to obtain some serum. This is then stained with a solution of 10 drops of Giemsa solution in 10 c.cm. of water for thirty to

forty minutes. As to the other question asked by Dr. StClair Thomson—namely, what experience Dr. Lieven had had as regards tracheotomy, he said there were two indications, the "*indicatio vitalis*," which, for example, might occur in a case of acute oedema following perichondritis, and the necessity of performing dilating operations in a very narrow larynx, which may be followed by an intense reaction. Dr. Lieven had never found any necessity for performing tracheotomy to give a rest to the larynx, as is done in tuberculosis, but he admitted that there might be cases in which this proceeding would prove beneficial.

Laryngological Section.

February 5, 1909.

Dr. DUNDAS GRANT, President of the Section, in the Chair.

Case of Tertiary Specific Atresia of the Posterior Nares in a middle-aged Woman.

By DUNDAS GRANT, M.D.

OPERATION was performed on December 18, 1908, for detachment of the soft palate from the posterior wall of the pharynx, including removal of a portion of the hard palate at its junction with the vomer, so as to increase the distance between the soft palate and the posterior wall, and to diminish thereby the risks of reclosure. Mooring of soft palate to the front teeth by means of suture for two days. Patient can now blow the nose, and drink; articulation fairly good.

Case of Malignant Specific Ulceration in a Young Male Patient.¹

By DUNDAS GRANT, M.D.

ULCERATION of the fauces and pharynx with great pain had occurred within a year after the primary infection, and did not yield to iodide of potassium and mercury. At Dr. Lieven's suggestion intramuscular injections of calomel were administered, and the patient was shown again on February 2, 1906, with complete subsidence of the ulcerative process.

DISCUSSION.

Dr. McBRIDE asked in what form the mercury was applied before the injections, and if it was inunction, was it thoroughly carried out?

¹ Previously shown January 12, 1906, *Proc. Laryng. Soc. Lond.*, 1905-6, xiii, pp. 33, 44.

Dr. J. B. BALL asked how many injections of mercury had been given, and whether any other form of treatment was given besides the intramuscular injections of calomel.

Dr. DONELAN asked whether it was proposed that the patient should have further treatment, or did the injections he had had end the course without fear of recurrence?

Sir FELIX SEMON said the case emphasized what he had stated two years ago—viz., that while the routine treatment of syphilis affecting the upper air-passages served well enough in ordinary cases, one must not be bound too rigidly by preconceived notions as to how cases of the kind should be treated. Particularly in the malignant cases, it might be necessary to try various treatments before hitting upon the right one. At the recent discussion at the Section he had again expressed himself as an adherent of the inunction method, but he wished to repeat that even that was not always successful, even as carried out at Aix-la-Chapelle. The sooner these facts were recognized generally, the better it would be for the profession and for the public.

Dr. GRANT, in reply, said the patient had had inunctions of mercury under a careful medical man, though possibly not quite as efficiently as at Aix. There had been 11 injections of calomel, first at weekly intervals, then every three days, and when the 11 had been given the fauces were healthy. He had had oral treatment by various remedies until January, 1907, but in spite of them he was getting worse both locally and generally. When he first brought the case forward the patient had dullness at the right apex, which caused anxiety lest it might be a mixed case of syphilis and tubercle. The opsonic index was normal, there were no tubercle bacilli, and the man was sent to the seaside, but it was only when Dr. Lieven advised calomel injections that there took place the extraordinarily good improvement a month later which the members saw. The patient has since remained well. The case might truly be called one of "syphilis maligna præcox."

Case of Thyrotomy with Restoration of Exceptionally Good Voice.

By Sir FELIX SEMON, K.C.V.O., M.D.

THE patient, a gentleman aged 45, was sent to me on March 3, 1908, by Dr. Kochmann, on account of hoarseness. Nothing but congestion of the vocal cords was visible at that time in the larynx. When seen in June, 1908, by Mr. Tilley, a small tumefaction of granular appearance had developed on the front part of the right vocal cord, the movements of which were still perfect. The little tumour slowly increased in size, but as it was extending towards the anterior commissure it was decided,

after consultation with Mr. Tilley and after Mr. Butlin's independent opinion had been obtained, also to the effect that an exploratory thyrotomy was indicated, to perform this operation, which accordingly was done on July 28, 1908. The entire right vocal cord was removed with the growth on it. Dr. Kochmann and Mr. Stabb kindly assisted me, and Mr. Shattock was present and examined the small growth immediately after removal, when it was found to be a typical squamous-celled carcinoma. The patient very quickly recovered from the operation, and has regained so surprisingly good a voice that this is the cause of his being shown to the Section. It is indeed hard to believe that the right vocal cord has been removed *in toto*. Its place is taken by a cicatricial ridge, which in this instance does even better service than is usual in such cases.

DISCUSSION.

DR. STCLAIR THOMSON asked whether Sir Felix could tell members how to do it. Did he coapt the thyroid *alæ*, or were they left to settle into their own position? He himself had shown a case in which, though the voice subsequently was strong, it was not musical, because the new cord was not exactly on the same plane as the old one.

MR. HERBERT TILLEY said the case illustrated a point which had been insisted on by Sir Felix Semon, but which had too frequently been forgotten—viz., that fixation of the cord was not a *sine qua non* of early malignancy. This patient had a small tumescence or thickening of the right cord, so small that there seemed to be little difference between the two sides. The cord was so freely movable that infiltration as by an epithelioma seemed out of the question. Large doses of iodide of potassium were given for some time during the few months while he was under his (Mr. Tilley's) care, and by Sir Felix himself, but there was no improvement. Even when the larynx was opened and the growth removed it seemed quite unlike epithelioma. Still, the microscope revealed the true malignant nature of the growth.

SIR FELIX SEMON replied that he laid such stress on the accurate adaptation of the two halves that he held them himself in position while an assistant did the stitching through the perichondrium or through the cartilage itself, taking care that the stitches should not penetrate into the interior of the larynx, otherwise there was apt to be formation of a granuloma in the anterior commissure. He had known, in a case of his own during his absence, a second thyrotomy performed because such a granuloma was regarded as a recurrence. If the two halves were not accurately coapted, a good voice could not result. In the present case, however, the voice certainly was exceptionally good. Theoretically it seemed almost impossible that a man from whom an entire vocal cord had been removed should be able to *sing* with a clear voice, as the patient had just done before the Section.

Hæmatoma of the Right Vocal Cord closely simulating a Fibroma.

By Sir FELIX SEMON, K.C.V.O., M.D.

THE patient is a gentleman, aged 45, who was sent to me by Mr. Whitehead and Dr. Hosford, of Leeds, on November 6, 1908. He had been hoarse for about six months. On examination a semiglobular smooth red tumour was discovered, rising with a broad surface from the much congested right vocal cord, the movements of which were quite free. The size of the growth was about that of half a split-pea. Two months later it had not altered, and the diagnosis of a fibroid growth seemed from the naked-eye appearances to be thoroughly confirmed. The tumour was removed *in toto* on the first attempt by Mackenzie's forceps, and after removal not only looked again to the naked eye like an ordinary fibroma, but was also hard and firm to the touch. It was therefore very surprising when the following report was received from Mr. Shattock: "The lesion is simply a blood-clot—a spheroidal clot, which lies in the connective tissue, a narrow zone of which separates it from the investing stratified squamous epithelium. No muscular tissue has been removed with it. The clot is everywhere immediately bounded by a zone of connective tissue furnished with flat cells loaded with blood-pigment. There is no trace of any new growth."

The specimen is shown under the microscope. The case is put on record on account of the supposed rarity of blood-clots in this situation. I have already put three other cases of blood-clots simulating benign and malignant new growths in the larynx on record¹, and I am inclined to believe that if every tumour intralaryngeally removed were microscopically examined, it would be found that this rarity is more apparent than real. The patient, immediately after removal of the growth, regained his normal voice.

DISCUSSION.

Dr. WYATT WINGRAVE emphasized Sir Felix's remarks about the frequency of blood-clot in connexion with innocent tumours of the larynx. Blood-clot was found in two forms: endovascular and perivascular, the former being generally a white fibrinous clot, whereas the others seemed to be extravasations. They were not very uncommon, as shown by the fact that many

¹ *Ann. des Mal. de l'oreille du Larynx* [etc.], Par., 1899, xxv, p. 241.

innocent growths of the larynx contained indications of old pigmentation, not of carbon, but of hæmoglobin derivation.¹

Dr. JOBSON HORNE considered that tumours, hæmorrhagic in origin, as well as submucous hæmorrhages of the vocal cords, were by no means common. At the same time he was of the opinion that they were of more frequent occurrence than was generally known. Those cases that had come under his own notice were mainly due to trauma or influenza. Some fifteen or more years ago he saw several cases in this country as well as in Continental clinics due to the latter cause, not only of submucous hæmorrhage of the vocal cords, but also of the trachea, and the latter cases naturally gave rise to a consideration of the question of hæmoptysis and of tuberculosis as the underlying factor. In similar cases he had also seen hæmorrhages into the tympanic membrane. As regards trauma as a cause, he had shown how the vessels coursing over the vocal cords at times assumed under a strained production of notes a condition which amounted to varicosity, and which might result at any moment in a submucous hæmorrhage. Dr. A. J. Brady, of Sydney, New South Wales, had described such a case occurring in a singer whilst singing the part of Mabel in the "Pirates of Penzance."² The speaker had had a similar case under his notice. He could therefore readily accept hæmatoma as a definite form of neoplasm occurring on the surface of a vocal cord, and under the microscope he had observed a comparable condition occurring within the substance of the cord.

Mr. CYRIL HORSFORD called the attention of members to an article which he published last July relating two cases which bore on the present discussion. Case 3 in his paper was one spoken of by Dr. Wingrave as endovascular clot. In Case 4 the so-called fibroma of the vocal cord had arisen as a result of submucous hæmorrhage during singing. The patient gave a distinct history of having ruptured a blood-vessel, and that probably took place before an attack of laryngitis had subsided—*i.e.*, when the blood-vessels were congested. That clot was probably the starting-point of the irritation which eventually resulted in the fibroma.

The PRESIDENT asked whether it was a case of the development of a hæmorrhage into a fibroma. There seemed to be a capsule of some kind into which there was hæmorrhage.

Sir FELIX SEMON, in reply, said that up till a short time back no textbook, so far as he knew, had mentioned those blood-tumours. In one of the cases which he described many years ago Mr. Butlin opened the larynx because the growth was supposed to be malignant, an opinion in which he (Sir Felix) concurred. When the larynx was opened the apparent tumour was found to be simply an extravasation into the tissue of the vocal cord. The second case he had reported had been sent to him from Naples as angioma,

¹ "Pathology of Fifty Innocent Laryngeal Growths," *Journ. of Laryng., Rhin. and Otol.*, Lond., 1906, xxi, p. 215.

² *Journ. Laryng., Rhin. and Otol.*, 1905, xx, p. 566.

and it certainly looked like one when removed by intralaryngeal operation. In reality, however, there was a blood-clot enveloping what looked like an unusual form of papilloma. Recurrence in precisely similar form took place, and the growth was found to be malignant. The third case was like the one shown to-day; it was situated in the anterior commissure, and looked like a fibroma, but turned out to be a simple blood-clot. The development in two of the cases was gradual, just as would be expected in an ordinary fibroma.

The Further History of "A Case for Diagnosis. ? Continuous Fibroma of Neck and Larynx, or Malignant Disease of the Larynx, with Enlargements of Glands in the Neck." ¹

By Sir FELIX SEMON, K.C.V.O., M.D., and
WILFRED TROTTER, M.S.

It will be remembered that in this case a tumour had been removed by Sir William Watson Cheyne from the floor of the mouth on the left side, and several enlarged cervical glands on the same side of the neck, in 1900, and that microscopic examination had proved the disease to be epitheliomatous. The patient remained free for nearly eight years. In October, 1908, however, huskiness and discomfort in the throat occurred, and the patient was brought to me by Dr. Daniel, on October 19, for consultation. A big, semi-transparent, rounded swelling was found occupying the region of the left arytaenoid cartilage and left ary-epiglottic fold, whilst externally a swelling of about the size of a tangerine orange was left behind the left sternomastoid.

On the day when the patient was brought before the Section, November 6, 1908, a spot of ulceration had made its appearance on the œsophageal aspect of the growth. In the discussion a very doubtful prognosis was given by various speakers as to the chances of a radical operation, an opinion in which Sir William Watson Cheyne had already previously concurred. Nevertheless, the patient decided upon having an exploratory thyrotomy performed, the result of which was to decide whether radical operation was to be joined to the thyrotomy, or whether the attempt was to be given up. This operation was performed by Mr. Trotter, in the presence of Sir Felix Semon, on November 23,

¹ Shown at the meeting of the Section on November 6, 1908, and described in the *Proceedings (Laryng. Sect.)*, ii, No. 2, p. 8.

when it was found that the larynx itself was quite free, and that what had appeared to be an intralaryngeal tumour was in reality a growth springing from the pyriform sinus on the left side, and hanging, with some globular projections, partly into the larynx, partly into the œsophagus. The growth was removed *in toto*, with a zone of healthy tissue around it, and the resulting defect in the anterior lateral and pharyngeal wall was immediately stitched up. The patient made an uninterrupted recovery, but the left vocal cord is now seen to have become fixed in the cadaveric position, either owing to cicatrization near the crico-arytænoid joint, or owing to implication of the left recurrent in scar tissue. Nevertheless, he has already regained a surprisingly good voice.

On December 22—*i.e.*, a month after the first operation—the glands on both sides were removed by Mr. Trotter, and were found to be less diseased than had been feared. The patient again made a good recovery, and is now recuperating in Switzerland.

The case teaches the important lesson that when there is any possible doubt in such cases an exploratory operation ought certainly to be undertaken. In this case it would have been possible to avoid thyrotomy altogether and to remove the growth by pharyngotomy. Of the probable chain of events causing the belated recurrence in a very unusual situation Mr. Shattock will give an explanation in the discussion.

The laryngoscopic appearance and the actual condition were demonstrated on the epidiascope, Mr. Trotter having reconstructed the precise situation of the tumour in a dead larynx.

DISCUSSION.

Dr. PERMEWAN asked what were the exact steps in the operation. He thought probably that the preliminary thyrotomy was an advantage. He had shown, years ago, a growth removed from the same region by subhyoid pharyngotomy, but though the growth was easily removed the patient died. He thought the proper course had certainly been adopted in this case.

Mr. TROTTER replied that the operation was begun under the idea that it would be a total laryngectomy; therefore Gluck's incision was used and a flap reflected towards the right side of the neck. An exploratory thyrotomy showed the larynx to be normal, except for œdematous mucous membrane on the left side. To the left of the upper opening of the larynx he found a large hard mass. He then made a transverse incision through the thyrohyoid membrane on the left side and found, occupying the left sinus pyriformis, a tumour the size of an egg. Most of the tumour was submucous, but it involved the mucous membrane and projected through it. The tumour shelled out readily

from the surrounding loose fatty tissue, and the overlying mucous membrane was excised. It was very easy to stitch up the opening in the pharynx, and the slackness of the mucous membrane obviated any difficulty in swallowing afterwards. But when one removed tumours of any size from the oral part of the pharynx, if large portions of mucous membrane were taken away the patient often had difficulty in swallowing for a long time afterwards. With regard to tumours occupying the sinus pyriformis, it was rare to see primary growths there while they were operable. He thought the case was one of late recurrence in an aberrant lymphoid nodule, and that that was explicable because the sinus pyriformis was the highway for the lymphatics from the back of the tongue to the cervical glands. He believed these lymphatics were infected at the time of the original growth eight years earlier.

Mr. S. G. SHATTOCK said: The two matters of pathological interest in this case resolve themselves into (a) the occurrence of a metastasis in the lymphatic tissue about the larynx, following upon a carcinoma of the floor of the mouth; and (b) the delayed appearance of the metastatic growths in connexion with the larynx and in the cervical gland. To take the "laryngeal" tumour first: From the density and general diffusion of the lymphocytic collection through the growth, and its circumscription, it must be inferred that the carcinomatous formation is a metastasis occurring in a focus of lymphatic tissue. This conclusion I arrived at from the study of sections of a small piece of the periphery of the growth covered with the mucous membrane, prepared by Mr. Trotter, whose further sections, as he has described, showed in addition the presence of lymphoid nodules embedded in the neoplasm. As to the anatomical location of lymphatic tissue in connexion with the larynx: The ventricle of Morgagni is provided with a layer of such tissue, the highest portion of which lies in the ventricular band. The disposition of this is figured from a coronal section of the parts, in a child, by Klein.¹ A study of the adult larynx by means of similar coronal sections carried through the middle of the ventricle shows a lymphocytic zone beneath the epithelium of the ventricle, but in relatively reduced amount, and in coronal sections made more anteriorly, through the sacculus, the latter, I find, is likewise invested with an extension of this tissue, which intervenes between the columnar ciliated epithelium and the layer of glands surrounding the recess. The actual position of the laryngeal tumour is not in accord with its origin in the lymphatic tissue of the sacculus. The sacculus lies too far forwards towards the epiglottis to come into relation with the sinus named, and is, moreover, in such close apposition with the inner surface of the ala of the thyroid cartilage that any growth arising from it, whether from its outer or its inner wall, could not but displace the soft parts inwards towards the laryngeal cavity in a way which did not clinically obtain. With the most posterior part of the ventricle it is somewhat different; a metastatic growth arising in the lymphatic tissue at this site might eventually

¹ "Atlas of Histology," 1880, fig. 5, plate 36, whose figure is reproduced in Quain's "Anatomy," iii, pt. iv, "Splanchnology," 1896, p. 157, 10th ed.

produce a swelling in the floor of the pyriform sinus. On pushing a probe through the most posterior part of the ventricle in a somewhat upward and backward direction, its point can be made to appear beneath the mucous membrane of the floor of the sinus in question, and on cutting deeply into the lax tissue of the aryepiglottic fold (in its long axis) the probe is exposed at the bottom of the incision in such a position as to show that a growth arising in its neighbourhood might fill the fold and encroach on the sinus, as was actually the case. The relation of the parts will be best appreciated by a coronal section. If the coronal section is carried through the most posterior part of the ventricle of Morgagni, not in a perfectly vertical transverse plane, but along the track of a probe pushed from the hinder part of the ventricle upwards,



A coronal section of the right half of an adult male larynx, made, after hardening, as described in the text, the posterior surface of the anterior portion of the section being that viewed. A dotted line is drawn between the ventricular band and the floor of the sinus pyriformis, through the intervening connective tissue. The muscular mass of the thyro-arytenoid lies below the level of the sinus in the section.

outwards, and slightly backwards, it will pass through the front of the sinus pyriformis. An inspection of the divided surface will show that nothing intervenes between the mucosa of the sinus and that of the ventricle except fat and connective tissue. The distance from the ventricular band to the floor of the sinus in the adult male examined was 9 mm.; the distance from the bottom of the ventricle to the same spot would, of course, be somewhat less. The muscular tissue of the thyro-arytenoid lies below the level of the floor of the sinus in such a section. It is therefore quite conceivable that the growth

which projected from the sinus arose as a metastasis in the lymphatic tissue which normally lies beneath the mucosa of the ventricle of Morgagni and which may have been, in this case, in unusual amount and have strayed beyond its usual position. The lymphatic tissue of the ventricle takes either a diffuse form or that of definite lymph-follicles.¹ As an alternative to this view we must believe that the metastasis arose in a lymphatic gland or nodule of an aberrant kind beneath the mucosa of the pyriform sinus. No such gland, at least, has been described by anatomists. That the location of the lymphatic tissue was exceptional appears from the rarity of such a metastasis in connexion with carcinoma of the floor of the mouth. The transference of cells from the growth in the floor of the mouth must be taken as having occurred about the same time in the lymphatic tissue of the larynx and in the deep cervical glands. At the first operation, when the growth was excised from the floor of the mouth, lymphatic glands were also removed from the neck. One infected gland, at least, escaped removal, and in this, metastasis subsequently declared itself. That the laryngeal growth is not of independent origin appears from the fact that the investing epithelium is intact; the growth is subepithelial. Clinically, moreover, the absence of any proper ulceration or alteration in the character of the surface of the swelling points to the same conclusion. The further conceivable possibility of the laryngeal tumour having arisen by the direct inoculation of the mucosa from debris transferred from the lesion in the mouth is also negated by the circumstance that the tumour has not originated at the laryngeal surface, but in the deeper part of the mucosa or the submucous tissue.

The second point of interest is the delay in the appearance of the metastatic growths in connexion with the larynx and in the cervical gland. Instances of secondary growth as long, and even longer, delayed, however, have been before observed in other situations. After excision of the breast for carcinoma the metastasis in the lymphatic glands may not declare itself for six years or more, and this without any recurrence having taken place at the site of operation. One such which was observed by Mr. Bowlby is peculiarly satisfactory, in that the microscopic examinations of the primary tumour of the breast and of the diseased lymphatic glands subsequently excised were both made by himself; there was no local recurrence, and the enlargement of the axillary glands occurred seven years after the primary operation. And in a case observed a few years ago at St. Thomas's Hospital an autopsy was made upon a man who died with melanotic tumours of the liver, and in whom no primary disease was discoverable. On searching the hospital records it was found that the patient had had one eye removed by Mr. Nettleship for a melanotic growth no less than fourteen years previously. The case is recorded by Mr. J. H. Fisher and Dr. C. R. Box.² Upon the precise classification of the melanotic growths differences of opinion were expressed. In the discussion which followed, Mr. C. D. Marshall stated that in one case in which the eye

¹ Luschka, Verson, Boldyrew, Coyne; cited by Klein, loc. cit., p. 238.

² *Trans. Ophthalm. Soc. U.K.*, Lond., 1900, xx, p. 140.

had been removed for sarcomatous disease he had found that the patient died with secondary growths in the liver eleven years and five months afterwards. No explanation of this inhibition of the growth of the cells transferred to the lymphatic or other structures can at present be given. Yet it would seem as if the organism had acquired, by the growth of the parent tumour, a certain degree of immunity which, although insufficient to put a stop to the main growth, sufficed, after removal of the latter, to hinder the growth of the few cells transferred to the more distant parts; but that this, like other forms of acquired immunity, was not permanent, and on its passing off, the dormant cells started into activity.

Macroscopic Specimen, Microscopic Section and Microphotograph of Epithelioma of the Uvula.

By W. MILLIGAN, M.D.

THE patient, a male aged 62, had suffered from a "gouty" throat for two or three years. There was a doubtful history of syphilis forty-five years previously. On examination the uvula was found to be slightly enlarged and indurated. The posterior surface was ulcerated and bled comparatively readily upon being touched with a probe. Ulceration extended for a short distance along the posterior surface of the free margin of the soft palate upon the right side. No enlarged glands could be detected. A small portion was removed for microscopic examination and pronounced epitheliomatous. Under chloroform a wedge-shaped piece of soft palate with uvula attached was removed with a galvanocautery knife. Uninterrupted recovery took place, with comparatively little effect upon the voice.

New Growth of the Epiglottis for Diagnosis.

By KNOWLES RENSHAW, M.D.

THE patient was a married woman aged 60. History: Frequent hæmoptysis for several months. Each hæmoptysis was of small quantity and commenced suddenly without apparent reason and terminated as suddenly.

Examination: On the posterior surface of epiglottis, about $\frac{1}{4}$ in. from the right upper angle, was a small projection, soft to the touch, the size of a split pea, having the appearance of a papilloma. Cocaine was applied, and the small growth was removed with a pair of StClair Thomson's postnasal forceps. The growth came away flush with the

surface, and there was only slight hæmorrhage for a minute or two. One week later the surface had entirely healed. Three weeks after the time of the removal a small projection was seen in the same position; this was touched on two occasions with chromic acid and destroyed. After another month a small growth similar to the original one, but considerably smaller, made its appearance. There had been no further hæmorrhage, but having in view the possible malignancy of the growth it was decided to remove the affected part of the epiglottis. Under cocaine, with cutting forceps, about a quarter of the epiglottis was removed, including the growth and a surrounding band of healthy tissue. There was only slight hæmorrhage, and the wound healed rapidly with little discomfort to the patient. The growth was examined by Dr. Arthur Sellers, at the Manchester Public Health Laboratory, who prepared a section and made the following report: "In microscopical examination the epithelium was found to be raised by a small collection of spindle-cells lying between it and the cartilage, thus forming a small rounded tumour-like projection. This suggests the possibility of a very early sarcoma, but more probably the condition is due to a chronic inflammation arising in connexion with a small varix. There is no evidence of tuberculosis." I may add that to the naked eye, at all events, there was no appearance of varix, though the history of the patient rather bears out this view. The age of the patient by no means excludes the possibility of sarcoma, and the rapid dilatation of the blood-vessels, and proneness to hæmorrhage which often is characteristic of the growths, would account for the symptoms observed.

**Case of Chronic Suppuration of the Frontal Sinus and
Ethmoidal Cells; Radical Operation by the Author's
Osteoplastic Operation, with no Facial Deformity.
(Patient shown.)**

By P. WATSON WILLIAMS, M.D.

MISS C., aged 33, suffered from purulent discharge from the left nasal passage, severe headaches, general adynamic condition, associated with suppuration in the nasal accessory sinuses.

The severe pains in the head dated from an attack of influenza six years ago, but the purulent nasal discharge had only been noticed for the last two years, during which time the headaches had become

increasingly severe and the general health so greatly impaired that she was unable to continue her work. The patient was found to be suffering from left frontal, ethmoidal and maxillary sinus suppuration.

Operation was performed under general anæsthesia on November 18, the incision extending along the margin of the left eyebrow. The skin tissue and periosteum having been raised, the frontal sinus was trephined and found full of pus. A longitudinal incision was made, extending from the median extremity of the first horizontal incision down to the middle line of the nose, as far as the tip of the nasal bone. The anterior wall of the frontal sinus having been removed above the initial incision, a second incision was made along the lower and inner margin of the left



FIG. 1.

Diagram to illustrate the method of forming the osteoplastic flap after the frontal sinus had been exposed. 1, The nasal process of superior maxillary bone being divided subcutaneously by the saw passed in through the nose so as to emerge at the opening in the lachrymal groove; 2 2, a Gigli saw, passed from above down the enlarged frontonasal duct so as to emerge at the lachrymal groove opening to divide the bone subcutaneously; 3, the osteoplastic flap which then resulted on the vertical division, by knife and saw, of the skin and bone near the mid-line, and which was then turned outwards like a doorway on a hinge; 4, the facial artery running up to 3, where it becomes the angular artery. As it courses inwards below the bony margin where the saw divides the nasal process the artery is not injured, and the preserved vascular supply of the osteoplastic flap ensures its vitality and the rapid reunion of its cut edges.

orbit exposing the lachrymal groove; then, the lachrymal duct having been turned out, entrance was made through the base of the lachrymal groove into the left nasal passage. The nasal process of the superior maxillary bone was divided subcutaneously by a saw passed through the left nasal orifice, and coming out at the opening into the lachrymal groove. A second saw-cut divided the bone from this point upwards subcutaneously. The left nasal bone was then divided by longitudinal saw-cut close to the septum, the osteoplastic flap thus formed turned outwards, the whole fronto-ethmoidal region being then exposed to view.

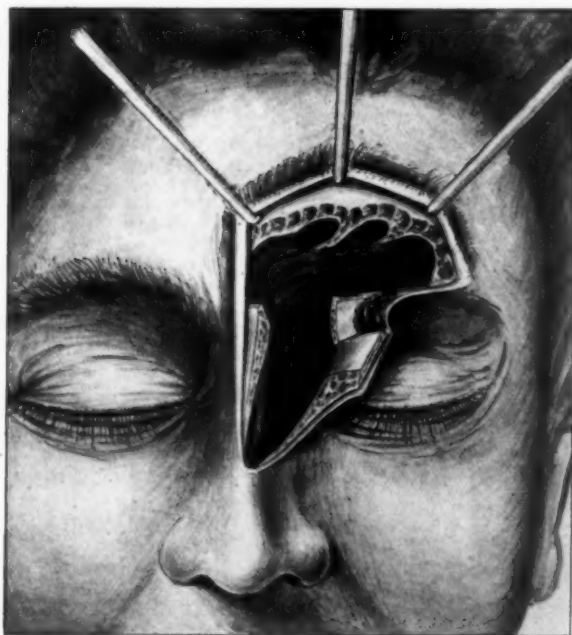


FIG. 2.

The osteoplastic flap operation on the patient's left frontal sinus. The frontal sinus cleared of mucous membrane, bony septa and the whole of the ethmoidal cells, as well as the orbital roof corresponding to the frontal sinus floor and the inner wall of the orbit in its upper half removed, exposing freely the sphenoidal sinus wall. Note on the nasal side of the wound the remains of the vertical plate of the middle turbinated body; on the inner side of this plate lies the narrow roof of the olfactory fissure—i.e., the cribriform plate. The vertical plate of the middle turbinal is preserved and used (a) as a guide in avoiding the danger area, and (b) to guard the cribriform plate from the purulent secretions, &c., from the operated area above during process of healing.

The floor of the left frontal sinus corresponding to the orbital roof was removed, as well as the corresponding portion of the inner orbital wall, the whole of the pyogenic thickened mucosa of the frontal sinus carefully curetted away, and the fronto-ethmoidal cells, together with the posterior ethmoidal cells, cleared away right back to the sphenoidal sinus. Radical antral operation was also performed on this side, but without curetting the mucous membrane, the antrum, like the frontal sinus, being full of thick pus. The patient was in a very adynamic condition, and it appeared inadvisable to prolong the operation. The whole of the fronto-ethmoidal region being rendered aseptic, a rubber tube was carried through the frontal sinus down to the external nasal orifice, and the osteoplastic flap having been replaced, complete closure of the incisions was made.



FIG. 3.

The patient at date of demonstration before the Section after operation (from an untouched negative), showing the absence of any facial defect and complete absence of any perceptible scar on the nose.

The following day, November 19, there was considerable swelling of the left cheek, and an erysipelatous flush beneath the chin, though the frontal sinus incision had practically united. The third day after the operation the flush was replaced by diffused cellulitis, *angina Ludovici*, beneath the chin, so 20 c.c. of antistreptococcic serum was injected, and the stitches were removed from the incisions. The discharge from the antrum was very profuse and foul, and consequently it was imperative to syringe the antrum, but so free was the communication with the frontal sinus that it was impossible to prevent infection of the fronto-ethmoidal regions.

November 21: A culture having been made from the antral pus it was found to be almost pure *Streptococcus pyogenes aureus et albus*, and the reinfection of the frontal sinus area made it necessary to flush out the sinus with antiseptic solutions (sterilla¹ 1 in 10); a further 20 c.c. of antistreptococcic serum was administered, and sulphate of quinine, 4 gr., and perchloride of iron given every four hours. The cellulitis had largely disappeared, being concentrated over circular area of 2 in. diameter, and the patient was in a much more satisfactory condition.

November 22: The swelling under the chin was incised, pus being evacuated. For the next few days the frontal sinus was flushed with



FIG. 4.

A similar sized frontal sinus opened by Killian's radical operation, with bony bridge. (After Professor Killian.)

1 in 20 sterilla and the antrum packed with 1 in 10 sterilla. After that the patient's progress was very favourable, the frontal sinus healing rapidly, though the antrum for some time continued to secrete pus freely. Now the patient no longer has to douche her nose, and on examination no pus will be seen at all and she will resume her work.

The frontal sinus became completely cicatrized and the scar is scarcely noticeable, and after a short period will not be visible. This

¹ Sterilla is a 5 per cent. saponaceous solution of β -naphthol.

was a particularly virulent streptococcal infection, with reinfection of the whole wound, but despite the fact that the orbital roof and inner wall were removed, the whole sinus area completely healed without any serious trouble. The patient was a nurse in poor circumstances, and in order to avoid the expense of a double operation and prolonged stay in the nursing home I did the frontal sinus at the same time as the maxillary antrum. As a rule this answers well, but, unfortunately, it happened to be an unusually virulent infection. The value of anti-streptococcic injections given early was obvious in the marked improvement in the general condition and in localizing the cellulitis. Moreover, it is noteworthy that the frontal sinus wound healed almost as rapidly and satisfactorily with flushing after it had become reinfected as in the more fortunate cases where no such procedures are required. The bone- and skin-flap united well, and with the exception of the suppuration below the chin the patient recovered with no untoward symptoms whatever and without any facial defect consequent on the operation.

Case of Thyrolingual Fistula in a Boy aged 7.

By CHICHELE NOURSE, F.R.C.S.Ed.

THE patient had a small fluctuating swelling about $\frac{1}{2}$ in. in diameter, situated at the root of the neck in the middle line, on a level with the upper edge of the sternum. It was surrounded above and on either side by a small fold of skin, and covered by a thin layer of epidermis which occasionally ruptured and gave exit to a milky fluid. The condition had been first noticed about three years before. From this point a cord could be felt passing vertically upwards in the middle line under the skin for about $1\frac{1}{2}$ in. It was operated on on January 21. The small dimple was isolated by a circular cut through the skin, from which a vertical incision was carried upwards as far as the hyoid bone. The structure was then dissected out. It consisted of a tough cylinder, the size of a swan-quill, lying in the subcutaneous connective tissue as far as the cricoid cartilage, whence it gradually passed more deeply between the muscles until it reached the thyrohyoid membrane, to which it was firmly attached in the middle line near the upper border, under cover of the body of the hyoid bone. A probe could be introduced from below for about two-thirds of the distance; the upper third of the cord appeared to be impervious. A small artery entered its deeper surface

opposite the cricoid cartilage. The wound was closed with sutures, except at the lower angle, where a small drainage tube was inserted for twenty-four hours. It healed by first intention. The fistula contained a quantity of thick opalescent fluid, a specimen of which, as well as a microscopic section of the structure itself, was shown by Dr. Wyatt Wingrave.

Dr. WYATT WINGRAVE said the discharge in the case differed from ordinary pus, in the fact that it consisted chiefly of a large number of epithelial cells, containing fat granules, like colostrum corpuscles, which stained well with osmic acid. There were also a few leucocytes. The matrix contained much mucin, and the thick mucilaginous character of the discharge was sufficient to distinguish it from pus. The tubes themselves were nearly always multiple—*i.e.*, several offshoots, or collateral tubes, lined with columnar epithelium. That condition he regarded as important, as it might account for the difficulty in curing such fistulæ by mere scraping or the application of the cautery, which might destroy one tube, but left the linings of others unaffected. Thus the only satisfactory treatment was complete removal.

Case of New Growth of the Septum.

By CHICHELE NOURSE, F.R.C.S.Ed.

A GIRL, aged 22, complained of obstruction of the left nostril. A smooth, red globular mass arising from the cartilaginous septum closed the left nostril. On the right side the corresponding part of the septum is granular, soft, and uneven. The prominent part of the tumour was removed by a snare; on microscopic examination it was found to contain giant-cells.

DISCUSSION.

Dr. WINGRAVE said, even in the absence of bacilli, he regarded the condition as a tuberculoma, because not only were there giant-cells and epithelioid activity, but the patient had tubercle in other parts of the body. It was interesting in comparison with Dr. Pegler's case shown at the previous meeting. Exception was taken to the latter because it was pronounced as tubercle on the strength of the presence of giant-cells, without evidence of bacilli; but if the diagnosis was to be made only when bacilli were demonstrated, he feared very few tuberculous cases would be recognized. The part played by epithelioid cells in connexion with giant-cells was a very wide one. They were found in the three stages of syphilis, particularly in the third, and in growths at the back of the pharynx,¹ in lupus, leprosy, lymphadenoma, and as isolated masses

¹ "Tuberculoma of the Nasopharynx," Abercrombie and Wingrave. *Med. Reports, Cent. Lond. Throat and Ear Hosp.*, i, p. 37.

in the tonsils and adenoids. He fully agreed with the view that giant-cells alone did not justify the histological diagnosis of tubercle. The term tuberculosis should be confined to cases where there was supporting clinical evidence. Experimentation could not always, as suggested by Dr. Jobson Horne, be resorted to when a rapid diagnosis was called for.

Dr. JOBSON HORNE said that he had so fully expressed his views on a similar case recently brought before the Society (see *Proceedings*, vol. ii, No. 3, January, 1909, p. 39) that his natural wish would have been to have refrained from taking part in any discussion on the present case. Dr. Wyatt Wingrave, however, in describing the pathology of Mr. Nourse's case, had expressed his inability to accept the views previously expressed by Dr. Jobson Horne. He (Dr. Jobson Horne) was therefore compelled to speak in defence of those views. He reminded his listeners that on the previous occasion he had pointed out how serious fallacies would creep into their work if they allowed the diagnosis of primary tuberculous disease of the nose to be based upon the presence of giant-cells alone in a microscopic section of the growth. He further pointed out that giant-cells were met with in the microscopic examination of nasal growths other than those occasioned by the tubercle bacillus. His own pathological and clinical experience compelled him to adhere to that statement. He did not say that the case exhibited by Mr. Nourse was not tuberculous, and it was far from him to say that his previous remarks had had any influence. But at the same time he would point out that although evidence of tuberculosis had been found in another part of this patient's body Mr. Nourse had not thought fit in his title to the present communication to describe the neoplasm, as Dr. Pegler had done in his case in which evidence of tubercle in any other part of the body was absent, as a tuberculoma. He (Dr. Horne) was astonished, but at the same time pleased, to find a footnote to a report of the meeting in which Dr. Pegler had exhibited his case,¹ to the effect that the section of the growth in Dr. Pegler's case had been submitted to Dr. Lazarus Barlow and to Mr. Shattock, and that they had without hesitation diagnosed the disease as tuberculous. Dr. Horne was pleased, inasmuch as he felt that if so eminent a pathologist as Mr. Shattock had come to the same conclusion as Dr. Pegler, then he (Dr. Horne) had published his observations on these nasal neoplasms none too soon. Dr. Horne's satisfaction was, however, but short-lived, for in course of conversation with Mr. Shattock he had learnt that the footnote in question had been submitted to Mr. Shattock in manuscript, and that he had expressed a wish that it should not be published and that he had drawn up another note which was incorporated in the *Proceedings* of the Society. Dr. Horne said he had spoken freely on this matter as he had noticed that Mr. Shattock was present, and he was desirous that the opinion of such an acknowledged critic as Mr. Shattock should be brought to bear upon a matter of public interest. He (Dr. Horne) dared not dwell upon the thought of how many cases of primary tuberculosis of the nose rhinologists would have cured at no distant date.

¹ Published in the *Journ. Laryng., Rhin. and Otol.*, Lond., 1909, xxiv, p. 29.

96 Lack: *Nasal Polypus treated by Ethmoidal Curetting*

Dr. PEGLER said he had not much to add to what appeared in the *Proceedings*, but if Mr. Shattock feared responsibility in the matter, Dr. Lazarus Barlow would share it with him, as that gentleman saw the section as soon as it was made, and without hesitation pronounced it as tuberculous. He added that, "humanly speaking, he had no doubt whatever." Dr. Horne spoke of the commonness of the condition, but in the life of the Laryngological Society, ten years, there had not been such a case shown. There had been many cases of lupus, in every clinical manifestation, but not primary tuberculoma of the nose, and therefore there should be no fear that the disease would be looked upon as of common occurrence. With regard to the cases mentioned by Dr. Horne, Dr. Pegler would be obliged if he would give details as to the fate of the guinea-pigs and other particulars.

Dr. JOBSON HORNE said that it was most gratifying to himself to have heard from Mr. Shattock that he (Mr. Shattock) had found himself in exactly the same position in this matter as Dr. Horne. Replying to a remark made by Dr. Pegler, Dr. Horne pointed out that the present was the second case of the same kind exhibited to the Society within a very short time. He (Dr. Horne) would also draw attention to at least one case of the kind brought before the late Laryngological Society of London within recent years, and in which there was no suggestion of lupus. In reply to Dr. Pegler's request for further particulars about the guinea-pigs which had been inoculated by Dr. Horne, he (Dr. Horne) was prepared to supply at a future meeting, so far as was in his power, the fullest details, as he considered that no obstacle should be placed by anybody in the way of the elucidation of a matter of such public importance.

Dr. STCLAIR THOMSON said that a case shown by another member some years ago as primary tuberculoma of the nose was in his hands subsequently, and was now clinically a chronic lupus of the nose.

Case of Nasal Polypus treated by Ethmoidal Curetting.

By H. LAMBERT LACK, M.D.

THE patient, a woman, was shown at the Laryngological Society of London in February, 1901, as a case of persistently recurring nasal polypus with suppuration in the frontal and ethmoidal sinuses. The patient has had polypus since she was aged 15, and for three years had them removed as often as once a fortnight, but in all that time had been unable to breathe through her nose. Two years before the case was previously shown, that is in 1899, the ethmoidal region had been curetted, and a year before the frontal sinuses had been obliterated. The case was shown to illustrate the advantage of ethmoidal curetting in

persistently recurring nasal polypus, and to show that the removal of the bone cured the polypus, although profuse sinus suppuration continued. The case is now brought forward to show that the results of ethmoidal curetting are permanent. In spite of the constant recurrence of the growths before the curetting in 1899, there has been no treatment since. This is the more interesting in that some speakers at the meeting in February, 1901, considered the case was not a cure.

Case of Syphilitic Stenosis of the Larynx.

By HAROLD BARWELL, F.R.C.S.

THE patient, a labourer aged 24, who contracted syphilis four years ago, and first noticed hoarseness eighteen months ago, was admitted into hospital on June 1, 1908, suffering from severe dyspnoea which had begun five weeks before, and had continued since that time with spasmodic exacerbations. On admission he was rather cyanosed, respiration very distressed with retraction at the suprasternal notch, and temperature 101.4° F. There was ulceration of the soft palate, both cords were fixed in the mid-line and ulcerated posteriorly, and there was marked arytenoid swelling. Tracheotomy was performed at once under eucaïne, and the dyspnoea was relieved. Injections of the benzoate of mercury were employed, and the ulceration healed fairly quickly; he was discharged from hospital on August 5, and went back to work, and has since been taking mercury and potassium iodide by the mouth. There is very marked stenosis of the larynx, so that on closing the tube air can hardly be forced through, but he manages to articulate so as to be understood by one standing close by.

Two Foreign Bodies removed by Bronchoscopy.

By H. LAMBERT LACK, M.D.

(1) A BONNET pin, 2 in. long. The head of the pin was lying in the right bronchus, whilst the point was sticking into the left wall of the trachea. The patient was a girl, aged 12, who had "swallowed" the pin seven days previously. It was giving rise to no symptom beyond a slight cough, and the child was quite well immediately it was removed.

(2) A piece of a whistle removed from the right bronchus of a boy aged 8. The foreign body had been present ten days, and was giving rise to signs of bronchitis.

Both objects were removed through a Killian's tube, under combined cocaine and chloroform anæsthesia.

Case of a Man one Month after Operation for Malignant Disease of the Nasopharynx.

By W. STUART-LOW, F.R.C.S.

THE patient, aged 38, a printer, was sent to the Central London Throat Hospital by his doctor for increasing deafness, especially in the right ear, nasal obstruction and intonation, painful cervical swellings and recurrent nasal hæmorrhage. The deafness had come on three weeks previously, and the nasal blockage and bleeding were of five weeks' duration, gradually getting worse. He had a large mass of soft, freely movable glands in the middle of right posterior triangle extending forwards under the sternomastoid. This enlargement had given a considerable amount of pain for a fortnight, preventing sleep. On the left side of the neck there was a smaller mass in the anterior triangle below the angle of the jaw. On inspecting the nasopharynx it was seen to be filled with a fleshy-like mass having an irregular surface, and on palpation a soft growth was felt filling the cavity. This bled very freely on palpation; it seemed to be attached to the roof with a broad undefinable base. His heart being weak and dilated, and his spleen and liver enlarged, it was not considered safe to give a general anæsthetic; therefore the glands were removed and laryngotomy performed under local anæsthesia—viz., the injection of a 1 per cent. cocaine solution. The pharynx was firmly plugged with a large captive sponge and the mass removed from the nasopharynx with forceps, bent scoops and scrapers. The bleeding was very free, but the patient made an excellent recovery and returned home in a week. The case is shown with the object of proving the value of local anæsthesia in such cases, and the advisability of early operation.

Dr. Wingrave's Report and Microscopic Specimens.—Blood count: white corpuscles, 35,000 per c.mm.; red corpuscles, 5,140,000 per c.mm.; leucocytes—polynuclear, 70 per cent.; mononuclear, 25 per cent.; lymphocytes, 5 per cent. Diagnosis, leucopænia: Tissue removed

from nasopharynx consisted of adenoid-like tissue, infiltrated and in parts replaced by endothelial cells. In parts these cells were diffused and mixed with lymphoid elements; in other parts the endothelial masses grew as solid interlacing cylinders with an intervening stroma of white fibres and elongated fusiform cells. No "giant-cells." No bacteria of any kind. Nature: Endothelial sarcoma. Glands removed from left side of neck: Five masses, largest weighing 28 gm., total 41 gm.; soft but cut like udder, firm texture and not brittle, no signs of caseation and few small foci of blood extravasation. Films of scraping from cut surface showed large endothelial cells mixed with a few erythrocytes, leucocytes and lymphocytes. No bacteria of any kind. Cultures sterile. All these glands consisted of endothelial growth replacing more or less the normal lymphoid elements. The capsules were attenuated, and just beneath in places a thin zone of lymphoid tissue was left. The endothelial growth was similar to that in the pharynx, but showed a greater tendency to lobulation. The stroma was better marked and in contrast with the endothelial masses. These consisted of round, oval and spindle-shaped cells having pale oval nuclei, some of them undergoing heteromitosis. There were no tubercle bacilli or any other forms of bacteria. Further tissue removed from nasopharynx: This was similar in structure to the fragment removed in the first instance for diagnosis, with the exception that the neoplastic portions were more sharply defined and very vascular. Glands removed from left side of neck, large and small, total weight 30 gm. They were similar to those on the right side. A few of the smaller ones showed but little of the neoplastic deposit. There were no giant-cells and no bacteria, but heteromitoses were present. Nature of the growth: An endotheliomatous neoplasm of the large alveolar type (sarcomatous).

DISCUSSION.

Mr. WESTMACOTT asked whether Mr. Stuart-Low was satisfied that there were not some glands on each side, just over the carotid. He was suspicious of a localized ill-defined thickening in that area on the right side.

Mr. STUART-LOW said he could not be absolutely certain about the glands on the right side, but a swelling came up on this side immediately after the operation, and he thought that it was either inflammatory or due to the same cause as the ecchymosis around the right eye—viz., effusion of blood.

Case of a Man with a singularly Symmetrical Syphilitic Septum.

By W. STUART-LOW, F.R.C.S.

THE patient, a young man, came complaining of stuffiness of the nose of three months' duration. A history of syphilis two years ago was elicited. On the nasal septum from the vestibule upwards on each side pale, papillomatous-looking growths were seen projecting into the passage and obstructing it. The septum felt very thin and unusually movable, and the upper triangular cartilages were loose and seemed to be falling inwards.

Dr. Wingrave's Report and Microscopic Specimens.—Growth from nasal septum consisted of granulomatous tissue showing a tendency to fibrosis. Lymphocytic infiltration was well marked with but slight endothelial activity. There were no "giant-cells" but considerable fibroplastic change. The surface epithelium was of the stratified squamous type, thickened and corrugated. No signs of superficial ulceration. Endothelial activity was visible in several small vessels (arteritis obliterans). No bacteria of any kind were present in the tissue. Nature of tissue: granulomatous, probably tertiary syphilis.

A Modification of the Posterior Rhinoscopic Mirror.

By DAN MCKENZIE, M.D.

THE modification consists in the mirror having a moderately convex surface instead of the usual plane surface. The advantages of the convex over the plane mirror are as follows: The field reflected is much more extensive; thus, the archways of both posterior choanæ, the septum from its upper to its lower end, and the turbinals can all be seen in the mirror at once; so that the relationships of polypi, tumours, &c., are more rapidly and accurately defined. The image in the convex mirror, though smaller, is certainly no less clear than that in the plane mirror, if a bright illuminant be used. Finally, with the convex mirror, the examination of the posterior nares occupies much less time than with the plane mirror, a great advantage when examining nervous patients.

Crescentic Subglottic Web occluding the Anterior Half of the Lumen of the Larynx in a Male aged 38.

By WILLIAM HILL, M.D.

It was proposed to remove it through a thyro-fissure and prevent reunion by means of the exhibitor's laryngeal splint, which was successfully employed in a case shown last November.

DISCUSSION.

Mr. BARWELL said that perhaps the printed description was not accurate, as he could not see anything which could be called a crescentic web. The ventricular bands appeared to come together in the front, and there was considerable swelling of all the front part of the larynx as far as the subglottic region, but it did not seem to be web-shaped. The whole of it was succulent and not very cicatricial, and he did not think it was now in a suitable stage for splitting the thyroid. If, as appeared from the history, it was due to syphilis, he suggested that antisiphilitic treatment should be continued longer, and he advised treatment with Schrötter's bougies before attempting thyro-fissure.

Mr. CLAYTON FOX said that at first he had great difficulty in finding any cords, and his opinion was the same as Mr. Barwell's. But after a time he got a clear view of them; there appeared to be a thin web between their anterior ends, such as one observes as a congenital condition. The ventricular bands were swollen and red, probably the result of chronic laryngitis, either simple or syphilitic. He thought that division of the web followed by the insertion of an O'Dwyer's tube would be good treatment; there would be less risk of impairing the efficiency of the cords than by thyrotomy.

The PRESIDENT said the picture was to him that of a tuberculous lesion, and if the history of syphilis had not been so definite he would have expected it to be such.

Mr. WESTMACOTT said the whole nose was occupied by crusts typical of syphilitic disease in the nose. The patient was probably getting infiltration or chronic catarrh of the larynx, which accounted for the thickening all round the glottic aperture.

Dr. SCANES SPICER said that, in addition to depression of bridge and perforation of nasal septum, there had been ulceration, loss of substance, and cicatricial deformity of soft palate, so that it was clearly a tertiary syphilitic case. Personally, in spite of repeated and careful examination with the laryngeal mirror, he could not see any web, but Dr. Hill had lately been working at the larynx a good deal by direct examination, and it was possible he might have seen a web by that means beneath the other thickening which was present.

Dr. HILL subsequently wrote to express surprise that so many experts were unable to detect what was described by the title as a subglottic web; syphilitic hyperplasia, semilunar in form, and occupying nearly the anterior half of the lumen of the larynx in the subglottic region would perhaps be a better description. Since the case had been under observation the anterior half of the glottis had been *nearly* obliterated, possibly from contraction of the subglottic web bringing the cords closer together, and it seemed probable that on the day of the meeting the approximation of the cords and ventricular bands from temporary catarrh was sufficient to occlude completely the anterior part of the glottis and shut out the morbid lesion below. Dr. Hill saw the condition plainly enough the day before the meeting, and also had verified it since both by mirror laryngoscopy and by endoscopy, the former giving the more extensive view. Fortified by what he overheard, the patient has for the time declined operation, but it is hoped that he may be shown again at the March meeting.

Cystic Disease of Right Antrum.

By H. W. FITZGERALD POWELL, M.D.

THE patient, a female aged 31, came under observation on February 1, 1909. She complained of swelling of right cheek and gums. Eight years ago she had pain in the face, caused by a decayed tooth which was extracted with some difficulty; after extraction pain was relieved. Since that time she had no trouble until recently, when she noticed a swelling. On examination a swelling of right side of face was observed, and on placing the finger on the anterior wall of antrum a portion of the wall was felt to be absorbed and a fluctuating swelling was felt. On palpating the hard palate a similar condition was found, and a distinct fluctuation could be felt between the two fingers. Transillumination showed the right antrum quite translucent. There was a question as to the character of cyst; it was probably dental.

Vasomotor Disturbances in the Mucous Membranes of the Mouth and Throat of a Gentleman aged 38.

By Sir FELIX SEMON, K.C.V.O., M.D.

THE patient was brought to me by Dr. Carswell last week on account of periodical changes and unpleasant sensations in the mucous membrane of his lips, cheeks, tongue, and soft palate, which had developed since last August without any special reason being known. The attack

usually begins by the formation of a slight pimple in the centre of his lower lip. Within a few hours, or sometimes days, this enlarges into a limited bright red spot, and then gradually, or sometimes very quickly, subsides. But after this initial phenomenon, as a rule, very quickly spots of intense congestion develop on the tongue and mucous membrane of the cheeks and of the soft palate. The right side is the one most usually affected. These spots change their aspect not merely from day to day, but sometimes within a few hours, in that they rapidly increase in size, become more deep in colour, and are sharply limited from their neighbourhood. After having been in existence for a few days they gradually, sometimes quickly, disappear, and the mucous membrane returns to its normal condition, only to become affected within a few days again in a similar way. When the congestion is at its height the parts affected look as they do to-day, like raw beef, and are very sensitive, especially after taking food. Otherwise they give no trouble, and the patient's general health is not disturbed. There have never been any manifestations on the skin. The patient is of very nervous temperament and inclined to dyspepsia.

At to-day's demonstration it will be seen that the whole right and part of the left half of the surface of the tongue is deeply congested, having the "beefy" aspect above mentioned, this being sharply limited against the healthy parts of the surface. Some small spots of congestion are seen on the left lateral margin of the tongue. Almost the whole right half of the soft palate is similarly deeply congested, but the uvula, which yesterday was similarly affected, looks much more normal to-day. The congested spot on the lower lip, which was very much in evidence as late as the day before yesterday, has quite disappeared, and there are no spots on the mucous membrane of the cheeks. Altogether the affection is stated to have become less violent in all its manifestations than it was at the beginning.

That the case is one of vasomotor origin seems evident, but opinions are invited as to the exact causation, mechanism, and treatment.

A Case of Tumour of the Right Tonsil and Palate.

By H. J. DAVIS, M.B.

THE patient is a man aged 69. There is a large infiltrating non-ulcerated mass implicating the soft palate and tonsil, which is almost in contact with the opposite side; symptoms very slight and of three

months' duration. Several glands at the angle of the jaw, but these are small, free, discrete, and movable. The growth is very hard, but so tense and rounded that it has the appearance of containing fluid; no puncture has been made. It is evidently a sarcoma, and probably cystic. The tumour is enormous—the largest the exhibitor has ever seen in the mouth. Suggestions as to advisability of operating would be welcome.

DISCUSSION.

The PRESIDENT said that, apart from the gland, the case looked promising. But the gland was soft.

Dr. FITZGERALD POWELL said the tonsil should be enucleated, if possible, and a microscopic section obtained of it. If it was malignant the soft palate had been involved, and a large part of it would require removal; if it was simple, it would easily be enucleated without further trouble.

Case of Unilateral Paralysis of the Tongue.

By W. JOBSON HORNE, M.D.

THE patient, a woman aged 41, a music teacher, was brought before the Section on February 7, 1908. The history given at that time was that the paralysis had set in suddenly five months previously and two or three days after an injection was made into the gums for the extraction of teeth (one right lower and two left upper dental stumps) prior to the making of a new dental plate; but before this extraction she had experienced severe pain, or rather tightness, in the left occipital region and pain over the left articulation of the jaw, with some inequality of movement of the same. Loss of taste had been noticed on the left side of the tongue. The patient had been under electrical treatment for two months at a general hospital, discontinuing on January 15, 1908; the condition had remained unchanged in spite of the treatment. The case was shown at that time with a view of eliciting opinions as to its nature and treatment. In the discussion upon the causation of the paralysis various views were expressed, a report of which will be found on p. 54 of the first volume of the *Proceedings* of the Section. Since that time the patient has been placed upon a course of iodide of potassium without any material change resulting. The case is brought forward again in accordance with a wish expressed by the members of the Section, who kindly interested themselves in it on the previous occasion.

DISCUSSION.

Dr. DAN MCKENZIE said that when the case was shown previously it was suggested by him that it was functional hemiplegia, and on looking at it again to-day he saw no reason for altering that opinion.

The PRESIDENT (Dr. Grant) remarked that there was no wasting of the paralysed half of the tongue, and he quite agreed with Dr. McKenzie's conclusion.

Mr. CLAYTON FOX said the patient had been for some considerable period under his notice, and he was aware of the various remarks which were made at the last meeting concerning the possibility of peripheral disease below the nucleus, and in regard to functional conditions. The nuclear considerations could be put out of court, as no other nuclei were involved. He had tested her for the functional stigmata, but could find none. He regarded the case as peripheral neuritis of the hypoglossal, cases of which had been recorded.

Case of Laryngeal Vertigo.

By W. JOBSON HORNE, M.D.

THIS case was exhibited in consequence of a discussion which arose out of a case of laryngeal vertigo occurring in early tabes brought before the Section on November 6, 1908.¹ The patient, a man aged 69, consulted Dr. Horne on account of difficulty in swallowing of some four weeks' duration. The patient's private medical attendant had regarded the case as one of spasm of the glottis. The patient had been subject to bronchitis in the winter for many years, with a good deal of expectoration but no blood; the voice had been hoarse for about ten years. Clinically nothing abnormal was found in the fauces, the larynx or the thorax to account for the symptom of dysphagia. There was no clinical evidence of tabes. The patient, however, gave the following history: Two or three years ago, whilst sitting, and feeling quite well, he had a sudden fit of coughing with a sense of suffocation, loss of consciousness, and he fell off the chair. He recovered his senses, according to statements made by his friends, in about two minutes. He had had several similar attacks since, but never with loss of consciousness. Dr. Jobson Horne submitted that the case was one answering to the commonly accepted description of laryngeal vertigo, but at the same time he would

¹ See *Proc. Roy. Soc. Med.*, ii, No. 2 (Laryng. Sect.), p. 16.

welcome most cordially criticism on that point by the other members of the Section. Further, he considered that if the case in the opinion of the members conformed to that rare condition of so-called laryngeal vertigo, it should be recorded in the *Proceedings* of the Society.

Removal of Foreign Bodies by Bronchoscopy and Œsophagoscopy.

By HERBERT TILLEY, F.R.C.S.

(1) METAL cap of a lead pencil removed by lower bronchoscopy from a primary branch of the left bronchus (with skiagram).

(2) Safety-pin removed from the œsophagus of an infant aged 6 weeks. The pin was open and the sharp end pointed upwards.

Laryngological Section.

March 5, 1909.

Dr. DUNDAS GRANT, President of the Section, in the Chair.

Case of Hunterian Sore inside the Lip of a Young Woman.

By DUNDAS GRANT, M.D.

M. C., AGED 23, first seen February 17, 1909, complained of swelling in the neck and ulcer in the mouth, the latter being of two and a half months' duration. The voice had been slightly hoarse. There was an oval ulcer, $\frac{1}{2}$ in. in long diameter and $\frac{1}{2}$ in. across, on the inner surface of the right half of the lower lip. When first seen on February 17 there was well-marked induration, which could be felt by grasping the skin. There was a mass of enlarged glands, discrete, firm, indurated and painless, in the right submaxillary region, and just faint redness external to the tonsils on the anterior pillar of the fauces. (She was ordered pil. hydrarg. c. cret.) On March 3 the induration of the ulcer was considerably less; the glands on the left side and the postcervical glands were enlarged. Spirochætæ were found in the fluid drawn from the base of the ulcer by means of a hypodermic needle, but not from the surface nor from the glands. There is no history obtainable to throw any light upon the mode of infection.

Case of Tuberculosis of the Larynx with Improvement following Galvano-caustic Puncture.

By DUNDAS GRANT, M.D.

A MAN, aged 28, became hoarse in November, 1906, when he had hæmoptysis. He was submitted to in-patient treatment during the months of November and December, 1907, both apices being affected. In January, 1908, when he first came under the exhibitor's observation, he was so hoarse that he could only whisper; there was

then considerable infiltration of the left cord, with superficial ulceration and slight infiltration of the right cord. He was at first treated with an inhalation of a powder of di-iodoform and instructed to give his voice complete rest. On January 21 his voice remained the same, and galvano-caustic puncture was made in the middle of the left cord, the same being done on the right side a fortnight later. Soon after this the voice returned to such an extent that the patient could utter a few words in a natural tone, but then it went off to a whisper. On March 3 it was observed that his voice had improved, and it has remained so ever since; there was no obvious ulceration, and there was some evidence of cicatrization and contraction of the left cord, although some hyperæmia still persisted. In May the throat was a little painful, and slight ulceration was observed at the posterior extremity of the right vocal cord; he was then treated with the inhalation of di-iodoform powder and a fortnightly injection into the trachea of guaiacol and menthol in olive oil. In July 50 per cent. lactic acid was applied, and in August a combination of formalin, lactic and carbolic acids, as recommended by Lake. Fortnightly applications of lactic acid, increasing in strength from 60 per cent. to 80 per cent., were made until last December. In January of the present year no ulceration was to be seen, though the mucous membrane was somewhat reddened, while the voice had gained in strength. At the present time his voice is better than it has been for eighteen months. There is no ulceration, but simply a slight hyperæmia of the mucous membrane of both cords.

**Case of Tuberculosis of the Larynx in which Galvano-caustic
Puncture Treatment has just been started.**

By DUNDAS GRANT, M.D.

THE patient, a man aged 42, has been affected with pulmonary tuberculosis for two years, and with hoarseness for nine months. He has just come under the exhibitor's observation with almost complete extinction of the voice and considerable expectoration. There is an extensive elongated sessile growth, apparently projecting from the left ventricle and covering the anterior two-thirds of the vocal cord. During phonation it fits into a hollow above the right vocal cord. There is slight infiltration of the aryepiglottic folds; the larynx is otherwise comparatively unaffected, and there is no defect in the mobility of the cords. A puncture was made into the middle of the base of the growth four days ago.

Case of Telangiectasis with Epistaxis.

By LAMBERT LACK, M.D.

THE patient, a woman aged 53, has suffered from epistaxis for thirty years. The bleeding at times has been profuse, and latterly has necessitated packing the nose once or twice a week. The bleeding always arises from the anterior part of the septum on the right side. The patient is, in consequence, very anæmic. There are typical raised blood-red nævoid spots on the right cheek, lips, tongue, and palate. The patient says these sometimes disappear. She has had them also on the chest. There is no history of a tendency to bleed in other ways. She has seven children, the eldest being aged 34, and many brothers and sisters, and there is no history of any similar disease in the family.

DISCUSSION.

Mr. WAGGETT asked what was the coagulation-time of the blood.

The PRESIDENT (Dr. Dundas Grant) asked whether there was any evidence of cirrhosis of the kidney, as the pulse-tension was above the normal, although, of course, it diminished with the hæmorrhages.

Dr. LACK replied that he was unable to answer either of the questions. The woman was intensely anæmic at first, but since she had taken calcium lactate the hæmorrhages had stopped, and she had much improved in appearance.

Case of Thyroid Tumour of the Tongue (?).

By W. STUART-LOW, F.R.C.S.

A FEMALE, aged 32, with a large, firm swelling at the base of the tongue, came recently to the Central London Throat and Nose Hospital, complaining of something growing at the back of the tongue, thickness of the voice, and of a constant desire to swallow. A similar condition had existed eleven years before, when an operation was performed at St. Bartholomew's Hospital. She was operated upon again for a recurrence nine months ago, also at St. Bartholomew's Hospital. It was now proposed to perform a preliminary laryngotomy, firmly plug the pharynx, and remove radically the tumour.

DISCUSSION.

Dr. SCANES SPICER thought the swelling was largely cystic, as shown by the translucency on the right side. He said he would puncture first, and, if his surmise was correct, cut out a piece of the cyst-wall and then scrape and mop out with chromic acid solution. If the swelling re-formed, an excision on the lines suggested was certainly indicated.

Mr. ATWOOD THORNE asked whether Mr. Stuart-Low had the notes of the previous operations in the case.

Mr. CRESSWELL BABER said that some years ago he showed at the Laryngological Society¹ a case of thyroid tumour at the base of the tongue which looked and felt like a cyst. It was removed with the galvanic snare. The question was whether, in the present case, a serious operation of the kind suggested was required. The patient's chief complaint was constant swallowing, but there was apparently no difficulty either in swallowing or breathing.

The PRESIDENT reminded members of a case which he showed a year ago, but in which the discomfort was much greater than in the present case, and the voice was also much interfered with. He was able to remove the tumour entirely, but he always had some misgiving as to whether there was any remnant of thyroid gland, or some accessory thyroid. But up to the present there had been no sign of myxædema, and he presumed sufficient of the thyroid body was present.

Dr. FITZGERALD POWELL thought it would be found to be a thyro-lingual cyst. There appeared to be some fluctuation, and had it been similar to a case of the President's, which he himself had had the privilege to see removed, he did not think there would have been a recurrence after operation. The fact of recurrence indicated to his mind either malignant growth or thyro-lingual cyst, which had not been thoroughly destroyed. He had had a case of thyro-lingual cyst in the same position in the case of a child, which at times was as large as a walnut, and he was able to remove it with a snare, and destroy the base with a Paquelin cautery. If it was a solid tumour, similar to that in the President's case, an extensive subhyoid pharyngotomy and possible removal of a large piece of the base of the tongue would be necessary.

Mr. STUART-LOW, in reply, said the temporizing operations which Dr. Spicer suggested might have been valuable if there had not already been two operations by distinguished surgeons. He proposed to do laryngotomy, plug the pharynx, and split the tongue. He hoped by pulling the two parts of the tongue asunder to get access. No doubt a piece of thyroid had been taken away in the former operations. He had examined the thyroid gland in the neck, and considered it normal.

¹ *Proc. Laryng. Soc. Lond.* (1894-95), 1896, ii, p. 1.

Case showing an Unusually Large and Long Tongue.

By W. STUART-LOW, F.R.C.S.

A BOY, aged 8, with a remarkably long tongue. From base to tip it measures 5 in. The right side seems to be semi-atrophied in the anterior two-thirds. He had a large mass of adenoids, which have been removed. The jaw is well developed, and the teeth are not crowded; this would seem to show the potent influence of the large tongue in pressing out the jaw into a good arch, and to militate against the view, so often expressed, that the presence of adenoids chiefly determines the shape of the jaw.

Dr. ANDREW WYLIE said he also had a case like Mr. Stuart-Low's. His case was a young lady, who was able to put her tongue over the tip of her nose, and also insert it into the nasopharynx, which was described by her school companions as "swallowed." When the tongue was so inserted, the tip of it could be seen by means of a speculum through the anterior nares. At one time, having a tooth extracted, during the administration of the gas this abnormal tongue covered the glottis, and for a few moments endangered her life. Anæsthetists should therefore always take into consideration any abnormally long tongue.

Uvula with Growth on Left Side from a Man aged 42.

By JAMES DONELAN, M.B.

FIRST noticed about two years ago as a small papilloma, which gradually became white and warty-looking. There is no history or other evidence of syphilis. From time to time a swelling as large as the uvula appeared above and below the papilloma whenever patient had a "cold," or had made any unusual vocal effort. During the last four months this secondary enlargement had been more persistent, and with a progressive increase in the uvula itself there had been constant cough. For some weeks before, and on the day the uvula was removed, only the papilloma could be seen, but at the instant of cutting the secondary enlargements assumed, and still preserve, the appearance previously observed. The uvula was removed only with scissors to avoid injuring the site of the swelling with forceps. A microscopic examination has not yet been made.

Right and Left Rectangular Chisels for removing Nasal Wall of Maxillary Antrum, either as an independent procedure for the Cure of Antral Suppuration or as part of the Radical Operation through the Canine Fossa.

By JAMES DONELAN, M.B.

DISCUSSION.

The PRESIDENT said that on one occasion, when he had used a rectangular chisel of another sort, and cut forward with it, the patient suffered for some days with epiphora, owing to his having damaged the nasal duct. She recovered from it, but it was an accident likely to ensue unless great discretion was exercised.

Mr. HERBERT TILLEY said he had seen epiphora follow the radical operation through the canine fossa, in which such an instrument had been used. Such chisels were excellent for the first case they were used on, because they were then quite sharp. To make them serviceable they should be properly sharpened before each operation. One was apt to break a piece of bone into the antrum if the chisel was not very sharp. A better instrument was one which would pull the inner antral wall into the fossa, so that it could be grasped with suitable forceps. He had not used Dr. Donelan's particular instrument, but others very similar—viz., those introduced by the late Dr. McKay Macdonald.

Dr. DONELAN replied that he had used these or similar instruments in a great number of cases, now close on a hundred, in the course of the last six years. The present form of the blade was the third and, so far, most satisfactory modification. Of course these, like other instruments, required to be overhauled before operation. A year or so ago he was operating on a young gentleman, and the chisel had been re-sharpened, but unfortunately ground too thin. On turning the instrument at the end of the cut, so as to jerk the removed fragment into the meatus, the blade broke, leaving a piece in the antrum. It could not be found at the time, owing to the hæmorrhage, but having packed the cavity lightly with some gauze, the piece came away in the packing a little later. Of course, one had to be careful about the nasal duct, and much depended on the site of the initial puncture. At the same time, it should be remembered the nasal duct had been wounded over and over again in all sorts of operations on the jaw and within the nose without any serious consequences whatever arising. The great advantage of instruments of this kind was that a clean-edged opening not so liable to cicatricial contraction could be formed, while by cutting the nasal wall of the antrum down to the floor adequate access and drainage were secured, and the radical operation obviated in many cases.

Laryngeal Growth in a Man aged 45.

By IRWIN MOORE, M.B.

PATIENT has been hoarse for eighteen years, and latterly there has been some stridor. About a month ago, when first examined, the left ventricular band and vocal cord were entirely concealed by what looked like a grey cyst, apparently springing from the laryngeal surface of the epiglottis. On attempting to remove this with Mackenzie's forceps, Dr. StClair Thomson found the growth extremely tough. On removal of the cyst the present condition was revealed—*i.e.*, infiltration of the left ventricular band with a curious abnormality of the left arytaenoid. The left vocal cord is now visible. It is healthy and moves naturally. Small portions of the left ventricular band were removed, and under the microscope were found to be simply granulation tissue.

DISCUSSION.

The PRESIDENT asked whether it was simple granulation tissue, and where the growth arose from. Also, was the stridor of gradual development? He hoped there would be an opportunity of seeing the case again later.

Dr. IRWIN MOORE replied that the growth appeared to come from the laryngeal surface of the epiglottis, and it looked like a cyst on January 15th. Dr. StClair Thomson got hold of it with some difficulty with the Mackenzie forceps, and it seemed to collapse, leaving the condition now seen. There was no specific history; he was a married man with four healthy children. Since the notes were made the vocal cord could not be seen, but it could be seen a fortnight ago. He had been hoarse for eighteen years. The voice had improved somewhat since the operation. There had not been stridor, except since the removal of the cyst.

(?) Gumma of Larynx.

By J. DUNDAS GRANT, M.D., and DAN MCKENZIE, M.D.

THE patient is a woman, aged 32. History of gradually-increasing hoarseness of three months' duration, followed by difficulty in breathing and stridor. On examination the left side of the larynx was seen to be swollen, and a conical outgrowth could be made out during deep respiration, springing from the left subglottic region. Attempts were made to

remove a piece of this outgrowth for microscopical examination, but the forceps could not reach it, and the attempt produced stridor and dyspnoea so grave that, after hurried intubation, a low tracheotomy was forthwith performed under cocaine-anæsthesia. The patient was given potass. iodid. gr. x t.d.s., with liq. hg. perchlor., and great improvement in the local appearances has resulted. The tracheotomy tube has now been removed and the larynx presents the following appearance: The left side is red and congested, and at the site of the left cord is a row of granulations of a lively red colour. No trace of subglottic outgrowth remains. Movements are normal. There are no signs in the lungs; no tubercle bacilli in the sputum; and there has been no reaction to Calmette's ophthalmo test.

Dr. W. HILL said he thought a piece could have been removed through Jackson's laryngoscopic tube, or by Watson Williams's straight-punch forceps.

Case of Chronic Infiltration of the Larynx.

By C. A. PARKER, F.R.C.S.Ed.

THE patient, aged 35, a butcher by trade, has done a lot of shouting in the street outside his shops. Five years ago he began to suffer from hoarseness, which gradually got worse until nine months ago he became voiceless. Five months ago the patient put himself under treatment, and his voice has improved to a limited extent. There is a definite history of primary and secondary syphilis at the age of 16 or 17. The patient has taken large doses of iodide of potassium for the last four months with but little alteration in the laryngeal condition. At the present time there is marked infiltration of the laryngeal surface of the epiglottis, extending on to the left aryteno-epiglottidean fold, giving the parts a pale and almost nodular appearance. There is a huge swelling in the interarytenoid region, extending below the cords, red in colour, and of firm appearance. There is also some infiltration of the ventricular bands, and below the left cord there is a definite pale growth.

The patient is shown from the point of view of diagnosis and treatment.

DISCUSSION.

Dr. LAMBERT LACK said he had seen a case even more marked than this, and removed the growth with Lake's cutting forceps. On section it was found to be a chronic inflammatory condition; fibrous tissue, with an

excess of epithelium. Such growths showed a great tendency to come back again. He believed it was only an exaggeration of what was often seen in the interarytænoid space in cases of chronic laryngitis.

Mr. MARK HOVELL said he had seen several such cases, but they had been in syphilitic patients. He regarded the present condition as chronic laryngitis in a syphilitic subject.

Mr. HERBERT TILLEY said these patients were apt to have hyperplasia of fibrous tissue in other parts of the body. Such conditions might be met with in those addicted to alcohol, especially spirit drinking, in whom cirrhotic kidneys and liver and a high-tension pulse were present. He asked what was the condition of those organs in the present patient. Some years ago there was a similar case in the Golden Square Hospital. The patient was in distress from dyspnoea caused by such an interarytænoid growth. She improved with purgation and rest in the hospital for about a week. Three days after leaving the hospital, however, she was walking across her room, and fell down dead. The growth was removed, and Dr. Jobson Horne sectioned it for him. It was well-marked interarytænoid hyperplasia. The speaker also found that her liver was of the hobnailed character. He believed such cases to be of the nature of a chronic submucous hyperplasia. They nearly always recurred after removal by cutting operations. He thought that a better result might be produced if the parts were well cocaineized, and, guided by the direct method, a galvanocautery point were passed into the interarytænoid mass once a week, so that shrinkage under the epithelium would be encouraged, and a clearer airway established.

Dr. JOBSON HORNE noted that the case was shown from the point of view of diagnosis and treatment. He regarded the case as one of *pachydermia laryngis verrucosa*. In using the term *verrucosa* he did not wish to draw any distinction in this case from what is commonly termed *pachydermia diffusa*, as he himself had pointed out,¹ the warty form was only a more advanced phase of the diffuse. In the present case, as had been described in the notes, there was a general involvement of the larynx, although the outstanding feature was the neoplasm in the arytænoid region. On the occasion to which Dr. Horne had already referred he had expressed the opinion that in the majority of cases the laryngeal condition was more than "skin deep," and was merely a local manifestation of a perhaps more distant and widespread disease. It was commonly more than idiopathic. Dr. Horne, therefore, fully agreed with Mr. Tilley that this was a case which might be associated with fibrosis in other organs. In the matter of treatment he regretted that he could not altogether agree with Mr. Tilley, who had suggested submucous punctures into the laryngeal growth with the galvanocautery. That was the measure one would adopt to induce pachydermia in a larynx, and it represented the scientific basis of the treatment of laryngeal tuberculosis by submucous punctures with the

¹ See *Lancet*, 1899, ii, p. 607.

cautery. By that method of treatment, as he (Dr. Horne) had pointed out, one initiated and imitated nature's method of bringing about an arrest of laryngeal tuberculosis, by causing a development of fibrous tissue around the deposit of tubercle. In the case before them that evening he would deal with the interarytænoid growth with a double-cutting curette. He was fully alive to the tendency of these growths to recur, but the recurrence would be infinitesimal by the side of the consequences of the irritation occasioned by a galvanocautery puncture. Other parts of the larynx would also require local treatment.

Dr. DAN MCKENZIE said that to these cases where the pachydermia, hypertrophy, and cedema were all present, the name "elephantiasis of the larynx" might be properly applied. What distinguished the class was the presence of lobulated tumours, due undoubtedly to lymphædema from obstruction in the lymphatic vessels of the larynx, as a result of long-standing chronic inflammation. The rational treatment of the disease would be the drainage of the swamped areas into the tissues of the neck by lymphangioplasty, if that operation could be applied to the larynx.

Dr. W. HILL said he did not see why the condition should not be removed. These growths were very tough, and were best removed through an endoscopic tube, which admitted of a good hard pull on them with forceps in a direct line. He had had his Dundas Grant forceps straightened out, and they would remove such growths without doing harm. He would expect great improvement from operation in such a case.

Dr. FITZGERALD POWELL thought it an unusual case of pachydermia following chronic laryngitis, possibly set up by syphilis. The pachydermia was also to be found on the ventricle and aryepiglottic folds, which was not usual in pachydermia. Of course, the patient should have treatment applicable to chronic laryngitis, and not have any alcohol or tobacco, and he should be put on large doses of iodide of potassium. After a time, if the swelling subsided somewhat, it could be removed with cutting forceps; but it would be difficult to get forceps strong enough to cut away that tissue through a Killian's or Brüning's tube.

Dr. P. McBRIDE said there was one respect in which the present case differed from ordinary pachydermia. Pachydermia might arise in those who drank and smoked, and in those who were tuberculous subjects and syphilitic subjects. But in this case, while he thought the thickening of the base of the epiglottis and aryepiglottic fold might be secondary to the condition of the larynx, there was an appearance on the left side, deep down, which gave the idea of being either a soft mass or an ulcerated area, and in that respect he thought it differed from other cases which he had seen.

The PRESIDENT thought the case might be called one of syphilitic pachydermia. The swelling was confined exclusively to the interarytænoid space, and there were several somewhat nodular excrescences elsewhere. He thought the syphilitic factor in the case was a very important one. He asked whether

Mr. Tilley would press the advisability of galvano-caustic puncture in the case; such a measure seemed better in a soft round-celled growth, so as to cause the production of an area of sclerosis. He always had some hesitation in using the galvanocautery with any vigour in the interarytænoid space. The arytænoid cartilages might be drawn together by cicatricial contraction, and serious laryngeal stenosis might occur. In soft tubercular swellings in the interarytænoid space he had found its use very valuable.

Mr. HERBERT TILLEY, replying to the President, said the growth looked softer than ordinary, and that was why he had suggested the measure. He had never used the cautery in such a case. No tube was required, and they could be reached with an ordinary laryngeal spatula.

Mr. C. A. PARKER, in reply, said the chief point in the case was the wide distribution of the infiltration. He had seen almost as large interarytænoid swellings, but never such a wide distribution, and it was for the latter reason that he brought the case forward. He agreed that it was chronic laryngitis, influenced by the fact that the man had had syphilis. His inclination had been to take away, by the direct method, the excess growth below the left vocal cord, as he thought that was the chief cause of the tremolo character of the voice. He thought that these interarytænoid pachydermatous swellings generally recurred if removed, and he had intended to leave that part of the trouble alone. However, after the remarks which had been made and the opinions expressed he would reconsider the matter.

Case of Laryngeal Neoplasm, with Microscopic Specimen.

By W. JOBSON HORNE, M.D.

THE patient, a woman aged 64, was brought before the Section at the meeting held on November 6, 1908. The history of the case was that there had been a severe cough following influenza in the previous April, leaving impairment of the voice, and at times aphonia. The growth occupied the posterior half of the right side of the larynx; it was situated above the vocal cord and between the ventricular band and the arytænoid region. It hid two-thirds of the right vocal cord from view, and appeared to spring from the ventricle or the ventricular band. The surface was moriform in appearance. The growth presented the appearance of an excrescence. The right cord moved freely, and so did the left. There was no other lesion in the larynx. A piece of the growth projecting beyond the edge of the cord had been removed by Dr. Horne, and a section had been cut at right angles to the surface of the growth for microscopic diagnosis. This section was exhibited. The

opinion expressed by Dr. Horne at that time was that the growth was an innocent one and its nature was that of a papilloma. Since the removal of a piece for purposes of diagnosis the excrescence had increased in size, and the movement of the right vocal cord had become impaired. A further portion was removed, this time more deeply, together with some of the underlying laryngeal tissue, for further microscopic investigation. The patient was exhibited again, with a view of eliciting the opinions of members upon the nature of the growth.

DISCUSSION.

The PRESIDENT asked how long it was since tissue had been removed, so that traumatism might be excluded. He thought the case had now an aspect suggestive of malignancy.

Mr. WAGGETT said that the case was not unlike that from which he exhibited a specimen to-day. The clinical conditions were also similar, with the exception that in his own case the growth no longer remained hooded by the ventricular band, but had already pierced it. The growth had been removed on two occasions by another surgeon during the past two years, and had recurred on each occasion. The microscope showed papilloma. Nevertheless, there was at the present time a large, hard, metastatic growth in the neck. He suspected that Dr. Horne's case was also malignant.

It was agreed to refer the specimen to the Morbid Growths Committee.

Case for Diagnosis.

By E. FURNISS POTTER, M.D.

PATIENT, a man aged 48. Infiltration and ulceration in the arytenoid, aryepiglottic, and epiglottic regions. Symptoms: dysphagia for five weeks only. Voice thick and toneless, suggestive of peritonsillar abscess; not hoarse. No luetic history. Epiglottis represented by stump apparently adherent to base of tongue. Cords not visible owing to swelling and presence of thick secretion. Indurated gland on each side of neck.

DISCUSSION.

Dr. DAN MCKENZIE said he regarded it as a gumma, breaking down fairly extensively, with a good deal of infiltration. The gumma seemed to be at the base of the tongue, close up to the epiglottis. Dr. Potter had informed him that there was great infiltration of the left arytenoid also, but Dr. McKenzie had only succeeded in seeing what he took to be a greatly tumefied and pendent

epiglottis, overhanging the larynx in such a way that he had been unable to get a view of the arytaenoids. With this interpretation of the laryngoscopic picture, however, Dr. Potter had been unable to agree.

Dr. FITZGERALD POWELL said the right arytaenoid appeared to be very large. He asked whether the sputum was examined, and if so whether tubercle bacilli were found. The right arytaenoid after the removal of the mucus could undoubtedly be seen swollen and somewhat pale. Tubercle should be carefully searched for and excluded before making the diagnosis of malignant disease.

The PRESIDENT said the question was whether it was gumma or epithelioma. He understood iodide of potassium had not yet been tried. Dr. Powell's interpretation of the picture was not like his own. He (Dr. Grant) thought the rounded body was the tip of the epiglottis forced back by infiltration and hiding the entrance to the larynx, the apparent cavity being really an ulcerative excavation in the infiltration. He asked whether palpation revealed the induration characteristic of epithelioma.

Dr. LACK said he did not think the diagnosis mattered very much in the case, as if it were malignant it was inoperable. He regarded it as malignant.

Case of Crescentic Subglottic Web or Hyperplasia in a Syphilitic Subject, causing Stenosis of the Anterior Half of the Lumen of the Larynx.

By WILLIAM HILL, M.D.

THIS man was shown at the previous meeting of the Section, Dr. Hill being absent, and although examined by a number of experts, only two were able to make out the condition as indicated in the title (p. 101). It was possible that on that occasion the patient had some catarrhal swelling of the ventricular bands and cords obscuring the subglottic region, though the condition had been easily enough seen the day before the meeting and subsequently. Some Fellows had suggested removal and intubation with Schroetter's tubes. The exhibitor of the case had considerable experience in endolaryngeal operations both by the indirect and direct methods; he doubted whether anyone could satisfactorily excise the formation endolaryngeally, and he had no hesitation in saying it was bound to recur, in spite of intubation, on account of its involving the region of the anterior commissure. He had previously shown (December, 1908) a patient cured of a web by a method which he believed was original. A thyro-fissure was performed, the band removed, and the alae

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kept apart for three weeks by an intralaryngeal splint; this effectually prevented the re-formation of a ledge at the anterior commissure, and he doubted if any other treatment would effect this except perhaps Schroetter's tracheotomy tube fitted with laryngeal dilating flanges; his own apparatus was, however, simpler and, he believed, certain. He proposed to treat this patient by the same method, and he would bring forward the case again after operation.

The PRESIDENT said that on the present occasion it answered much more to the description given last time than it did previously. He then failed to see it because it seemed to be hidden by infiltrated vocal cords. A web in the larynx was a wedge, not a membrane. Members might have expected Dr. Hill to carry out the surgical treatment of it by means of direct laryngoscopy, which he had advocated so vigorously.

Kuhn's Laryngeal Intubation Anæsthetic Apparatus for Administering Chloroform and Sealing the Larynx as a Substitute for Laryngotomy in Sanguinary Operations in the Mouth and Jaws, &c.

By WILLIAM HILL, M.D.

Dr. HILL had intubated the larynx of a patient under cocaine that afternoon with the apparatus; it was introduced by aid of the mirror without the least difficulty, and was well borne after the initial spasm had passed. He had as yet not had an opportunity of employing it at an actual operation. Its uses were described in a leading article in the *Lancet* of January 23 of this year.¹ It was kindly lent to the exhibitor by Messrs. Allen and Hanbury.

¹ *Lancet*, 1909, i, p. 251.

Laryngological Section.

April 2, 1909.

Dr. DUNDAS GRANT, President of the Section, in the Chair.

Two Cases of Retrobulbar Neuritis probably attributable to Sphenoidal Disease.

By J. DUNDAS GRANT, M.D.

Case I.—The patient, a man aged 32, was first seen on March 3 in view of the possibility of a retrobulbar neuritis, which dated from February 17, being due to disease of the posterior sinuses of the nose. The only exceptional feature in the nose was an extreme hypertrophy of the middle turbinated body which intruded into a cavity on the right side of the nasal septum. I removed the anterior part of it. Previous to this no improvement had taken place in his vision, but three days afterwards he began to notice a slight improvement which steadily continued, and within a few days more he could read the upper four lines of the test types. On March 24 he states he was able to read all the lines, and now he only notices a slight want of lustre in coloured objects. How far in this instance the operative interference in the nose was the cause of the improvement it is difficult to say, but all that it can have effected would be a removal of pressure and possibly thereby a relief of venous stasis. In view of the fact that improvement has been so considerable I have not felt justified in carrying out more radical proceedings, although I intended removal of the middle turbinal to increase the accessibility of the sphenoidal sinus. I am informed that recovery is not unusual in such cases apart from surgical treatment.

Case II.—The patient, a man aged 38, was first seen on December 4, 1907, for examination of nose on account of retrobulbar neuritis dating

from the early part of the year, when he first noted a halo round the gas, and in two months was almost blind and unable to read any of the lines of test types. He consulted Mr. Wray in September, and he sent him for examination of the nose. There was hypertrophy of both middle turbinated bodies; the posterior extremities of these were removed to make the sphenoidal and posterior ethmoidal cells accessible. Within two or three months he could read the largest test type. Very slight but steady improvement has continued, and the patient can now read a newspaper without the help of glasses. A feeling of "pressure" disappeared after the operation, and the nose has been freer. The actual complicity of the posterior cells is doubtful.

DISCUSSION.

Dr. DUNDAS GRANT said the first case seemed to be one in which the dependence of optic neuritis on sphenoidal disease was not absolutely decisive. It seemed as if the removal of the turbinals permitted of relief of the stasis, or perhaps of drainage from the posterior cells, and that that played a part in the recovery, although it was known that recovery was not unusual in such cases without surgical treatment.

Dr. SCANES SPICER said this question had been of great interest to many rhinologists for years—namely, the connexion between different forms of nasal disease and symptoms referred to the eye; and though any one rhinologist might not see enough of any one sort of case to make general assertions, many eye symptoms certainly did disappear after the nose had been treated. Ophthalmologists and rhinologists should co-operate in working out these co-existences and sequences. He questioned whether "probably attributable to sphenoidal disease" was applicable to the present cases, because no evidence of sphenoidal disease had been described or was visible. He thought that some congestive conditions of the disc were associated with abnormal pressure between the middle turbinals and the septum. More than once he had observed that relief of that pressure, by removing the redundant part of the septum or reducing the middle turbinals on account of nasal symptoms, had been followed by disappearance of visual symptoms which the patients had previously noticed. He did not at the present moment see anything abnormal in the discs of Dr. Grant's first case, and in the second no drops had been instilled into the eyes, so that he could not get a view of the discs. He would suggest that in these cases the retrobulbar congestion was most probably due to pressure which Dr. Grant had removed; that pressure having either caused collateral venous engorgement or having been a reflex vasomotor engorgement from pressure on the intranasal nerves. He recommended Dr. Watson Williams's very valuable article and

diagrams in this year's *Medical Annual* as an introduction to the subject of these connexions to those who were sceptical about them.

Dr. C. O. HAWTHORNE desired, as one who had no expert knowledge of the diseases of the nasal cavities, to know what was the exact condition in the sphenoidal sinus which the operation removed. The record of the first case seemed to be the natural history of many cases of so-called retrobulbar neuritis quite apart from any intranasal operation, and hence, at least at first sight, the question arose whether the operation here had anything to do with the favourable issue. He thought it would be a great mistake if retrobulbar neuritis, so called, were to be regarded as specially confined to the ophthalmologist on the one hand, or to the rhinologist on the other. Retrobulbar neuritis was a condition which, when it occurred, threw a suspicion on the patient's central nervous system. It might be months or years before further signs appeared, but in a certain number of instances retrobulbar neuritis was the first event in the evolution of disseminated sclerosis. This wider relationship should certainly be borne in mind.

Mr. WESTMACOTT said that a similar case came under his notice last November. The patient was seen by an oculist for sudden blindness in the right eye. The diagnosis was right sphenoidal disease, and an immediate operation was advised. Before anything was done the patient's brother, who had been under Mr. Westmacott's care, asked him to see him. He found the sphenoidal sinuses both clear, but on transillumination there was a shadow in the left maxillary sinus, the side opposite to that of the blindness. That antrum was full of foul pus. He removed a tooth and treated the sinus by an obturator plate, and the patient got well. From the day of operation the sight began to improve, and now it was almost normal; there was simply a misty central portion of the field of vision. He believed the two conditions in the patient were merely a coincidence, that the blindness was a retrobulbar neuritis, probably of central nervous origin, and had nothing to do with accessory sinus disease.

Sir FELIX SEMON asked where the connexion between a right retrobulbar neuritis and a left maxillary sinus disease came in. He did not think that even the most far-fetched theories could establish such a connexion.

Mr. HERBERT TILLEY said he had seen a large number of cases of sphenoidal sinus suppuration, but in none had he seen the optic disc affected, though he had had the discs examined in every case. There was an excellent article on the relationship between diseases of the eye and nose in the *Transactions* of last year's International Congress of Laryngology at Vienna (1908). Kuhnt's paper read at that meeting gave an excellent review of the subject, showing that many of the eye diseases were due to the conveyance of infection from the nose by way of the lachrymal canal.

Mr. CLAYTON FOX asked whether there was any lesion of the posterior ethmoidal cells in that case, as it was stated by modern writers that a posterior ethmoidal cell often came into intimate relation with the inner wall of the optic

foramen, and that being so it would be more easy to conceive that the nerve would suffer from a lesion of this cell, closed and suppurating, than from sphenoidal sinus disease.

Dr. GRANT, in reply, thanked Dr. Hawthorne for his apposite suggestions. No doubt the ophthalmologist would only refer such cases to the rhinologist when there was reasonable ground for suspecting that there was disease in the nose. He referred Dr. Hawthorne to the observations of Onodi upon contralateral optic neuritis in cases of suppuration in the posterior ethmoidal cells. Onodi pointed out¹ that the mechanism in such cases was that the posterior ethmoidal cell of one side sometimes passed even behind and across the sphenoidal sinus of the same side, so as to come nearer to the optic nerve than the sphenoidal sinus; and if it happened that there was very thin bone, or a dehiscence at that spot, contralateral neuritis might be present as the result of suppuration in the ethmoidal cells. If Dr. Westmacott's case was not a mere coincidence, he suggested that the explanation might have been found in simultaneous suppuration in posterior ethmoidal cells on the same side as the antrum, but extending round the sphenoid to the opposite side.

Case of Angeioneurotic Œdema.

By J. DUNDAS GRANT, M.D.

THE patient, a man aged 29, has just come under observation on account of swelling of the face which came on somewhat suddenly about two months ago. It then occupied the whole of the forehead, and there was some loss of sensation in the swollen region. It has spread over the whole of the right side of the face and part of the left, along with the nose. It is reported to have disappeared at one time for five days, and then to have appeared again after a sleepless night. There is also a white vesicle on the right pharyngo-epiglottic fold, but no symptoms seem to have arisen from it. At the time of the onset of the facial swelling the patient was suffering great mental distress due to domestic causes; he has also a tendency to bleed and to bruise very readily, and his gums are pale and spongy. There is no history of swellings or hæmorrhages in any other part of the body. The coagulation time of the blood was three times greater than normal, and the hæmoglobin amounted to 90 per cent. of the normal. There was slight excess of lymphocytes, but otherwise nothing remarkable.

¹ *Journ. of Laryngol.*, Lond., 1907, xxii, p. 382.

DISCUSSION.

Dr. JOBSON HORNE said the statement made by Dr. Grant that the case, of which they had the notes before them, had been prevented from being present at once explained the difficulty he (Dr. Horne) had had in identifying the substituted case as one of angeioneurotic œdema. In response to Dr. Grant's request for Dr. Horne's reasons for not regarding the case exhibited as one of angeioneurotic œdema, Dr. Horne pointed out that the history of the œdema, its localization, and relative permanency since its onset were alone contrary to his¹ own clinical impressions of the disease.

Mr. BEDDOES asked whether the case had been treated for any length of time; whether local antiseptics had been used, and whether attention had been directed to the alimentary system. The tongue was furred, and had teeth-marks upon it. No doubt the condition was œdema, but whether it was angeioneurotic was open to question.

Dr. GRANT replied that the case was to be treated by means of chloride of calcium internally.

Case of Rhinitis Caseosa.

By J. DUNDAS GRANT, M.D.

THE patient, a woman aged 42, complained of great pain and obstruction on the left side of the nose, with slight purulent discharge and a slight degree of boggy swelling over the superior maxilla. On first inspection there was found in the nose a ragged, red, very unhealthy-looking granulating mass which gave an impression that the case was one either of malignant or of tertiary specific disease. She was ordered iodide of potassium, and a portion of the obstructing mass was removed for microscopical examination. She was not seen again for about six weeks, when Dr. Grant observed beyond the red mass a quantity of putty-like material, and recognized the condition as one of rhinitis caseosa. The mass was cleared away by means of Higginson's syringe with a Eustachian catheter, and relief was immediate. The cheesy mass consisted of broken-down pus, and the case illustrates the satisfactory feature that the result of this simple treatment is effective as regards both temporary and permanent benefit, recurrence being scarcely known.

Dr. GRANT, in reply to Dr. Donelan, said the antrum had been punctured, and was found to be free. He proposed to bring the case forward again on another occasion.

¹ *Vide* St. Bartholomew's Hospital Reports, Lond., 1894, xxx, p. 195.

Additional Note on a Case of Malignant Disease in the Left Pyriform Sinus.¹

By Sir FELIX SEMON, K.C.V.O., M.D.

THE patient has just returned from Switzerland in the best of health, with powers of voice and swallowing fully restored. The left vocal cord has begun to resume its normal movements, thus showing that the interference with its movements, whether it was due to mechanical impairment or to lesion of the recurrent laryngeal nerve, must have been of transitory nature. There is no evidence of any recurrence.

Additional Note on a Case of Thyroid Tumour at the Base of the Tongue.²

By W. STUART-LOW, F.R.C.S.

(Now shown after removal, with specimen, microscopic slide, and illustrations.)

UNDER chloroform anaesthesia laryngotomy was performed, and the pharynx being firmly plugged with a captive sponge, the anaesthetic was administered through this tube. The tongue, well protruded from the mouth by means of two deeply-inserted stout silk cords, was split from tip to base, and so the tumour, the size of a small tangerine orange, was effectively exposed. The growth was found to be solid and enveloped in a very firm, thick capsule; it occupied nearly all the region of the base of the tongue, the structures being so stretched and attenuated over it that there was considerable danger of the whole tongue coming away during the extrusion of the mass. Part of the capsule, very deeply situated near the hyoid bone in the vicinity of the thyro-lingual duct, had to be resected, but this was first thoroughly curetted and the surface well rubbed with a 60 gr. to 1 oz. solution of chloride of zinc. The tongue was then stitched up from tip to base with interrupted

¹ Shown at the meetings of the Laryngological Section on November 6, 1908 (p. 6), and February 5, 1909 (p. 78).

² Exhibited at the last meeting (March 5), p. 109.

sutures of chromicized catgut, except in the last $\frac{1}{2}$ in. above and below which was left open for drainage. Rectal feeding was employed for two days, and a spray of peroxide of hydrogen used for the mouth. She made an uninterrupted recovery and left the hospital on the sixth day. The tumour proved to be of a thyroid-adenomatous nature.

A Case of Pharyngo-keratosis of five months' duration in a Man aged 34 ; the Secretion swarming with Diphtheria Bacilli.

By H. J. DAVIS, M.B.

THE case is a very important one, owing to the fact that the patient is head master of a large orphanage in the suburbs, containing 250 children of both sexes, their ages ranging from 6 to 14 years. In November last the patient had an attack of "follicular tonsillitis which has never got well, in spite of active treatment." He was sent to me by his medical adviser in February last. The condition was typical of pharyngo-keratosis. There were white spots on both faucial tonsils, both lingual tonsils, the pharynx, and palate; there were no symptoms beyond slight soreness. The patient is a man of splendid physique; he says he feels perfectly well, but he looks pale. A microscopic examination of the secretion showed the presence of "Klebs-Löffler bacilli" in large numbers, and the pathological report was "diphtheria." The patient was at once isolated, and I suggested injections of antitoxin. He has had five injections, and there is no amelioration in the condition.

The medical officer to the orphanage tells me that during the last four months there have been "outbreaks of tonsillitis among the children," about forty being affected, but though some cases were very suspicious there was no membrane. He sent me swabs from three children—the last affected—and the pathological report was "diphtheria," and they were certified as such. No cases of paralysis have occurred, but since the patient's isolation "the outbreak of sore throats has ceased." The patient himself had diphtheria eighteen years ago.

In view of the now generally accepted theory that no treatment is of any avail in pharyngo-keratosis, the question arises as to what is to be done in such a case as this. The area affected is very large, the lingual and faucial tonsil crypts are plugged with secretion containing diphtheria bacilli in large numbers, and though in a quiescent state in

the patient they are capable of producing diphtheria in others, as evidenced by the outbreak. The patient really has chronic diphtheria. I have suggested diphtheritic vaccine injections, but understand that this is not feasible.

DISCUSSION.

Dr. J. B. BALL said it seemed to him that the patient had pharyngo-keratosis, that an outbreak of sore throat occurred in the school two or three months after that had existed, and that then he probably contracted diphtheria bacilli in his throat. He did not think there was any connexion whatever between the presence of diphtheria bacilli and the pharyngo-keratosis.

Mr. CLAYTON FOX said that, assuming the patient to be the subject of true diphtheritic bacilli, the sooner, for the sake of others, he were rid of them the better. As the bacilli had been present so long the patient was probably immune from diphtheria, so that removal of the keratotic processes (the carriers of the bacilli in this instance) by operative methods would not be attended with danger of infection. Morcellement would be appropriate for the faucial tonsils, and the lingual tonsils might be dealt with by the application of the galvano-cautery.

Dr. HARMER said he had a nurse under his charge for twelve months with a somewhat similar condition. Clinically the case appeared to be pharyngomycosis, but the pathologist reported it to be diphtheria and said that it would be impossible to differentiate it from diphtheria without using the different sugar tests. That was done, and it was found not to be diphtheria. Eventually an organism typical of pharyngo-mycosis was discovered. The patient did not infect other people. After various forms of treatment X-rays were employed, directed through a speculum, and appeared to benefit the case. She was cured at about this time.

Mr. ATWOOD THORNE and Dr. PETERS asked whether guinea-pigs had been injected for confirmation.

Sir FELIX SEMON said there was no doubt in his own mind that the first step to be taken in the case was inoculation of guinea-pigs, as it was necessary to ascertain whether the affection was true diphtheria. He understood that there had not been cases of genuine diphtheria in the school, only tonsillitis. Dr. Harmer had referred to the fact that in an apparently similar case the organism found was the typical one of keratosis; but surely no one would mistake leptothrix for a diphtheria bacillus. If there were not the danger of infection, he would have suggested the treatment which he had found efficient in keratosis—namely, sending the patient to a bracing place and giving a tonic. But if in this case the patient should be found to be a carrier of infection, he should be sent to a place where he could not infect others.

The PRESIDENT (Dr. Dundas Grant) said he thought the pharyngeal tonsil shared in the general condition with the other tonsils. By posterior rhinoscopy they could see several white specks in the pharyngeal tonsil. The tonsil was

enlarged, and there was one focus which had not yet been cleared up. He agreed that morcellement was the proper treatment, and treatment of the lingual tonsil might follow by any of the methods, such as a saturated solution of salicylic acid applied locally. The patient seemed to be rather a carrier than a sufferer from diphtheria. No doubt he had pharyngo-keratosis before he got the bacilli, and the coexistence of the two conditions was accidental.

Mr. HETT said it was easy to remove the lingual tonsil at the same time as the palatine tonsil.

Dr. DAVIS, in reply, said the parts affected were both faucial tonsils, both lingual tonsils, the tongue, palate and post-pharyngeal wall; if the secretion was scraped with a curette it returned. He agreed that inoculating a guinea-pig was the only positive proof of diphtheria, but he did not see how that would do the patient any good. There was no doubt that the patient was infectious and, to his mind, that he had chronic diphtheria. If the keratosis could be cured, he thought there would be no favourable surface for the growth of the bacillus and the affection would subside.

Further History of a Man, aged 40, with Epithelioma of Right Tonsil.¹

By H. J. DAVIS, M.B.

THE operation proved a very extensive one, and was performed by my colleague, Mr. Armour. The external carotid and its main branches were tied, numerous glands removed, the jaw sawn through, and the growth excised. It was found to extend to the post-pharyngeal wall, and after removal it was freely cauterized. The wound in the sub-maxillary region has granulated up. The patient has lost all his pain, but is emaciating. No preliminary tracheotomy was performed.

Further History of a Man, aged 56, with an enormous Sarcoma of Right Palate and Tonsil.²

By H. J. DAVIS, M.B.

THE operation was performed by my colleague, Mr. Armour, and carried out on the same lines as the above. The carotid was tied and glands removed, the jaw sawn through, and the growth—a very large

¹ Exhibited at the December meeting, 1908 (p. 45).

² Exhibited at the February meeting, 1909 (p. 103).

one—shelled out completely. Though the patient left the theatre in a satisfactory condition he died suddenly two hours after. No preliminary tracheotomy was performed.

Case for Diagnosis.

By H. J. DAVIS, M.B.

GIRL, aged 18, attending West London Hospital, and sent to me by Mr. Archibald Smith, with growth on left side of the tongue. No pain, but inconvenience in swallowing and speaking. The growth is of most unusual size, and extends along the left margin of the tongue from the middle of its length backwards, and curving towards its middle in front of the epiglottis and partially obscuring the glottis. The cords are normal. The growth does not infiltrate the tongue, and there are no glands.

DISCUSSION.

Dr. BALL said he thought the case was of the same nature as, though much more extensive than, a case he had shown at the Laryngological Society about nine or ten years ago as a case of "wart growth of the tongue." As soon as Mr. Butlin saw it he expressed the opinion that it was a localized macroglossia, an affection of the lymphatic system of the tongue, and this opinion was generally accepted.

Mr. HERBERT TILLEY said he asked the girl whether there were any lumps on any other part of her body, and she replied that there was one on the outside of her thigh; and even through her dress one could feel a distinct movable mass. He thought there should be a more general examination to ascertain whether any relationship existed between such swellings and the lingual condition.

Mr. STUART-LOW said that by placing one finger at the back and another under the tongue he was able to examine the enlargement thoroughly. It had existed all her life, and was, he considered, a lymphatic enlargement. He would describe it as a cystic hygroma, located in the soft tissue of the tongue. He thought the tongue should be split from tip to base, as in the case of lingual thyroid on which he had just operated and was showing at this meeting, as this method gave excellent access and greatly facilitated the removal of these growths.

Mr. CLAYTON FOX said that most of the swelling was over the foramen cæcum, and possibly its origin might be in the thyro-lingual tract. Probably it was tubulo-dermoid tissue, similar in nature to Mr. Stuart-Low's case. The only point against that was that it had spread laterally.

Mr. ARCHIBALD SMITH said the patient came last Friday to the surgical out-patients' department at West London Hospital, and he recognized that it was a condition such as he had not seen before. He asked Dr. Davis's opinion of it.

The girl's mother declared that she had got it at birth, but the patient could not say how large it was when she first noticed it. She was now aged 18, and had had it ten years. Obviously it was not malignant, and the front part of the growth had the appearance of a papilloma. He was not prepared to say what the remainder of it was. It might be a lymphatic or a lipomatous growth. The question of it being thyroid tissue was, he thought, negatived by it being so much on one side. The patient would be taken into hospital and the tumour removed.

Further Notes on a Case of Primary Tuberculous Granuloma of the Septum in a Woman aged 54.¹

By L. H. PEGLER, M.D.

A REGROWTH of the disease has taken place; on the left side, where formerly there was a lobulated tumour, there is now a small sessile body, the size of a filbert kernel, in front of the small perforation. On the right side the deflected cartilage is much thickened and studded over with tubercles. The perforation passes through the centre of this mass. The general health remains as before. Microscopical sections of primary growth are again shown.

Mr. HERBERT TILLEY desired to point out the frequency with which in tubercular growths of the septum the small cutaneous gland in front of the lower half of the masseter muscle was enlarged. It had been said that lupoid nodules occurring in the cheek arose from primary infection in the nose.

Case of Laryngeal and Buccal Syphilis presenting Unusual Features.

By W. JOBSON HORNE, M.D.

THE patient, a man aged 39, has had sore throat since April, 1908; onset gradual. In November, 1908, mucous plaques were on the lateral parts of the pharynx, buccal cavity, and oral surface of epiglottis. The right vocal cord in posterior third has been affected. Improvement has taken place under mercury and iodide of potassium.

The PRESIDENT (Dr. Dundas Grant) said it would be agreed that the epiglottis was in a most unusual condition. It looked like a mucous plaque of singularly symmetrical form.

¹ Sections were shown at the January meeting (p. 89).

The "Teat" of a Comforter removed from the *Œsophagus* of a Child 4 days old.

By HERBERT TILLEY, F.R.C.S.

THE infant had swallowed the rubber portion of a comforter which was filled with cotton wool. The patient was very restless, and milk swallowed was returned. Under chloroform narcosis the *œsophagus* was examined by Brüning's instrument for direct examination of the lower air passages and *œsophagus*, and the teat was recognized and easily removed by means of a slender pair of Paterson's forceps. The smallest sized bronchoscope was passed into the *œsophagus*. No complication of any kind hindered the immediate restoration of the normal power of swallowing.

A Case of Paralysis of the Right Cord. Right Sternomastoid, Trapezius, Soft Palate and Pharynx (Spinal Accessory).

By FREDERICK SPICER, M.D.

THE patient, Mr. R. R., aged 32, is a traveller. He had always enjoyed good health until about January 14, 1909. He then began to complain of a sore throat and weakness in his voice, with inability to swallow, and pain on right side of head, right shoulder, and right arm. He was treated by his local doctor and a nerve specialist, but getting no better was sent up to London, and when I first saw him, on March 4, he was very much in the same condition as now. There was no history of syphilis, nor injury. Since then he has been treated with iodide of potassium and mercury, with massage and electricity.

DISCUSSION.

Dr. DONELAN said there was a history given of an attack of influenza with persistent cough in November, and he thought the symptoms might be due to influenzal poisoning of the vagus and spinal accessory, of which many cases had been reported in the literature of the past twenty years.

Mr. CLAYTON FOX said he had hoped, for the sake of the explanation, to find that the patient had deafness. But as he had not, it was difficult to find the seat of the lesion. The patient had Schmidt's syndrome. He supposed the lesion must be where the spinal portion of the spinal accessory immediately

came into contiguity with the accessory fibres from the nucleus ambiguus before entering the jugular foramen. He thought probably it was due to thickening from syphilitic meningitis, although there was no history of syphilis.

Sir FELIX SEMON said he was not a Chauvinist, but in the interests of truth as well as of British medicine, he protested against the use of Schmidt's or Avelli's names in connexion with what had been described by Dr. Hughlings Jackson and Sir Morell Mackenzie as early as 1864-67. If any author's name was to be given to the "syndrome," it should be the "Hughlings Jackson-Mackenzie" syndrome.

Dr. F. SPICER replied that several nerve specialists had seen the case and agreed with the diagnosis. Dr. Harry Campbell had reported that it was a "root lesion."

Cases of Lupus.

By H. W. CARSON, F.R.C.S.

Case I.—B. V., female, aged 27; under observation four and a half years. Lupus of epiglottis, larynx, pharynx, uvula, and soft palate. Shown to Laryngological Society, December, 1904; curettage and partial removal of uvula for dysphagia, February 2, 1905; epiglottis amputated, May 25, 1905; in hospital with abscess of lung, June to October, 1905. During the last two years there has been a steady improvement under tuberculin injection, with local applications of lactic acid, and the case is now practically cured.

Case II.—A. F., female, aged 20; duration of disease, four years. Lupus of faucial pillars and uvula; larynx unaffected. Tuberculin injection since February 19, 1908, with lactic acid locally— $\frac{1}{1000}$ mg. every three weeks for seven months, increased to $\frac{1}{750}$ mg. for four months, when condition relapsed; dose reduced to $\frac{1}{1000}$ mg.

Case III.—E. C., female, aged 10; duration of disease, eight years. Lupus of soft palate, pillars of fauces, posterior pharyngeal wall, epiglottis, larynx, and nasal septum. Treated as out-patient, except for short period when she was admitted for tracheotomy, which was not done; treated by tuberculin injection and lactic acid locally. Practically healed now, but owing to scarring there is much deformity and some stridor.

Case IV.—H. A., male, aged 15; duration of disease ten and a half months; father said to be phthisical. In addition to the throat condition the patient has skin lesions, diagnosed as tuberculous by a dermatologist. Has been under treatment for six weeks.

DISCUSSION.

Dr. JOBSON HORNE considered that the first of the cases of lupus exhibited by Mr. Carson presented, in the larynx, a change in the interarytænoid region to which he (Dr. Horne) had drawn attention¹ as very suggestive of the patient being the subject of pulmonary tuberculosis.

Mr. HERBERT TILLEY supported Dr. Horne's suspicion of tubercle because the patient had a characteristic swelling in the interarytænoid fold. He therefore thought the lungs should be examined.

Mr. BARWELL also agreed as to the suspicion of tubercle in the one case. He wished to congratulate Mr. Carson on his results on the whole. He believed they were brought to show the value of tuberculin injections. But in spite of those very satisfactory results one must remember that many cases of lupus of the larynx and pillars of the fauces got well without the injection of tuberculin, and without any special treatment at all. With fresh air and feeding, the use of arsenic internally, and with curetting and lactic acid they were very apt to clear up. One or two of the cases were obviously most extensive, notably No. 17. One could not say absolutely that the cure had been due to tuberculin.

Mr. CARSON, in reply, said the first case was in the hospital with an abscess of the lung, and at that time it was thought to be tuberculous. That was four years ago, and the patient was now apparently in excellent health, with no evidence of anything wrong with her lungs. The condition in the interarytænoid space he knew to have been there three years; it had not altered, but had remained after everything else had cleared up. They were all having tuberculin, but his experience did not enable him to say whether the injection of that altered the Calmette reaction.

Case of Fixation of the Left Cord, with Swelling of the Left Arytænoid Region.

By J. DUNDAS GRANT, M.D., and DAN MCKENZIE, M.D.

THE patient, a woman aged 54, came to hospital eight months ago suffering from hoarseness. The left cord was seen to be fixed in abduction, but no cause could at that time be found to account for the fixation. Two months ago swelling of the left arytænoid region was noticed for the first time, and since then has undergone a steady increase in size. At the time this note is written the arytænoid swelling forms a rounded tumour involving the pharyngeal rather than the

¹ "The Pathogenesis and Earlier Clinical Evidence of Laryngeal Tuberculosis," *Brit. Med. Journ.*, 1898, ii, pp. 1245-7.

laryngeal aspect of the region, so that it projects into and almost entirely obliterates the left hyoid fossa. By practising hypopharyngoscopy one is able to see that the tumour is not strictly confined to the left arytaenoid region, but that, having traversed the middle line, it encroaches a little upon the right arytaenoid region; the surface of the tumour is rounded, and perhaps nodular; the mucous membrane covering it is nowhere broken by ulceration, as far as can be seen on examination both by the direct and indirect methods. No enlarged glands have been discovered, and the patient has never suffered from rheumatism. Syphilis may be excluded by the fact that she has been taking potassium iodide for six months.

DISCUSSION.

Dr. GRANT said he did not think it was definite abductor paralysis. He had seen cases of epithelioma commence in that way, but at the present time this looked innocent enough.

Dr. H. J. DAVIS said he thought the swelling was due to oedema, and he was about to suggest that it was malignant disease. He believed the cord was fixed in the abducted position, as described by Dr. Grant.

Sir FELIX SEMON said that if a cord stood in the *middle line* it was *adducted*. He called *abduction* the position in which the cord stood when fixed by the *side* of the larynx. If it was fixed in "abduction" there was complete aphonia, because, however much the tendency might be for the healthy vocal cord to cross the middle line in complete recurrent paralysis of the other cord, it would never compensate to that degree. The present patient had a normal voice, which alone sufficed to show that the affected cord could not be fixed in "abduction."

Mr. BARWELL said he thought the cord was fixed in the middle line. The swelling at the base of the arytaenoid looked as if it went further down, and without Killian's tube or hypopharyngoscopy one could not be certain of its nature.

Mr. HERBERT TILLEY asked whether the upper part of the œsophagus had been examined by the direct method; if not, he would suggest it might afford valuable information.

Dr. GRANT, in reply, said all would be agreed that it was mechanical fixation, that it was not recurrent paralysis through pressure on the recurrent nerve. He asked whether the opinion was that it was a new growth or an inflammatory condition. Apparently it was not syphilitic, as iodide of potassium had had no effect upon it. It was a question whether it was malignant.

Dr. MCKENZIE replied that the tumour had been examined by the direct method, but he had not been able to see any ulceration. One got beyond the tumour with great ease by direct tubes, and beyond it the mucous membrane seemed normal. With regard to abduction, he saw the cord on the right side

coming over the middle line to the fixed left cord. When she was under his care six or eight months ago there was nothing to be seen of the swelling. Perhaps they would be able to show the case again.

Foreign Body removed from the Œsophagus.

By G. SECCOMBE HETT, F.R.C.S.

THIS was a case under the care of Dr. Bond at the Throat Hospital, Golden Square, to whose kindness I am indebted for permission to publish these notes. On January 4 the patient, a boy aged 8, swallowed a brass purse clip; X-rays showed the foreign body to be impacted behind the cricoid; the foreign body could not be seen with the laryngoscopic mirror. Under chloroform anæsthesia Brüning's tube was passed, and the foreign body was seen, but it was firmly impacted and could not be drawn up; the patient became collapsed, and consequently manipulations were stopped. A second X-ray showed the body to be lodged opposite the eighth and ninth dorsal vertebræ; the child was again anæsthetized, the tube passed, and the foreign body extracted; it was lying with its long axis at right angles to the œsophagus, and the sharp corners held it firmly to the walls of the tube, so that the purse clip had to be turned round before it could be withdrawn. It is, perhaps, noteworthy that the clip was lying in such a position that on examination through the tube it only presented a thin edge; also the mucous membrane of the œsophagus bulged round it. The tube was passed into the stomach, and on slow withdrawal the clip could be clearly seen, although hidden by mucous membrane when the tube was passed downwards.

Primary Intra-laryngeal Endothelioma.

By WILLIAM HILL, M.D.

EXCISED larynx, removed by Solis-Cohen's method of total laryngectomy, from a female who had primary "party-wall cancer" of the larynx (*i.e.*, truly *intrinsic* in origin) which spread whilst under observation from the interarytænoid region to the left vocal cord anteriorly, and became *extrinsic* by invading the contiguous pharyngeal mucosa posteriorly. As the operation was performed so recently as March 17 the patient was not sufficiently recovered to be shown.

Laryngological Section.

May 7, 1909.

Dr. DUNDAS GRANT, President of the Section, in the Chair.

Case of Fixation of Vocal Cord. ? Aneurysm.

By J. DUNDAS GRANT, M.D.

THE patient, a man, aged 42, was first seen on April 21, 1909, and complained of loss of voice of ten weeks' duration, which developed in twenty-four hours. The left vocal cord was found to be fixed in the middle line and its edge quite concave. The left pulse was extremely feeble as compared with the right, and tracheal tugging could be elicited. The apex-beat was displaced downwards to the left.

Dr. IRONSIDE BRUCE reported that the X-ray examination of the thorax in the antero-posterior direction showed marked increase of the median opacity of the chest towards the left; this increase of opacity was abrupt and rounded in outline. In the antero-lateral right direction examination showed marked bulging of the arch both in the anterior and posterior direction. From the X-ray point of view, therefore, this case was one of aneurysm of the transverse and descending aorta.

Case of Epithelioma of Epiglottis and Vestibule of Larynx.

By J. DUNDAS GRANT, M.D.

THE patient, a man, aged 63, was first seen on April 14, and complained of sore throat of four months' duration. The right two-thirds of the epiglottis was occupied by a thick, pale, papillated swelling which

turned backwards over the vestibule of the larynx in the form of a horn and blended with a swelling of the aryepiglottic fold. Iodide of potassium was given for a week without result. The epiglottic swelling was extremely hard to the touch. There were no enlarged glands. The prominent portion of the epiglottis was then removed by means of a snare for microscopical examination. The stridor which was present was not relieved, the aperture of the larynx being still almost occluded. The microscopical examination revealed typical epithelioma extending into the cartilage. Tracheotomy has been performed, and Dr. Grant thought there would be probably agreement that a radical operation was out of the question.

Case of Left Bronchocele in a Woman aged 40.

By H. J. DAVIS, M.B.

THE patient had only been seen once (April 27). She was hoarse, occasionally aphonic, and had dyspnoea. The larynx was displaced and distorted; the narrowed lumen of the trachea was plainly visible just below the cords. The left cord was thickened. The right was hardly visible owing to the displacement of the larynx to the left.

Opinions were requested as to the question of removal of the tumour. The laryngoscopic appearance was peculiar.

DISCUSSION.

Mr. HERBERT TILLEY suggested that the bronchocele should be removed, as it would be easy to do so. The patient told him she sometimes woke up in the middle of the night with great difficulty of breathing, and this symptom seemed to demand active interference.

Dr. DONELAN asked why it was called a bronchocele and not an adenoma of the thyroid body. He agreed that it could be easily removed. As the internal condition was now aseptic, he thought that there was no necessity to drain and that the wound would heal up by first intention.

Mr. STUART-LOW said that on giving the patient water to drink in the usual way, and placing the fingers lightly on the tumour, there was no elevation detected, only a heaving outwards. This was an anomalous condition, and he doubted whether it was a thyroid cyst; it might be a branchial cyst. These often came up very quickly, as in this case. He advised operation without delay.

Dr. FITZGERALD POWELL said he thought the tumour distinctly moved upwards on swallowing, and he had no doubt that it was a cystic adenoma of the left lobe of the thyroid. No doubt the right treatment was excision by a transverse incision. In all his late cases of excision of the thyroid gland and its tumours he had found great advantage from making a small counter-opening below the transverse incision. It allowed the long transverse incision to heal by first intention and any oozing of blood to drain away from the deep cavity that was left, and also obviated the absorption of any thyroid secretion.

Mr. ROUGHTON said he thought it was clear that it was a cyst of the thyroid body. He thought it was important to wait until the pustular eruption had disappeared. He did not think there was any necessity to drain for more than twenty-four hours. Union by first intention was nearly always obtained in such cases.

Dr. H. J. DAVIS, in reply, said he had only seen the patient once, and he thought it was a cystic adenoma. It had been treated by an ointment consisting of equal parts ung. hydrarg. biniodidi and ung. pot. iodidi, and it was this which had produced the pustular eruption. He would advise the patient to have the tumour removed.

**Case of Chronic Empyema of the Antrum in a Woman aged 68,
now Malignant (Fibro-myxo-sarcoma).**

By H. J. DAVIS, M.B.

I was asked to see this patient last March. She had had many polypi removed from the left nostril for nine months with little benefit. Transillumination pointed to empyema of left antrum. This was punctured and found to be full of pus. The patient was admitted into the West London Hospital, and the radical antrum operation performed. The antrum was a fine shell of bone full of pus and polypi, but in one part the granulations removed looked very suspicious of malignant disease. This was, however, negatived by microscopic examination.

The patient did not get well, and slight proptosis and bulging of the cheek became apparent. A week ago the antrum was again opened and scraped, and the tissue examined was reported to be a "fibro-myxo-sarcoma." The palate and post-nasal space are not involved, and it is proposed to remove the left superior maxilla. The patient is frail, but general health is good.

DISCUSSION.

Mr. BARWELL said he gathered from the statement that it was chronic supuration of the antrum which had become malignant as a result of chronic irritation. That was such an important point that he thought it required to be supported by stronger evidence than was at present forthcoming. In the notes of the case it was only stated that she had suffered from polypi for nine months. Another point was that if it were removed, as it probably could well be, by excision of the upper jaw, there must be a thorough clearance of the glands of the neck on that side, which were already enlarged.

Dr. DAVIS, in reply, said he thought from the history that it was an ordinary case of empyema of the antrum, which went unrecognized for six or seven years and had now become malignant. She had polypi removed from her nose for a long time. There were mucous polypi there now. In an ordinary case of malignant disease of the upper jaw he did not think such a condition would be found.

Case of Laryngeal Growth.

By P. R. W. DE SANTI, F.R.C.S.

THE patient is a woman, aged between 40 and 50, with a history of hoarseness followed by variability, and finally loss of voice, of some two years' duration. There has been no pain or breathing trouble until quite lately. Examination reveals a tongue-like, movable soft growth apparently growing from the anterior commissure of the larynx, and extending forwards between the two vocal cords. It moves up and down on respiration. A piece has been removed by endolaryngeal forceps, and microscopically pronounced to be an "angioma." In my opinion the growth is a soft fibroma.

Mr. HORSFORD said the note stated that it appeared to be growing from the anterior commissure of the larynx; but he thought it was growing from the middle of the right vocal cord. He thought it was easily removable by a small snare.

Laryngeal Case for Diagnosis.

By P. R. W. DE SANTI, F.R.C.S.

MALE, aged 54, complains of sore throat, husky voice, pain on swallowing and speaking, and some difficulty of breathing of four months'

duration. He has always been a healthy man; there is no history of syphilis or any signs of that disease, and the lungs are healthy. Examination of the larynx reveals considerable bilateral œdema and swelling of the ventricular bands, the latter being in apposition anteriorly for about half their length, and the breathing space being confined to the posterior half. There is also œdematous swelling, especially on the right side posteriorly extending towards the cricoid plate. There seems to be some ulceration of the right half of the larynx posteriorly, and immobility on that side; some glandular enlargement on the right side of the neck. The case seems to me to be of a malignant nature, and opinions are invited as to diagnosis and treatment.

DISCUSSION.

Dr. JOBSON HORNE said he could see no reason for regarding the condition as malignant disease of the larynx, though, as to what the condition was due to, it was difficult to offer an opinion without further details about the man's general health. It might be entirely due to local causes.

Sir FELIX SEMON said he saw no evidence of malignancy. The anterior parts of the vocal cords did not separate from one another during inspiration. This was possibly due to the general infiltration.

Laryngeal Tuberculosis in a Man aged 43.

By JAMES DONELAN, M.B.

THE patient was first seen fifteen months ago. The epiglottis was a mass of tuberculous infiltration and ulceration. The patient declined amputation. Treatment by rest, open air, lactic acid and guaiacol locally and internally had produced considerable improvement. In each lung there was a small focus which had been quiescent for a long time. The opinion of the meeting was desired as to whether this case was likely to receive further benefit by treatment by tuberculin T.R.

DISCUSSION.

The PRESIDENT (Dr. Dundas Grant) said it seemed to him to be getting well under Dr. Donelan's treatment. Otherwise, he would advise galvanocautic puncture, which could easily be applied.

Mr. BARWELL said it seemed to be one of the cases, of which he had seen a certain number of examples, of chronic, rather mild, non-malignant tuberculous laryngitis, which was almost on the border-line between lupus and tuberculosis of the larynx, affecting the epiglottis most extensively, and causing much less pain than the more acute forms of tuberculosis. It now seemed to be getting better. He was more inclined to use galvano-caustic puncture for that type of case than any other form of treatment.

Mr. TILLEY suggested that the free, diseased portion of the epiglottis should be amputated. It did not seem to be a healthy condition of things to leave there, and it would require many punctures with the galvano-cautery to make it heal and cicatrize, whereas in two sittings practically all of it could be removed and there should be very fair healing.

Dr. DONELAN, in reply, said that where the amount of tubercular tissue was limited, the question of the use of tuberculin arose. He had at first proposed to amputate the epiglottis, but he could not get the patient's consent. He would try again if he were convinced the active lower edge had not advanced too low. In that case he should prefer to use the cautery.

Case of Recurrent Occlusion of Naris.

By JAMES DONELAN, M.B.

THE patient, a man, aged 26, had complete occlusion of the left nasal cavity from deflected septum, large bony spur and adhesions. These were removed and a fair passage formed, which, however, closed again. A second operation consisted in detaching the base of the bony and cartilaginous parts of the middle third of the septum by means of a special chisel, without disturbing the mucous membrane of the right side, and removal of all bony and synechial obstructions. The septum was maintained in the new position during healing. Though complete freedom was obtained on this occasion, synechiæ had again almost completed the occlusion. Suggestions as to further treatment were requested.

DISCUSSION.

Dr. H. J. DAVIS said he thought the obstruction was due to bowing of the septum. He did not suppose the patient had ever had a perfectly clear passage.

Mr. HERBERT TILLEY suggested that the adhesions towards the lower part of the septum on the left side should be divided and an ordinary submucous

resection done on that side, going in on the right side, not on the left. The operator could dissect off that mucous membrane and get plenty of room, and then he did not think it would recur in its present condition.

Mr. HORSFORD asked whether Dr. Donelan had ever tried keeping the parts separate by such a simple device as a celluloid plate. He had found it to be very effective in such cases.

The PRESIDENT said he agreed with what Mr. Tilley said about submucous resection. He did not find patients tolerant of even such simple devices in the nose as celluloid plates, and he avoided them.

Dr. DONELAN, in reply, said a resection at first was impossible owing to the amount of bony obstruction that had to be cleared away. Though the patient was very plucky, even while the saw was being used, a general anæsthetic had to be given, and the passage was cleared so that the operator's little finger, passed through the nostril, met the index of the other hand at the choana. So large a raw surface was left that submucous resection was then also impossible. For that reason he had moved the middle of the septum and used a splint, but as soon as the latter was removed cicatricial contraction closed the passage. He thought Mr. Tilley's suggestion a good one, and would endeavour to carry it out.

Chronic Suppuration in the Left Sphenoidal Sinus ; Recovery.

By HERBERT TILLEY, F.R.C.S.

F. L., a WOMAN, aged 27, first consulted me early in 1903 for chronic nasal catarrh and headache. The headaches were frequent and severe, and chiefly fell on the left occipital region and rather to the left of the vertex. Nasal examination showed an appearance somewhat similar to unilateral atrophic rhinitis, except that the crusts which collected in the upper and posterior region of the nose did not possess the characteristic smell. Irrigation of the sphenoidal sinus caused pain over the occiput, and pus could be blown out of the sinus when a cannula was inserted.

May, 1903 : I opened the sinus, removing as much of the anterior wall as possible ; the middle turbinal was also removed. The patient obtained great relief from her symptoms and disappeared from my clinic. She returned again last autumn with a recurrence of her old symptoms, in addition to much mental depression. I readmitted her into the hospital, and again opened up the sinus, which was filled with pus. The

margins of the old opening were freely cut away, as also were the neighbouring posterior ethmoidal cells. Since the operation the margins of the opening into the sinus have been frequently cauterized with the galvano-cautery and strong nitrate of silver (100 grs. ad 3i) and for the past month there has been no tendency to cicatricial closure of the opening. Her symptoms are greatly but not entirely relieved—that is to say, she occasionally suffers from headaches, but of greatly diminished intensity, and the nasal discharge has practically ceased.

DISCUSSION.

Dr. WATSON WILLIAMS said in many such cases, even after free removal of the anterior wall of the sinus, the great trouble proved to be the tendency to gradual contraction of the opening from cicatrization, and his experience had been that if one also removed the anterior portion of the floor of the sinus it tended to overcome the difficulty more than anything else.

Mr. STUART-LOW said he observed that there was a considerable amount of pus on the posterior wall and on the septum; and he doubted if the opening was in the sphenoidal sinus at all; it was too low down and too far forward. He considered that the opening had been bored into the remains of the ethmoid and not into the sphenoidal sinus. He took exception to the term "recovery" being applied to cases where purulent discharge was still visibly going on, as in this instance.

Dr. JOBSON HORNE said he was inclined to the same opinion as the previous speaker. The opening referred to did not appear to him (Dr. Jobson Horne) to be that of the sphenoidal sinus, although a probe passed into it might eventually lead into that sinus. As regards the use of the word "recovery" in the title of this case, he (Dr. Horne), speaking quite generally, had often been reminded by the exhibited results of treatment of accessory sinus disease, other than that of the antrum of Highmore, of the Irishman's definition of the *grippe* as "a disease from which one suffered for six months after one was cured."

Mr. CHICHELE NOURSE said it seemed perfectly clear to him that the cavity was the sphenoidal sinus. Certainly the aperture was rather low down, but it was in the anterior part of the body of the sphenoid bone. Pus appeared to be coming from the opening. In dealing with such cases he had found it very convenient to insert a small captive tampon, with silk attached, using equal parts of glycerine and glycerine of carbolic acid. That not only kept the aperture into the sinus open, but it tended to cause a more healthy condition of the lining membrane. The tampon was changed every two or three days.

Mr. ROUGHTON said he suggested to Mr. Tilley that he should have a skiagram taken with the probe passed into the opening. The opening seemed

to be in exactly the same position as one which was under his care, in which the skiagram showed that the probe passed into the sphenoidal sinus.

Dr. SCANES SPICER said he thought it was impossible at the present moment to say whether the opening seen led into the sphenoidal or not, since that sinus varied so much in size and the whole anatomy of the parts had been so modified by the thorough operation. The result was excellent on account of the great relief which had been given, and in spite of some exudation and crusting which nearly always persisted.

The PRESIDENT (Dr. Dundas Grant) considered it a very good result indeed. Those who had done several of those operations knew that the resultant aperture was not in the same position as the normal aperture of the sphenoidal sinus, but was lower down and external to where the normal aperture was. That was because the new tissue formed there contracted in such a way that it left an opening in a little different position.

Mr. HERBERT TILLEY, in reply, said that he had the advantage over his critics in that he had operated on the patient himself and had examined her many times. He performed the first operation on the sphenoidal sinus four years ago (removal of the middle turbinal and anterior wall of the sinus), and this gave her considerable relief from the headaches and purulent nasal discharge. She then ceased attending his clinic, and only returned last autumn, when she complained of severe occipital headaches, nasal discharge, and much mental depression; these were so severe as to entirely unfit her for domestic service. She was readmitted into University College Hospital and the sinus freely reopened, as well as the adjoining posterior ethmoidal cells. The apparent low situation of the sinus aperture was due to two causes: (1) The anterior wall of the sinus was a very small one; (2) in order to make a larger opening than in the previous operation he had chiselled away a considerable portion of the solid floor of the sinus by means of a hammer and long chisel. The opening at the end of the operation would have easily admitted an ordinary lead pencil, and at a future meeting he hoped to bring a skiagram showing a probe passed through the artificial opening into the sinus cavity. Had Mr. Stuart-Low used the probe with which the patient was provided, he would have found that it was possible to pass it backwards about $1\frac{1}{2}$ in. beyond the sinus opening. The latter had remained at its present size about four weeks, and it had only been touched that afternoon with a wool-covered probe moistened with cocaine. What had been described as pus consisted only of crusts of mucus which had dried over and upon those cicatrices which were necessarily produced when large areas of the ethmoidal regions had been removed. Her former condition was one of utter misery, and if the adverse critics had seen her condition then and compared it with the present one, he did not think they would have raised the minor points which had been brought forward.

**Case of Post-cricoid Epithelioma in a Woman aged 30 ;
Removal by Laryngectomy.**

By E. B. WAGGETT, M.B.

THE case presents the following points of interest :—

(1) It is an instance of comparatively early diagnosis of post-cricoid epithelioma, very commonly seen in women between the ages of 30 and 40, and of which the exhibitor has never seen an instance in men of the same age.

(2) The exact limits of the disease were made out by the direct method before operation.

(3) As the arytenoids were, as usual, œdematous and probably involved, the larynx was removed and the truncated trachea stitched to a buttonhole on the suprasternal notch, with a view to the immediate as well as to the subsequent safety of the patient.

(4) After a sufficient margin of healthy mucous membrane had been removed, what remained of the hypopharynx and upper section of the œsophagus formed a ribbon, which, in its retracted condition, appeared to be about 10 mm. in breadth. This was, with some difficulty, stitched round a drain tube of 6 mm. diameter, passed through the nose. Since the removal of the tube, some four weeks after the operation, the patient can swallow bread with comparative ease.

The œsophagus was rendered watertight by the suturing of muscular and connective tissue about its anterior aspect, and rapid healing took place, without septic phenomena.

The case is shown at this early date after operation (the patient does not live in England) as an additional instance in which an otherwise rapidly fatal form of malignant disease has proved amenable to operation, thanks to the age and recuperative powers of the patient.

DISCUSSION.

Sir FELIX SEMON congratulated Mr. Waggett heartily on the result, but he took exception to the phrase post-cricoid epithelioma "very commonly" seen in women between the ages of 30 and 40. He knew that if malignant disease occurred in women at all in these parts that was the seat of preference, but he did not regard cancer in that region in women as at all common. In his

thirty-three years' experience he did not think he had seen more than about twenty cases of the kind, though he had seen a good deal of cancer of the larynx.

Mr. WAGGETT, in reply, said he did not suggest that the condition was as common as enlarged tonsils, but he believed that every year he had to ask his surgical colleagues to perform four or five gastrostomies on young women with cancer in the post-cricoid region; in his experience it was a common condition as cancer went, and it was entirely confined to hospital practice; he had never seen a case in private practice, and the difference in Sir Felix Semon's experience might possibly be due to his not now seeing hospital patients, and to the fact that now very many more throats were examined throughout London than formerly. He had not himself seen a case in a young man. The patient owed her life to having received, at the instigation of his colleague (Mr. French), an injection of adrenalin with saline into her veins at the end of a trying operation. She was formerly in very bad health, weighing a stone less than at present.

Abnormality in Right Tonsillar Region.

By NORMAN PATTERSON, F.R.C.S.

THE patient, aged 45, has projecting downwards, forwards, and inwards from the right tonsil a pointed process. It can be traced upwards into the substance of the tonsil and appears to extend towards the point of attachment of the normal styloid process. The free portion is evidently cartilaginous, but the part situated in the tonsil appears to be bony in structure.

DISCUSSION.

Dr. H. J. DAVIS said he thought it was a piece of bone connected with the hyoid. He put his finger into the part, and he thought it was ossification of the stylo-hyoid ligament.

Dr. FITZGERALD POWELL said this was a very interesting case; it appeared to him to be a congenital elongation of the styloid process. From the fact that some slight movement could be obtained on pressing the point he thought it possible that it might be partly cartilaginous. He thought it was congenital and not traumatic—the latter was most unlikely. He suggested removing a portion of the protrusion.

Dr. LAW said the patient stated that she had suffered from fits, and therefore the part might have been injured without her knowledge of the accident.

Mr. WAGGETT pointed out that the skiagram showed the elongated condition of the styloid process to be bilateral, and therefore it could scarcely be due to traumatism.

THE PRESIDENT (Dr. Dundas Grant) looked upon it as an ossification of the stylo-hyoid ligament, and said that its oblique position probably resulted from a blow, as normally it ran almost vertically downwards.

Mr. PATTERSON, in reply, said the stylo-hyoid ligament was joined to the small cornu of the hyoid and not to the great cornu. One of his objects in showing the case was to get the opinion of the Section as to whether it was traumatic or congenital.

Case of Retrobulbar Neuritis from Purulent Disease of the Sphenoidal Sinus cured by Drainage of the Sinus.

By J. DUNDAS GRANT, M.D., and DAN MCKENZIE, M.D.

THE patient, a woman, aged 29, first attended hospital five years ago with nasal polypi. After three or four curettings of the ethmoidal region, at considerable intervals, recurrence ceased, and, although complaint was made of discharge down the back of the throat, the patient seemed to be quite well. A few weeks after the last curetting some interference with the vision of the left eye was noticed, but the patient did not seek advice on that account until five months later (November, 1908), when she went to the Royal Eye Hospital (Moorfields). Here she came under the care of Mr. Claude Worth, who diagnosed retrobulbar neuritis, probably caused by the nasal suppuration. In consequence of this opinion the left sphenoidal sinus was opened up in order to provide free drainage. Its mucous membrane was felt to be thickened and velvety. Rapid improvement in the vision of the left eye followed the operation, and the vision is now normal.

THE PRESIDENT (Dr. Dundas Grant) said the visible orifice was in the new tissue which was formed in front of the sphenoidal sinus, and did not correspond exactly with the position of the original ostium of the sinus. The neuritis was probably due to traumatism during curetting of the ethmoidal cells, and the relief following the opening of the sphenoidal sinus was the result of the simultaneous opening of the posterior ethmoidal cells as formulated by Hajek. In support of his contentions the President, in reply, referred to the description of retrobulbar neuritis as given in Fuchs's "Text-book of Ophthalmology" (translated by Duane, 1908). It could be unilateral and even contralateral, as shown by Onodi, according to the anatomical variations of the posterior ethmoidal cells.

Laryngeal Growth in a Man aged 48.

By W. H. KELSON, M.D.

SLIGHT hoarseness was first noticed six months ago, with pain in the throat. The pain now shoots up to the right ear. On examination a warty-looking growth is visible, involving the inner surface of the epiglottis, right arytenoid and ventricular band, and obscuring view of right ventricular cord. It also extends on to the pharyngeal wall; great loss of movement on right side of larynx. Enlarged glands may be felt in neck. History of syphilis twenty years ago, but potassium iodide in 15-gr. doses has produced no improvement. Chest, *nil*. No tubercle bacilli found in sputa.

Infiltration of the Vestibule of the Larynx, with Intralaryngeal Fungation.

By J. DUNDAS GRANT, M.D.

THE patient, a woman, aged 53, was first seen on April 27, 1909. She had suffered from cough and aphonia for eleven months. On examination of the chest evidences of bronchitis and emphysema were found. She had been a widow for eighteen years, and before that was married for five years. Her husband had died of consumption, her father of cancer of the throat. She had no children or miscarriages. By the laryngoscope there was to be seen pale infiltration of the aryepiglottic folds which were quite immobile; there was also infiltration of the ventricular bands, and on the middle of the right vocal cord there was an extremely pale oval swelling with a rough surface and apparently of soft, almost fluffy, consistency. On external examination the anterior margin of the thyroid cartilage was twisted to the right, and the left ala was almost completely covered by a hard sessile swelling immovable with it, but separated from it in front by a slightly perceptible groove. There were no enlarged glands and no difficulty in swallowing. The induration suggested the probability of a malignant growth and the oval swelling on the vocal cord was removed by means of forceps. It was found to be extremely soft, and some tissue of a similar nature was removed

from the region below the right cord. On microscopical examination it was found to be of a granulomatous nature without the slightest evidence of epithelioma. The patient was accordingly being treated with mercury and iodide of potassium, and, so far as a week's treatment would indicate, she seemed to have improved. The case was probably one of tertiary specific infiltration.

The exhibitor brought before the Laryngological Society on February 6, 1903, a case of chronic œdema of the larynx of twelve months' duration, which he thought answered to the description of amyloid disease, but in February, 1904, he brought her forward again as further developments had indicated that the disease was really a tertiary specific infiltration which ultimately disappeared almost completely under treatment with iodide of potassium, perchloride of mercury, and mercurial inunctions.

DISCUSSION.

Mr. BARWELL asked if Dr. Grant would show the case again after anti-syphilitic treatment had been tried. It was neither malignant nor tuberculous, and he was inclined to believe that it was not even syphilitic, but rather one of those curious cases of simple hypertrophic inflammation.

The PRESIDENT (Dr. Dundas Grant), in reply, said he would be glad to bring the case forward again.

Epithelioma of Larynx.

By W. STUART-LOW, F.R.C.S.

A MAN, aged 45, a porter in the General Post Office, came to the Clinic at the Central London Throat, Nose and Ear Hospital, complaining of hoarseness and difficulty of swallowing of two months' duration. There was a history of repeated attacks of influenza, but not of syphilis. A large, grey, mushroom-like mass was seen covering over and projecting into the larynx. About one-third of this mass was removed with forceps, giving him great relief.

DISCUSSION.

Mr. HERBERT TILLEY agreed as to its malignant nature and thought it was too extensive to be operated upon. He did not know whether Mr. Stuart-Low had made a digital examination of the growth. There were many enlarged glands in the neck.

• Mr. STUART-LOW, in reply, said he had examined it with the finger and found it was quite soft on the surface, though firm in the deeper parts. He took away one-third of the mass with forceps, but there was still much left. Dr. Wingrave reported that it was certainly a rapidly-growing epithelioma.

**Loss of Substance in the Palate and Scarred Pharynx following
Scarlet Fever.**

By WILLIAM HILL, M.D.

THE patient, a girl, aged 7, had had scarlet fever when 2 years old.

Case of Tuberculous Ulcer of the Tonsil.

By H. LAMBERT LACK, M.D.

THE patient, a man, aged 40, is suffering from phthisis, and tubercle bacilli can be obtained in the sputum. For eight weeks he has suffered from a sore throat. On examination, a single, superficial, extensive ulcer can be seen on the left tonsil. The base of the ulcer is sloughy. Iodide of potassium given for a fortnight has had no effect. The case seems unusual, and, in the absence of tubercular mischief elsewhere, might be mistaken for tertiary syphilis.

Case of (?) Malignant Growth of the Right Vocal Cord.

By C. A. PARKER, F.R.C.S.Ed.

THE patient, a man, aged 60, complained of gradually increasing hoarseness for one year. He was otherwise in good health, and there was no history of syphilis or tubercle. On examination, the whole of the right cord was found to be occupied by a feathery-looking growth of a peculiarly white appearance; there was apparently no very deep infiltration, and the cord moved well. In spite of this the appearances of the growth were very suggestive of malignancy.

Fixation of the Right Vocal Cord of Intermittent Occurrence.

By W. JOBSON HORNE, M.D.

THE patient, a man, aged 40, sought advice on account of pain in the right side of the throat and occasional loss of voice. The onset was comparatively sudden, in November, 1908. One night he went to bed with nothing amiss with his voice and awoke in the morning almost voiceless; he had been husky some two or three days previously. Examination of the larynx in March, 1909, revealed fixation of the right cord and free movement of the left; apart from this there was nothing abnormal to be seen in the larynx. The right cord recovered its mobility, and Dr. Horne regretted that he had not had the opportunity of obtaining an exact analysis of the thorax. He would exhibit the case on an occasion of an intermission.

Laryngological Section.

June 4, 1909.

Dr. DUNDAS GRANT, President of the Section, in the Chair.

Subsequent Report on a Case of Infiltration of the Vestibule of the Larynx with Intra-laryngeal Fungation.¹

By J. DUNDAS GRANT, M.D.

SINCE the last meeting this patient has continuously taken iodide of potassium and biniodide of mercury. She has no sore throat, stridor, or difficulty in swallowing. The voice is still reduced to a whisper. There is no return of the neoplastic growths in the larynx; but, on the contrary, there has been a retrogression of the granulation formation below the right vocal cord, and a slight increase of mobility of the ary-epiglottic folds.

Dr. DUNDAS GRANT, in answer to Dr. W. Hill, said he thought there was a syphilitic basis and that it was a gummatous infiltration. It seemed to be improving under antisiphilitic treatment.

Case of Radical Operation for Frontal Sinus Suppuration, Killian's method, with exceptionally Rapid Healing, but with Persistence of Supra-orbital Pain, in a Young Woman.

By J. DUNDAS GRANT, M.D.

THE patient, aged 17, was first seen in January, 1908, complaining of purulent discharge from the nose of eighteen months' duration, and of pain over the left frontal and maxillary region. Her frontal sinus

¹ Shown at the meeting on May 7 (see p. 149).

had already been opened, but the symptoms persisted, and in April the exhibitor performed Killian's radical operation in its complete form. The original infundibulum was already obliterated, but a new passage was made and the ethmoidal cells were well curetted. A plug of gauze was placed in the inner part of the sinus and carried down to the external nostril. The antrum was explored through the nose, but the wall was found to be greatly thickened; it was not, therefore, opened in the canine fossa. The wound was closed with horsehair and silk sutures. Healing took place with exceptional rapidity and the patient returned home in a week. Since the operation she had complained of severe pain in the left supra-orbital region, but there was no evidence of return of frontal sinus suppuration, and it was thought probable that the pain was due to a neuritis of the supra-orbital nerve. The pain varied in its intensity and was sometimes very marked in the infra-orbital region and lateral part of the right orbit; under these circumstances it was doubtful whether an exposure and extraction of the left supra-orbital nerve was likely to be of benefit. She has been treated with quinine, and latterly with gelsemium and with croton chloral, also bromide of potassium with valerian. The exhibitor would be glad to hear the experiences of members with regard to such a course of events.

DISCUSSION.

Mr. HERBERT TILLEY said that six weeks ago he had an identical case. He did not perform the radical operation on the frontal sinus, after the healing of which the patient complained of most severe neuralgia, which incapacitated her from the duties of her ordinary life. Faradism, phenacetin, aspirin, &c., were tried, but she obtained no relief. As nothing further could be suggested she was sent to the seaside, but the neuralgia continued. He saw the case with the surgeon, and it was thought advisable to remove some of the ethmoidal cells and polypi which were still present. This seemed to be the only thing which promised relief. Mr. Tilley operated, and had that day received a letter saying that she had had no pain since the operation, which was performed six weeks ago. All that was done was to remove the polypoid tissue with the adjacent cells in the anterior ethmoidal region. He suggested that in the President's case there was another feature which might account for the neuralgia—namely, a very firm adhesion between the middle turbinal and the septum, and he thought that this adhesion might be divided with advantage, and the anterior group of ethmoidal cells, with the accompanying polypoid tissue, should be curetted at the same time.

Dr. SCANES SPICER said the adhesion on one side and the hypertrophic swellings of the ethmoid on the other were the things he would always attack first, before opening a frontal sinus, for chronic empyema. He had been early impressed by Dr. Woakes's recognition of the cardinal importance of the ethmoid region, and how greatly nasal problems centred around it, and his whole experience led him to agree. He thought suppurating frontal sinuses should be approached almost invariably by reducing as much as possible any disease of middle turbinals, and that then it would be rarely necessary to do an external operation on the frontal sinus.

Dr. H. J. DAVIS said that in many cases of pain following frontal sinus operation, a simple way of affording relief was to give Easton's syrup. In sciatica, one of the things which he had found to give most relief was that syrup, given three times a day, and that applied to neuritis or neuralgia from any cause. A month ago he saw a case of double frontal sinus operation; the result was excellent, for the patient had no discharge, and nothing the matter with the nose, but there was acute pain over the forehead. He gave her 1 dr. of Easton's syrup three times a day and she was now all right.

Dr. DONELAN asked whether the pain had recurred in the same situation, or was it further out? He operated upon a case five years ago—an ordinary frontal sinus operation—and the patient had recurring pain further out over the brow. There was a complete septum dividing the sinus operated on from a secondary sinus further out, with a separate passage to the nose behind the first one. This sinus was purulent and was the cause of the continued pain; it was cured by throwing the two cavities into one in their whole extent. He agreed with Dr. Spicer that a good thing in all cases of post-operative frontal sinus trouble was to clear out the ethmoidal region before proceeding to reopen the frontal sinus, as the latter measure was thereby often obviated.

Dr. GRANT, in reply, said the pain was not merely in the supra-orbital region, but also in the infra-orbital, and the supra-orbital on the opposite side. The anterior part of the middle turbinal was removed in the first instance. When the patient first came under his care she had been operated upon elsewhere, and the infundibulum had been completely obliterated. He did not think there was any retention in the meantime. He thought the pain was neuralgic, and Dr. Davis's suggestion appealed to him; perhaps the several operations had disturbed the patient's nervous system; he had been treating her with quinine, gelsemium, croton chloral, bromide of potassium, valerian, and iodide of potassium. When he opened the left antrum through the nose there was tremendous thickening of the facial wall of the antrum. He would bring the case forward again.

Case of Frontal Sinus Suppuration of Short Duration (five months) in a Young Man ; modified Operation unsatisfactory ; Killian's radical Operation. Rapid recovery.

By J. DUNDAS GRANT, M.D.

THE patient, a man aged 29, was first seen in December, 1908, on account of symptoms of suppuration in the left maxillary antrum. An alveolar opening was made and he syringed it out with borax and boracic acid and peroxide of hydrogen. When first seen there was dullness on transillumination of the left antrum only, but when he was examined again in April the frontal sinus was also found to be opaque. The anterior third of the middle turbinal was removed, also the anterior lip of the hiatus semilunaris. It was found possible to wash out the frontal sinus and some pus was evacuated. No pain or tenderness was elicited, but on account of persistence of the suppuration it was decided to open the frontal sinus. An opening was made in the anterior wall and pus was found under tension. The cavity contained a large amount of granulation tissue. The sinus was very large, extending slightly across the middle line, but externally reaching the outer angle of the orbit. In view of the short duration of the disease it was considered justifiable to remove only a portion of the anterior wall, and to make a counter-opening in the bone at the outer angle of the orbit. Drainage tubes were introduced through this outer opening and also down the infundibulum to the nose. Syringing was carried out through these drainage tubes, but in a fortnight's time, when the drainage tubes were removed, the discharge into the nose from the sinus was so considerable that the exhibitor decided to carry out Killian's complete radical operation on the frontal sinus. This was done and at the same time the antrum was freely opened through the canine fossa. The cavity was packed and drained through the inferior meatus of the nose. The antrum was found to contain a quantity of foetid pus. In two days the plugs were removed, the healing of the frontal sinus wound took place with great rapidity, and the discharge diminished in a way it showed no sign of doing after the modified operation.

**Case of Frontal Sinus Suppuration due to Gunshot Injury,
in a Female patient, aged 26 ; radical Operation with
unusual findings. Relief.**

By J. DUNDAS GRANT, M.D.

THE patient, a woman aged 26, was first seen in March, 1909, complaining of discharge from the nose of seven years' duration, also of pain in the left supra-orbital region. There was a vertical depressed scar in the middle line of the forehead dating from a gunshot wound which had required some seven operations, probably for the removal of shot and portions of necrosed bone. On account of the pain and discharge the exhibitor thought it advisable to perform Killian's radical frontal sinus operation. When the anterior wall of the sinus was removed a bluish bulging pulsating membrane was exposed, which was of extremely thin consistency. The whole of the anterior wall was then removed outwards and upwards, but no posterior wall could be found. During the exploration the membrane was punctured and some watery fluid escaped, after which the membrane collapsed. The bony floor of the frontal sinus was then removed in the usual way and the operation was completed. The exhibitor was in complete uncertainty as to the nature of the pulsating membrane, but in view of the disturbance of the normal topography produced by the injury and by the previous operations he felt that it was safer to close up the wound in uncertainty rather than to risk making an opening into the meningeal space. During the following night the patient had some delirium and vomiting of a "pumping" nature for twenty-four hours. This ceased, however, towards the evening, and when the wound was dressed it was quite healthy in appearance. The temperature never went above 99.2° F., and no disturbing symptoms presented themselves. She had still some discharge from the nose, but asserted that her head was freer from pain than it had been for several years. The exhibitor requested opinions as to the nature of the exposed membrane.

DISCUSSION.

Sir FELIX SEMON said he asked the patient what sort of injury she had suffered ; it appeared not to have been a gunshot wound, but one inflicted by a pea rifle. That would explain, first, why the bullet entered the cavity of the

frontal sinus, because unless a gun was fired at close proximity one would scarcely expect an ordinary pellet from a gun to enter through the front part of the frontal bone and get into the frontal sinus. Moreover, the patient had been told that the bullet splintered up in the interior of the sinus. That might serve as an explanation why the whole posterior wall also was injured extensively, necrosed, and ulcerated away in course of time. This, in turn, would explain there being at the time of the operation no bone on the posterior wall, and the existence of the pyogenic membrane of which Dr. Grant spoke. If an ordinary gun had been discharged so close there certainly must have been extensive injury to the face; but not a trace of injury was visible.

Dr. DAN MCKENZIE, who had assisted Dr. Grant at the operation, said that the explanation of the case which had occurred to him was that the bulging membrane was the wall of an arachnoid cyst, which had formed as a result of the antecedent traumatism.

Dr. FITZGERALD POWELL said it was rather improbable that the gunshot injury sustained should destroy the *posterior* wall, leaving the *anterior* wall intact. He suggested that the sinus originally might have been absent, or a very small one, and that the *anterior* wall of the skull or sinus might have been destroyed by the injury, so that in operating the posterior wall might have been removed, exposing the dura. He had himself found great difficulty in locating the walls in a case of multiple sinusitis with abscess of frontal bone in which the frontal sinus was thought to be involved, and in which much diseased bone was found and a very small sinus.

Dr. GRANT, in reply, said the tumour pulsated with respiration. The gun was in the patient's own hand, so that it must have been fired point blank. He considered Dr. Fitzgerald Powell's suggestion most ingenious, but he did not think, with the care taken in the operation, that if the condition mentioned had been there it would have been overlooked, because in removing the wall the infundibulum was found behind it.

Case of Congenital Fistula in Mid-line of Nose.

By H. LAMBERT LACK, M.D.

THE patient has a small fistula in the median line of the nose opposite the junction of the cartilages and bones. The opening barely admits the finest probe. The sinus runs upwards towards the nasal bones. Four patients with this exceedingly rare condition are at present under my care. I have never seen one before, but one case of dermoid cyst in this region was shown at an early meeting of this Society. The microscopical specimens were prepared by Dr. H. M. Turnbull, of the

London Hospital, from the first case operated on. The fistula dilated below the surface, forming a small, trumpet-shaped mass. Dr. Turnbull reported that on longitudinal section the epithelium dipped down with its horny covering on each side of a narrow channel which dilated to form the wider cavity. This latter cavity is lined with epithelium with horn, and in one of its walls are hairs. The deeper part of the trumpet-shaped mass consists of vascular connective tissue with a central small oval mass of squamous epithelium surrounded by a "palisade" layer of basal cells lying on a hyaline basement membrane. It is solid and shows no horn. At one end the cells are swollen, suggesting a sebaceous gland. In one section there is a special layer of connective-tissue cells, apparently dermis of a hair follicle. These epithelial growths have, therefore, the structure of hair follicles. There are some sweat ducts in this section.

DISCUSSION.

Dr. FITZGERALD POWELL said he had seen three or four of those small congenital fistulae which exuded cheesy sebaceous matter, and situated in the same position on the anterior aspect of the nose as in the cases shown. He did not think they were uncommon, and must be in the experience of the members of not infrequent occurrence. He thought they were congenital and were similar in character to those found about the auricles.

Dr. KELSON said that in 1904 he showed at the Laryngological Society a case which was very similar in a girl. He termed it a sinus of the nose, and he thought the term sinus was more correct than fistula, because in most cases there was no connexion with any cavity. In nine out of ten such cases the parents strenuously denied that there was anything there when the child was born, or for the first few years of life, and that raised the question whether the condition was not originally in many cases really cystic, and that it suppurated or broke down, leaving a sinus. This was quite consistent with their being foetal remains, and the honey-like fluid and microscopical appearances seemed to show them to be of a congenital nature. He asked whether other members had noted this peculiarity in the history of these so-called congenital fistulae.

Dr. PEGLER said he had a similar case in a patient who came over from Australia in 1897 and was cured by operation.

Mr. HERBERT TILLEY said he had recently seen a similar case, and the nasal fistula was associated with a dermoid cyst in the temporal region near the external angular process of the frontal bone.

Sequel to Case of Intrinsic Laryngeal Neoplasm (chiefly of Left Vocal Cord) of unusual appearance, in a Man aged 76.¹

By R. H. SCANES SPICER, M.D.

A SPECIAL examination of fresh fragments for mycosis was made, with negative results. The patient continued to have a loud but hoarse voice after the last operation, and desired to have laryngo-fissure done, if it was thought he could be permanently relieved of his trouble. Postponement of this was advised and a watching policy. In July he passed under the charge of another surgeon, to whose courtesy I am indebted for the after-history. On July 2 that surgeon, basing his action on the increased irritation about the growth which was now marked on both cords, the great hoarseness, the fact that some pearl nests had been observed by Mr. de Santi and Dr. Pegler in the previous sections of the growth, at the patient's strong desire, performed laryngo-fissure and cleared out the soft tissues inside the larynx, after doing a tracheotomy and using Hahn's cannula. The patient was very weak. No temperature the next day; but he died forty-eight hours after of pneumonia. No post mortem was allowed. The tissues removed from the larynx were again examined and reported to be squamous-celled papilloma (keratosis), but in view of the possibility of this case being an instance of combination of innocent and malignant growth, or a transformation of the former into the latter, further detailed examination of all the specimens will be made and again reported on later.

Dr. PEGLER reminded the Fellows that, whereas in the days of the Laryngological Society, when this case had first been shown, the microscopical appearances were those of squamous papilloma crested by delicate horny cells, giving rise to the delicate white peaks that at least resembled keratosis, there had been a marked change in the sections presented on April 3, 1908. Pearly cell-nests were then invading the papillomatous tissue, giving rise to suspicions of commencing malignancy which had since manifested itself.

¹ Previously shown June, 1905; February, 1906; March, 1908; April, 1908 (see *Proceedings*, i, pp. 81-89.

Removal of a Sewing-machine Needle rigidly impacted in Median Sagittal Plane of the Larynx of a Youth.

By R. H. SCANES SPICER, M.D.

EFFORTS had been made to remove the needle by other surgeons for two hours before his arrival. The patient had an anxious look and was voiceless, but could breathe easily. Cocaine was well applied locally, and the needle could be seen with the laryngoscope in the position stated; it could be grasped with forceps, but it would not budge. It could also be felt with the tip of the finger. The patient was then examined with the X-rays and screen, and the exact position of the needle confirmed. A general anæsthetic was then given in the sitting posture and the needle seized with a pair of Mackenzie forceps, and watched in the laryngoscope. While manipulating it and trying to dislodge it cautiously, something was felt suddenly to give way, and the patient became of a death-like pallor, stopped breathing, and was pulseless. He was laid flat on his back, when it was perceived that the needle had passed through the thyrohyoid membrane, head first, and could be seen and felt as a little hard cone under the skin. He at once cut down on it and withdrew it, when breathing recommenced, and the patient recovered without further ado.

A Case of Mycosis of Tonsil, Pharynx, and Tongue in a Man aged 30.

By R. H. SCANES SPICER, M.D.

THE patient complains of a feeling of stiffness in his throat, discomfort as if of a foreign body, and fatigue after using voice. These symptoms are relieved by taking meat or drink. As the discharge of his duties involve much use of his voice, he is much troubled. There is a faint unpleasant smell in the breath which he notices. He has had it four or five months, and has been told he had "relaxed" throat and "acne of the throat." The points of interest are: (1) The diagnosis

from chronic tonsillitis; (2) as seen in the case shown at the last meeting, the bacterial findings in such a case may include the *Bacillus diphtheriæ*. (3) If the condition is essentially a keratosis, is it allied to pachydermia? (4) Should energetic treatment be pursued when the patient is so troubled? At present he is only having an alkaline antiseptic gargle and is slightly improved.

Laryngeal Growth in a Man aged 31.

By IRWIN MOORE, M.B.

THE patient, a clerk, came to the Throat Department, King's College Hospital, on April 6, complaining of hoarseness of voice for three years, coming on gradually, with sore throat for the past seven weeks; only as voiceless as he is at present since February last. There are no signs of tuberculosis of lungs; no temperature; no enlarged glands. On examination, an irregular growth is seen projecting from the left ventricular band, covering all but the posterior end of vocal cord on that side, which is fixed; no ulceration of surface. The case is similar to one shown at this meeting on March 5 last.¹

DISCUSSION.

Dr. H. J. DAVIS said it looked to him like a papilloma, but the position was an unusual one.

The PRESIDENT (Dr. Dundas Grant) considered it more likely to be a fibroma, possibly œdematous, on account of the smoothness of its surface.

Chronic Maxillary Suppuration simulating Malignant Disease.

By JAMES DONELAN, M.B.

THE patient, a woman aged 54, was admitted to the Italian Hospital on March 14, 1909, under the care of Mr. T. P. Legge. There was a large fungous mass occupying almost the whole right alveolar border

¹ See p. 113.

and extending nearly to the middle line of palate. Two or three decayed tooth stumps present and whole area of superior maxilla swollen and tender. The condition had arisen gradually in previous three months. Appearances suggested sarcoma. On March 19 Mr. Legge extracted the stumps, perforated the alveolus, and found pus. Microscopically the fungous mass consisted of inflammatory tissue. Treatment of suppuration and inflammation by irrigation through alveolar border. Mr. Legge transferred patient to exhibitor for intranasal operation. This was done on April 16, and consisted in the removal of the middle third of the inferior turbinal, together with a large triangular portion of the external nasal wall by means of rectangular chisel. The posterior portion of turbinal being polypoid was removed on April 22. Patient made good recovery, though there is pus still at times, probably due to re-infection through alveolar opening, which it is now proposed to close. The anterior portion of turbinal was left because it bears no useful surgical relation to the antrum, while its removal endangers the nasal duct, and because leaving it gives some protection to the newly exposed cavity from the respiratory current.

DISCUSSION.

The PRESIDENT (Dr. Dundas Grant) asked whether anyone could explain the swelling on the palatal surface on the exterior border, which might be periostitis from the diseased tooth.

Dr. DONELAN thought, from the progress of the case, that the condition referred to by the President was merely the result of the dental irritation.

Primary Sore on the Upper Lip in a Girl aged 12.

By H. J. DAVIS, M.B.

THE child attended in the department for what was diagnosed as "mumps" fourteen days ago. This week what is undoubtedly a Hunterian sore is visible in the middle line of the lip. The enormous collar of glands, which are as hard as stones, is characteristic of the disease. No family history of any affection; other children well. The mother is a manageress in a laundry. Though it is possible that herpes

with septic glands, anthrax, or infection from a vaccination pustule, might cause such a condition, the colour and dry appearance of the "scab" on the lip and the unusual glandular enlargement is typical of primary syphilis at the junction of skin and mucous membrane. Several who have seen the child do not agree with the diagnosis, and I should therefore be glad of the opinion of the Section.



Primary sore in centre of upper lip. Collar of glands marked.

DISCUSSION.

Dr. VINRACE said he would like to know whether there were any means of fixing the exact date when the lesion was noticed on the lip. The mother said that five weeks ago the upper lip was absolutely healthy; there was nothing to be seen on it. On the other hand, there was every appearance of secondary psoriasis on the body of the child. He asked whether Dr. Davis thought the glandular enlargement might be due to some concomitant condition, partly due to the sore and partly to other causes.

Mr. ARCHIBALD SMITH said he was responsible for the specimen showing the *Spirochæta pallida*. The spirochæte shown was not as typical a specimen as one would wish, but in the same smear he found eight or ten in half an hour. The specimen was from the under surface of the scab covering the chancre. He also withdrew some juice from one of the submaxillary lymphatic glands, and made four preparations, but did not find the spirochæte in any of them. In cases of chancre of the lip he thought it very likely that the glands

were not enlarged purely from the syphilitic infection, but that there was some septic infection, and that perhaps made the finding of the spirochæte more difficult than it would be in the inguinal glands in an ordinary genital chancre.

The PRESIDENT (Dr. Dundas Grant) said that when Dr. McKenzie punctured the glands in a case of his two months ago no spirochætæ were obtained from that fluid, nor from the surface of the chancre, but only from needling in the depths of the chancre, and that bore out what Mr. Archibald Smith had said.

Dr. H. J. DAVIS, in reply, said that when he first saw the child—it was three weeks ago—she had adenitis, and looked ill. He could not find any cause for it. He felt sure the crack was not on the lip then, or he would have detected it. When next seen the case was diagnosed as one of mumps. She had a few carious teeth, which might have accounted for one or two of the enlarged glands. A fortnight later he thought the brown character of the lip was typical of syphilis in that position. He did not think there was ever much induration of the lip at the junction of the mucous membrane and skin. The sore looked flat and more like a little brown scab. In May, 1906, he showed three cases of syphilis¹ in one family, all communicated by oral infection from one to the other. A little girl aged 10 contracted syphilis at a beanfeast and gave it to her grandmother, aged 65, who slept with the child and nursed her. The grandmother communicated this to her own daughter, who was a laundress, and who had a hard chancre in the right nostril. She had a baby afterwards and gave it to the baby, and they all four had the disease at the same time. The lesion in the nose was mistaken for malignant disease; there was enormous cedema and swelling of the face. His colleague (Dr. Abraham) had told him that at the Cape it was not uncommon for such cases to run through a whole family, without signs of genital syphilis, probably because they drank out of the same cup. The present girl had no signs of genital syphilis. Her sister, aged 20, had secondary syphilis; this was only detected a week ago, and he exhibited a specimen showing the *Spirochæta pallida* taken from a scraping of the lip; there was therefore no doubt in the diagnosis.

Subsequent Report on a Case of Left Bronchocele.²

By H. J. DAVIS, M.B.

THE patient is a woman, aged 40, exhibited at the last meeting, with a unilateral thyroid tumour distorting and displacing the larynx, and causing pressure symptoms by involving the left recurrent laryngeal

¹ *Proc. Laryng. Soc.*, 1906, xiii, p. 91.

² Shown at the last meeting (see p. 138).

nerve. The tumour was removed by my colleague (Mr. Armour) on May 15; all the symptoms have already subsided. The larynx is free and in the mid-line, and the cords move naturally and symmetrically. A specimen of the tumour is shown; it is a large thick-walled cyst of the shape and size of a swan's egg.

Subsequent Report on a Case of Chronic Empyema of the Antrum, in which part of the Antrum had become affected with Malignant Disease.¹

By H. J. DAVIS, M.B.

THE patient is a woman, aged 68, exhibited at the last meeting. A specimen of the upper jaw is now shown. My colleague (Mr. Bidwell) performed the operation, which was an extensive one, the growth involving the sphenoid, perforating the orbital plate and extending into the orbital fat. The patient is doing well, but has lately developed diplopia. The growth is a sarcoma; in other parts simple mucous polypi are seen.

¹ Shown at the last meeting (see p. 139).

PROCEEDINGS
OF THE
ROYAL SOCIETY OF MEDICINE

VOLUME THE SECOND

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE
SESSION 1908-9

MEDICAL SECTION



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1909

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MEDICAL SECTION.

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Corrigendum.—The synopsis (p. 118), having been extended and included in the longer résumé (pp. 111-117), should be omitted.

The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

Medical Section.

October 27, 1908.

Dr. SAMUEL GEE, President of the Section, in the Chair.

Observations on Endemic Cretinism in the Chitral and Gilgit Valleys.

By R. McCARRISON, M.B., Captain I.M.S.

(Introduced by Mr. JAMES BERRY.)

THE present study of endemic cretinism is based on an analysis of 203 cases of the disease, comprising the total cretinous population of the Gilgit and Mastuj districts. These districts extend over an area of about 500 miles of Himalayan country. The cases have been collected by a house-to-house examination of almost every goitrous village in the district, so that few examples of the disease have escaped my observation.

At the outset I should, perhaps, direct attention to the fact that the observations to be detailed and the conclusions to be drawn refer only to cretinism as prevailing among the Chitrali and Gilgiti races. It will be found necessary when applying these results elsewhere to take into consideration such factors as racial differences, habits of life, climatic conditions, &c., which are of importance in determining the prevalence of the disease [4].

For convenience of discussion I propose to divide my subject into the following six sections:—

- (I) The incidence of cretinism and its relationship to the incidence of goitre.
- (II) Goitre in the individual and its relationship to cretinism.
- (III) Goitre in the mother and its relationship to cretinism.
- (IV) Debilitating factors and their influence on the mother in producing cretinism.
- (V) Types of the disease with associated symptoms; and
- (VI) Conclusions.

2 McCarrison: *Endemic Cretinism in Chitral and Gilgit*

(I) THE INCIDENCE OF CRETINISM AND ITS RELATIONSHIP TO THE INCIDENCE OF GOITRE.

As is well known, endemic goitre, cretinism, and deaf-mutism are associated conditions. In this respect Gilgit and Chitral provide no exception to the rule. There are certain facts, however, with regard to the association which are deserving of comment: (a) Where goitre is commencing in epidemic form, as for example in Nagar [4] or among troops introduced into infected areas [1], the younger members of the community are the first to suffer. Such an epidemic, however, produces no cases of cretinism. (b) In Chitral, on the other hand, where a relatively high percentage of children is goitrous, cretinism does occur, but is not common. I have been unable to trace a single instance in which goitre in the child has given rise to cretinism. Cretinism does not make its appearance in a goitrous family until the second or even the third generation. (c) Now, in Gilgit, children suffer much less from goitre than do the children of Chitral, but they suffer much more from cretinism. Goitre is said to be of comparatively recent introduction into Chitral. In Gilgit it has prevailed for centuries, and, in districts where goitre is more prevalent among the adult population, cretinism is more common and of a graver type. (d) Cretinism shows a marked tendency to occur in certain families. It is common to find several children in the same family cretinous. I have met with instances where every child in the family has been a cretin. While, therefore, cretinism is found to be intimately associated with goitre, the degree of this association is determined by the age of the endemic of goitre and by the extent to which the disease prevails among the adult population.

(II) GOITRE IN THE INDIVIDUAL AND ITS RELATIONSHIP TO CRETINISM.

Just as endemic goitre is rarely found to produce myxœdema in the adult, so this condition is rarely a cause of cretinism in the child; indeed, I have never met with such a case. This fact is opposed to the present-day view that 75 per cent. of cretinism in goitrous localities is due to goitre in the individual [6]. Facts bearing on this point have already been dealt with in the preceding section; the following further observations remain to be recorded:—

(1) There are in the present series of 203 cases eighty-eight, or 44 per cent., in which there is an accompanying goitre. With the exception of

two cases, in which the goitre was congenital, the history shows that the thyroid enlargement was subsequent and not prior to the appearance of the cretinic symptoms. It is possible that in some instances these histories may be incorrect; the accompanying chart (fig. 1), however, should establish this general truth. The unbroken line shows the number of cretins, the interrupted line the number of goitrous cretins in the district at each year of age up to and over 20 years. From the chart it will be observed that the older a cretin is the more likely he is to have a goitre. While only 17 per cent. of all cretins under the age

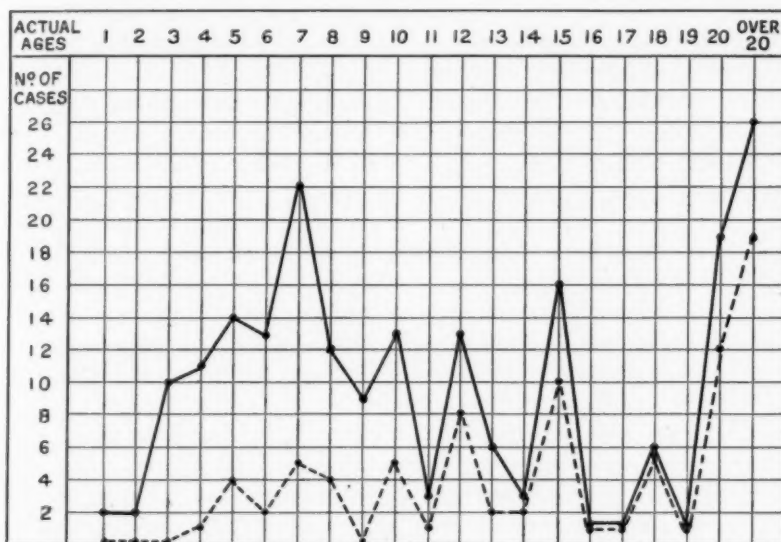


FIG. 1.

Chart showing the actual number of cretins at each year of age; also the number of goitrous cretins at these ages. The unbroken line shows the number of cretins, and the broken line that of goitrous cretins.

of 10 are goitrous, no less than 70 per cent. over that age have an accompanying goitre.

(2) I can find little support for the view that goitrous cretins are, as a rule, less swollen and their condition relatively less grave than that of those without a goitre [2]. Of the eighty-eight goitrous cretins, 20 per cent. are noted as being much swollen, while in the non-goitrous cases 25 per cent. are similarly much swollen. So far as my experience goes,

I find that the presence or absence of a goitre is a matter of very little importance to the child's myxœdematous condition. The mental defect is, however, frequently greater, and nervous symptoms are more commonly present in these without a goitre. It is to be remembered that the so-called "goitre" is in reality made up, in the vast majority, of one or more adenomata in a functionally inactive or imperfectly active organ. The presence of such a goitre would not be beneficial to the child. There are, however, some few cases in which the development of a goitre would appear to have been beneficial. Mr. James Berry, in his work on "The Thyroid Gland" [2], has instanced a case where the general body swelling diminished as the goitre enlarged. I have met with two similar cases (Nos. 5 and 159). I have noted the presence of "fatty tumours" in 24 per cent. of my cases.

(3) Cretins are much more commonly goitrous than are healthy children.

(III) GOITRE IN THE MOTHER AND ITS RELATIONSHIP TO CRETINISM.

Our conception of endemic goitre has undergone some change of late years. The disease has hitherto been regarded as non-infectious. My investigations, however, have convinced me of its infectious nature. This view, although perhaps not yet sufficiently proven to demonstration, nevertheless provides the better explanation of its general phenomena and of its sequel—endemic cretinism.

There are, as is well known, certain infectious diseases of the mother, such as tuberculosis, erysipelas, acute rheumatism, malaria, and influenza, which are capable of producing pathological effects on the child's thyroid gland. It is believed that the toxins produced by the organisms of these diseases circulate in the foetus and give rise to the morbid condition. It is to these toxins that cases of sporadic cretinism, in which there is no associated goitre, are attributed [6]. Although infectious agencies have an undoubted influence in the production of endemic cretinism this action is not limited, as has been supposed, to non-goitrous cases of the disease. Of all infectious diseases which impair the unborn child's thyroid mechanism the most important is endemic goitre. It is that disease which is beyond all others most frequently associated with cretinism.

Now in almost every case of cretinism goitre is present in one or both parents. It is present in the mother in 86 per cent. of my cases, in the father in 40 per cent. The presence of a goitre was not noted in

the mother in twenty-eight cases. In twenty of these the mother herself was not seen or was dead. If these are excluded as uncertain, goitre is found to have been absent in the mother in only eight cases, or 4 per cent. While, therefore, cretinism can occur in the child of a woman free from goitre, it must be established, as a rule, that in endemic localities goitre in the mother is one of the most essential conditions for the development of cretinism in the child.

Maternal goitres are in over 80 per cent. of cases degenerated, the seat of adenomatous or of cystic change. Such an organ cannot be regarded as possessing the same potential powers of functional activity as a normal gland. The investigations of Baumann have shown that a goitre contains less thyroidin than a normal gland, thus demonstrating the functional deficiency of the goitrous organ. Despite this defect the thyroid mechanism of the majority of goitrous women is capable of meeting the additional demands which pregnancy or other accidental circumstances may make upon it. There is, however, a minority in which this is not the case, and it is this minority which constitutes the mothers of cretins.

The experiments of Halsted and Edmunds on animals have shown the effect of an impaired action of the thyroid mechanism of the mother on the offspring. They afford, I think, an explanation of the train of events which gives rise to cretinism, especially when they are considered in connexion with the infectious origin of goitre. In describing these experiments I quote from Edmunds's [3] work on the subject: "Halsted found in the puppies of a bitch from which the thyroid gland had been removed, and which had been sired by a dog that had also in part been deprived of its thyroid gland, that the thyroid lobes in the puppies were twenty times larger than those of normal puppies" (p. 35). Edmunds repeated this experiment and obtained a similar result. He found that the changes observed on microscopical examination were those of "compensatory hypertrophy," and "were presumably due to an attempt to compensate for the absence of thyroid in the mother." The function of the thyroid mechanism is to neutralize toxins produced in the ordinary course of metabolism. In the case of the partially thyroidless bitch of this experiment there were more toxins circulating in the blood than her impaired thyroid mechanism could deal with. These toxins called forth a response on the part of the puppies' glands and determined the resultant congenital goitre.

Richardson, in his work on "The Thyroid Gland," surmises that the reverse of this experiment would probably occur, and would account for

a certain percentage of the cases. He says: "Should the mother have an excess of thyroid secretion the gland in the young would not develop, and consequently the child would show cretinic symptoms after weaning. . . . The occurrence of a cretinic condition without goitre where goitre is endemic suggests that the parenchymatous increase of the maternal gland, in conjunction with the normal hypersecretion of pregnancy, prevents the development of the foetal gland." I am of opinion that it is unnecessary to assume a reverse picture to that of the experiment quoted, believing as I do that its results are applicable directly to the goitrous pregnant woman. That in such a woman there is no excess of unutilized secretion is shown by the fact that the administration of the gland extract causes a reduction in size of the hypertrophied organ in both pregnancy and goitre. It appears to me that it is not the excess of secretion, but the greater excess of demand, that is of importance. It is the failure to meet all demands which constitutes a temporary inefficiency of these glands, and places the woman in a position identical with the partially thyroidless animal of the experiment. The goitrous mothers of cretinous children very commonly exhibit in their own persons signs of thyroid insufficiency during pregnancy. Of these signs perhaps the most noticeable is that of tetany.

There is, then, a certain minority among goitrous pregnant women in which the thyroid mechanism is deficient; these are mothers of cretins. The results of the experiment are applicable to them, but with this difference—that whereas the partially thyroidless bitch, under non-goitrous conditions, gave birth to offspring congenitally goitrous, the partially thyroidless woman, under continuous exposure to goitrous influences, gives birth to cretins.

The children of Gilgit, as I have already stated, are relatively immune to goitre. Succeeding generations have, under continuous goitrous influences, developed this degree of natural resistance to it. That children in other localities are not naturally immune to goitre, but are indeed more susceptible to it than adults, is shown by the case of the epidemic in Nagar [4]. This immunity wears itself out, and from puberty onwards the disease becomes more and more frequent, until almost half the population suffers from it during the later years of life. I regard the immunity as due to the minimal action of the toxic agent of goitre on the developing foetal organism, which gives rise in the child to considerable cumulative powers of resistance. But where, owing to insufficiency of the maternal thyroid, toxic agencies are allowed freer play, this action on the developing foetal organ is continuous and excessive, resulting in compensatory

hypertrophy, or atrophy of whole or part of the thyroid mechanism. I believe that just in so far as the mother's thyroid potentiality possesses the inherent power of response to every demand, so far may we expect her child to be born temporarily immune to goitre, with congenital goitre, or with cretinism; all of these I regard as being but stages in the same process and the evidence of the minimal, medial, or maximal action of the toxic agents on the unborn child's thyroid mechanism. The following cases may be quoted in support of the views which have been expressed; others will be found in the appendix:—



FIG. 2.

Myxœdematous mother with her cretinous child.

No. 100.—The mother is partially myxœdematous (fig. 2). She has a small goitre and suffers from tetany. These attacks are worse during pregnancy; they are more frequent during the spring months, when she may have as many as two or three during one month. There is no unconsciousness during them. She has always been myxœdematous, but believes that she is better than she used to be. She gives a very goitrous and myxœdematous family history. She has had eight children before the present child. They were all, according to her,

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"born cretins." All were very swollen from birth, and all died before the age of 3. The child shown in the photograph is aged 2. It is remarkably swollen. This child has improved very markedly under thyroid feeding.

Nos. 190 and 191.—Family very poor. Mother has a large tumorous goitre and suffers from tetany during pregnancy; she is coarse-skinned and somewhat swollen. Her son, aged 24 (No. 190, fig. 3), is a typical nervous cretin of an extreme degree; he is a deaf-mute. Her daughter (No. 191) is a typical myxœdematous cretin, aged 18, whose hearing and speech are defective. The mother has had nine children, of whom



FIG. 3.

A "typically nervous" cretin, aged 24. The right hand is blurred in the illustration owing to tremor.

four are alive, and all show signs of cretinism, the two youngest in lesser degree than the two eldest just described. The father has a large tumorous goitre (fig. 9).

No. 82.—Mother has had three perfectly healthy children. She then developed goitre, and subsequently gave birth to the present child, who became a cretin after a convulsive fit when aged 2. This cretin is aged 20 and is very swollen.

(IV) DEBILITATING FACTORS AND THEIR INFLUENCE ON THE MOTHER
IN PRODUCING CRETINISM, CONGENITAL OR ACQUIRED.

(1) *Mental Disease*.—The importance of mental disease in the parents of cretinous children is so slight that from an etiological point of view it may be neglected.

(2) *Alcoholism*.—Alcoholism, owing to the fact that the religion of the people prohibits its use, exerts no influence.

(3) *Syphilis and Tuberculosis*.—Syphilis and tuberculosis are rare as yet, although of late years they have become more prevalent. In only one of my cases is there a history of tuberculosis (No. 180).

(4) *Nervous Disease: Tetany*.—The only important nervous disease is tetany. In a recent study of the affection I found that of fifty-six mothers who are sufferers from tetany, thirteen, or 23 per cent., have cretinous children.

(5) *Consanguinity*.—Among the Syeds of Gilgit cretinism is much more common than among other classes of the community. The Syeds of all Mohammedan countries, as descendants of the Prophet, are permitted to marry only in their own sect. In Gilgit these Syed families are few, and it is practically impossible for one of their members to marry out of a goitrous family. As a consequence the stock is goitre-tainted and the taint is accentuated by in-breeding. There is a history of near marriage in 14 per cent. of my cases, and in some it is the only factor present in addition to goitre in the mother. In-breeding is therefore a factor of some importance.

(6) *Psychic Factors*.—Fright, worry, mental depression, and impressions received by the mother during pregnancy have great weight as determining factors. There is reliable history of one or other of these in over 40 per cent. of my cases.

The following cases are examples:—

No. 142.—A goitrous mother, haunted by spirit while pregnant with her first child. This child is a cretin and deaf-mute. Her second and third children are alive and healthy.

No. 157.—A goitrous mother lost her first two children, who were healthy, while pregnant with her third child. She (in her own words) "remained always crying and in grief for them." Her third child was "born a cretin" and is a deaf-mute. The next three children are alive and healthy.

No. 177.—The mother is goitrous. The first two children (girls)

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are alive and healthy. The third child, a son, died during the fourth pregnancy. The fourth child was "born a cretin" and is a deaf-mute. The fifth child was healthy, but was a girl. The sixth child was a deaf-mute. The cause was stated by mother to be "grief at the death of her only son."

No. 175.—The mother is goitrous. The first five children died young, the deaths of several of these occurring during the mother's seventh pregnancy. Her seventh child is a cretin and a deaf-mute. Her sixth child is alive and healthy. She attributed her evil fortune at the death of her children to a "spirit" which preyed greatly upon her mind during her seventh pregnancy. Afterwards the priests exorcized the "spirit." Her eighth child was born normal and is alive and well.

No. 22.—The mother is goitrous. After having borne two healthy sons she went to live in a house which she believed to be haunted. Her next three children are all cretins.

Nos. 85 to 90.—The mother was a goitrous woman whose husband died after she had borne to him a male child. This child fell from a roof at the age of 1 and became a typical nervous cretin (the boy to the right of the spectator, fig. 4). The woman suffers from tetany and has a small tumorous goitre. The second husband was a man of another village to whose house she took her cretinous son. She had two sons by this husband; they are both cretins and deaf-mutes, and both of an extreme grade of "nervous cretinism." There were, as is so often the case in Gilgit, living in the same house as her second husband two other married women. Of these one had already borne two daughters who are perfectly healthy. But after the arrival of the first woman and her cretinous son she gave birth to a child who is a "nervous" cretin and deaf-mute. This child is quite helpless (the boy to the left of the spectator, fig. 4). The other woman was not a mother at this time, but she afterwards had two children, a boy and a girl, both of whom are "nervous" cretins and deaf-mutes.

It will have been observed that the type of cretinism is in all cases the same. The mothers attribute the fact that they gave birth to cretinous children to "their continually seeing the first woman's cretinous boy in the house" and "to fear that their unborn child might be like him." There is no other history of difficult labour, near marriage, or illnesses during pregnancy to account for the condition of the children in the case of the last two mothers. A very exceptional fact in the case of the last mother is that she has no goitre. There is in her case no other history whatever apart from the mental impression.

A history of the baneful influence of "the powers of evil" is very common, and so much so that one is forced to acknowledge it as real. Other frequent histories are that "while in the jungle with the goats the mother was haunted by a fairy," that she "saw visions," or that she "saw the dead," forms of delusional insanity which, though regarded by the mother as being the causal or exciting factor in producing cretinism in the child, may perhaps be considered as evidence of the defective functional activity of her own thyroid gland. It is known that some cases of delusional insanity are due to this cause and that they may be



FIG. 4.

"Nervous" cretins with slight myxedematous symptoms, swelling of face, of the wrists and ankles, and in the armpits. (Cases 85 and 86.)

relieved by thyroid feeding. On the other hand, it is possible that the psychic influences caused the functional depression.

(7) *The Influence of Illnesses in the Mother.*—As already stated, the most important factor is maternal goitre. There are, however, three other diseases which, when occurring in goitrous women, appear to favour the production of cretinism. These are malaria, rheumatism, and arthritis deformans. Others less frequently occurring are painful eye diseases and severe abscesses about the head. In 20 per cent. of

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all cases there is a history of illness in the mother to which the child's cretinism may reasonably be attributed. The following will serve as illustrative cases:—

No. 169.—The mother, who is goitrous, had suffered from granular ophthalmia before her third child was born. The disease resulted in her total blindness. The first two children were healthy, the third was a cretin, and the fourth died at the age of 4, but was healthy.

No. 177.—A goitrous mother who had suffered from rheumatism while she was pregnant with her second child. This child is cretinous to a very severe degree, but is non-goitrous. The first and third children are healthy. The fourth child is dead.

No. 164.—The mother, a goitrous woman, developed severe bronchitis before her sixth child was born, from which she still suffers severely. Her first five children are normal. Her sixth is a cretin (fig. 5).

No. 161.—The mother had abscess of the jaw before her third child was born. Her first two children are healthy. Her third child is a cretin. The fourth and fifth children are dead, but they "looked cretinous" according to the mother's statement.

I believe that illnesses in the mother are of more frequent occurrence than I have noted; it was not till I had collected seventy-eight cases of the disease that a few histories of such illnesses which had been voluntarily offered drew my attention to them.

(8) *Prolonged or Difficult Labour*.—There is such a history in 14 per cent. of cases. It is very frequently given in addition to other factors, such as fright, or mental distress, or illnesses during pregnancy. Its importance is therefore difficult to estimate. It does, however, occur in certain cases where there is no other history, and in these it may be regarded as a debilitating factor acting on the child directly.

These are the main influences which operate on the unborn child to produce cretinism. They account for about 88 per cent. of the cases, and since their action is solely through the maternal environment they may be regarded as "congenital." Cretinism, however, is not always congenital in the strictest sense of the word. It may also be "acquired." It need not, that is to say, make its appearance at birth, but may ensue upon certain external quasi-mechanical eventualities. These may be divided into two classes: (a) nutritional, (b) accidental.

(a) The nutritional factors are: insufficient milk and ill-nourishment generally, exposure to cold, defective hygiene, and the like. Their action, however, is slow and their influence slight. They account for

2 per cent. of my cases. (b) The accidental circumstances which give rise to cretinism are three: injury, fright, or nervous shock, and disease. Many illustrations of these will be found in the appendix.

Slightly over 10 per cent. of all cases of cretinism are to be classed as "accidental." About one-half of these are goitrous, and in these also, with the exception of one case, in which the goitre is congenital, the thyroid has enlarged subsequently to the onset of the cretinous symptoms. The ages at which the disease has made its appearance in these cases vary between 6 months and 10 years. It should, however, be

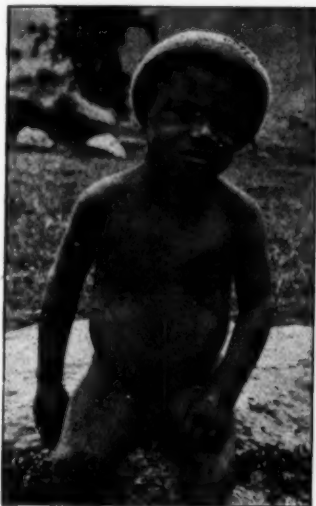


FIG. 5.

Myxedematous cretin. Case No. 164.

pointed out that these "nutritional" and "accidental" factors are exciting rather than causal in their relation to the disease. To account for them I believe that it is necessary to presuppose a congenital instability of the thyroid mechanism together with the continued action of goitre toxins.

I recognize the possibility of a perfectly healthy child becoming a cretin after prolonged exposure to goitrous influences. I have not, however, met with such a case. These influences, of course, play a very important part in aggravating congenital cretinism, and if from any

cause whatever the child's thyroid mechanism is incapable of combating them, it is reasonable to conclude that cretinoid symptoms may manifest themselves.

Cretinism and Sex.—In the series of cases which came under my observation I have found a considerable preponderance of the male over the female sex. The proportion is as 5 : 2. This preponderance is still maintained among cretins who are also goitrous, but it is much less marked, the proportion being as 5 : 4. The relatively higher death-rate among female children in Gilgit is responsible in some part for the lower proportion of female cretins.

(V) TYPES OF THE DISEASE.

There are in this district two distinct types of the disease apart from the many divers grades of the affection which are ordinarily met with : (1) the myxœdematous type, and (2) the nervous type. Cases commonly present the clinical features of a combination of these. Deaf-mutism is an almost constant accompaniment of both types of the disease. With regard to the myxœdematous type of cretinism few remarks are necessary. It corresponds with that form of the affection met with in Europe, and it is described in any text-book of medicine. It is noticeable that in Gilgit it is found for the most part among the richer families ; such cretins are better clothed and fed, and the conditions of life under which they live correspond more closely with those of European cretins.

Nervous Cretinism.—One-third of all cases in the present series belong to this type of cretinism. Among these are included some of the very worst examples of the malady. Cretins of this type, in which the disability is more especially of the central nervous system, in contradistinction to those of the myxœdematous type, in whom the defect is more especially physical, are usually to be found among the poorest of the people. They are commonly quite helpless, and their bodies invariably bear the scars of burns or other injuries. Their parents frequently do not take the trouble to clothe them, and they are exposed to extremes of heat and cold greater than anything met with in England. Their diet consists only of a daily cake of unleavened bread. The general appearance of such a case is as follows (fig. 3) : The skull is elongated, the antero-posterior diameter being long in proportion to the narrow lateral diameter (fig. 10). There is, as a rule, complete deaf-mutism. There is a knock-kneed spasticity of the lower limbs, and the patient exhibits a complete or partial inability to stand upright. When supported on his feet he usually rests on his toes, and the knees may be

close together or actually crossed, or the lower extremities may remain in a position of rigid extension. There is an increased knee-jerk and there may be marked flexion of the toes on the sole. In those cases in which the cretins are capable of walking there is a peculiar stiffness of gait, and they may walk on their toes; as each foot reaches the ground there is a certain amount of "give" at the knees and ankles, which produces a sort of bobbing motion. There is sometimes flat-foot. The upper limbs assume a position of right-angled flexion; the thumb may be drawn into the palm and the fingers closed over it, whilst the wrist is flexed. Purposeless movements of the upper limbs are common. The spastic rigidity is always worse in the lower limbs. The head may be turned slowly from side to side, and in several of the worst cases I have seen, grimaces occurred. The face is characteristically cretinoid; the degree of swelling varies considerably—it may be marked or slight, and confined to the face, hands, wrists, and ankles. The abdomen is, as a rule, swollen and protuberant. There is always considerable stunting in growth, which may be extreme or relatively slight. The patient's mentality is much disordered; there appears to be a loss of sensibility in the skin; puberty is delayed and the sexual organs are ill-developed. A history of convulsive seizures has in a few instances been obtained; a coarse nystagmus and internal strabismus have been noted in some cases. All degrees of this condition are seen, from a spastic paralysis of the lower limbs to a general rigidity; in short, the condition is one of cretinous idiocy with associated cerebral diplegia. Fig. 3 affords a good illustration of this class of case. The subject is aged 24, is about $3\frac{1}{2}$ ft. in height, is obviously myxœdematous, and presents practically every feature of the type which I have just detailed. His sister is a typical myxœdematous cretin and is very swollen.

I have sought in the course of my observations to find in the histories of these cases some etiological reason for dissociating the obvious cretinoid condition from the no less obvious spastic condition of the limbs. I have not been able to find that cretins of this type are more frequent among the class of "accidental cretins." Nor has a history of prolonged labour, of infectious diseases, of convulsions, or of any other affections of childhood afforded any grounds for the dissociation of the nervous from the cretinoid symptoms. The factors which give rise to the diplegic symptoms are ante-natal in all cases; and I believe that it is to the congenital disability of the thyroid mechanism that this condition, like the myxœdematous type, is due. Even in those cases where there is a history of possible injury at birth I believe that this

factor operates as an "accidental" circumstance acting in the way I have described in the preceding section.

In the course of this paper I have referred to the thyroid defect in cretinism as being one of the "thyroid mechanism," a defect in which the parathyroids are also included, in contradistinction to the more limited defect of the thyroid proper, which is usually considered to be the morbid anatomical factor in cretinism. There can be no doubt that such a lesion exists, but that it should extend to the parathyroids is not usually considered. These organs are, in the light of the most recent research on the subject, capable of functionally replacing the thyroid in the absence



FIG. 6.

Nervous type of cretinism.

of the latter, though not completely so. [7] [8]. They are, while possessing their own functional powers, an added safeguard to the organism in the case of insufficiency of the thyroid proper, while their own insufficiency would appear, in some measure, to determine the nervous symptoms present in many cases.

The symptoms which are characteristic of "nervous" cretinism are very similar to those which occur in animals after the complete removal of the thyroid and parathyroid glands. Indeed, as the symptoms are described by Murray [5], they are practically identical—a fact which

affords some ground for the belief which I have expressed. I have, however, obtained results in three cases by means of the therapeutic test of thyroid feeding, and by detailed post-mortem examination of the glands in a fourth, which to my mind amount to actual proof that the nervous symptoms are due to a thyroid defect.

I have had an opportunity during the past year of treating three cases of nervous cretinism on the lines indicated. Without giving full details of these cases, it may be said that the administration of the fresh and dried extracts of sheep's thyroid has produced a marked improvement in the nervous symptoms. The spasm has disappeared; in one case the double interval strabismus (fig. 6), with the associated coarse nystagmus, has almost entirely disappeared. In another, a child (fig. 7) who could only rise to its feet by means of a support, and who could only take two



FIG. 7.

"Nervous" cretin with marked swelling.

stumbling paces before its legs gave way, after three months' treatment walked for a distance of over 30 yards without falling. This child is aged 9. It was very much swollen and, according to its mother, could not speak the simplest word. She affirms that it can now say "Ma" and "Da," though it refused to do so before me. There is not the slightest doubt that its hearing has very much improved, and the mother has found it possible during the last month to employ it in certain little offices, such as the collecting of bits of wood. The child has grown 1 in. in height in three and a half months, while the swelling has disappeared and the skin become smooth and soft. The therapeutic test, then, has provided results in these three cases which amply justify my views as to the nature of the condition.

Since writing this paper I have succeeded in one case in overcoming the intense prejudice of the people against post-mortem examination. This case (No. 85) has been referred to in the fourth section of the present article, and is one of very great interest. The disease made its appearance at the end of the first year of life, and the factor which determined its manifestation was said to be a fall from a low roof. It will be remembered that the mother suffered from signs of thyroid insufficiency. The nervous symptoms in the case (fig. 4) were very marked and were the most striking feature of the condition. The swelling was slight and limited to the face, wrists, and ankles, with fatty pads in the axilla. There was no marked stunting of growth, and the case might readily have been considered to be one of cerebral diplegia with pronounced mental defect. The naked-eye appearances seen at the post-mortem examination of the child were a slight but uniform enlargement of the thyroid gland. It was very firm to the touch and was not nodular. Parathyroid glands could not be found in spite of the most careful search.

I have made at the Laboratories of the Royal Institute of Public Health, with Dr. H. Dold, Pathologist to the Institute, a study of the histological appearances of the thyroid gland in the case. We found that there was a great and uniform increase of the fibrous stroma of the organ (fig. 8). The glandular elements were compressed. Typical vesicles were wholly absent and such as were present were almost completely obliterated. Traces only of colloid were seen scattered here and there over the sections. The appearances were those of a struma fibrosa. We were unable to find any trace of parathyroid tissue.

Clearly, then, in this case there existed a pronounced defect not only of the thyroid but also of the parathyroid glands. The condition of the thyroid and the deficiency of colloid material furnish, when considered in relation to the results obtained by thyroid feeding in three similar cases, a very striking proof of the truth of the views which I have expressed.

I may here draw attention to the similarity between nervous cretins and cerebral diplegia, not only in symptomatology, but also in such facts as are known of their etiology. In cerebral diplegia cortical degeneration is, it is believed, due to the action of some toxic agent. In cretinism, the nervous symptoms are attributed, in my view, to toxins which, owing to a congenital insufficiency of the thyroid mechanism, are no longer restrained in their action. In these cases of cerebral diplegia, in which the etiology is obscure and in which the condition cannot reasonably be attributed to direct injury at birth, thyroid feeding may prove of benefit.

Deaf-mutism as associated with Cretinism.

In no less than 87 per cent. of all cases there is an associated degree of deaf-mutism. In the majority of cases it is complete; in the minority it is partial. In the nervous type it is almost always complete, less frequently so in the myxœdematous. The defect of speech may be caused in part by a swollen condition of the tongue, but it is mainly dependent on imperfect development of the higher brain centres, due, I believe, as in the case of the other nervous symptoms, to the unrestrained action of toxins. It is more frequently present in males than in females.



FIG. 8.

Section of thyroid gland from Case 85 (from photo. by Dr. C. E. Harris).

The following cases are of interest as showing the relationship of goitre and cretinism to deaf-mutism :—

No. 5.—The hearing and speech are said to have improved after the appearance of a goitre at the age of 12.

No. 137.—The patient could hear and speak normally before the age of 5, when he fell from a roof and became a cretin and a deaf-mute.

No. 149.—The patient could hear and speak before the age of 7, when—after a fright—he became a cretin of the nervous type and quite deaf and dumb.

N—12a



FIG. 9.
Goitrous father with cretinous son.



FIG. 10.
Side view of face of Case 190.

No. 159.—The patient's hearing and speech are improving slowly since the development of a goitre at the age of 35. I have referred to the case of nervous cretinism under treatment where the hearing has undoubtedly improved and the child is said to be beginning to talk after three and a half months' thyroid feeding.

(VI) CONCLUSIONS.

(1) The degree to which cretinism is associated with goitre is determined by the age of the endemic, and varies directly with the extent to which the latter disease prevails among the adult population.

(2) Cretinism is rarely, if ever, due to the development of a goitre in the individual. The thyroid enlargement is, or may be, an effect; it is not the cause of the disease.

(3) Defective thyroid function in the mother is the essential factor in the production of cretinism.

(4) Cretinism is due to the action of toxic agents, notably that of endemic goitre, on the developing thyroid of the unborn child.

(5) The thyroid defect is congenital, but it may remain latent pending its manifestation through the impulse of some accidental circumstance.

(6) The defect in cretinism is one of the whole thyroid mechanism, of the parathyroids as well as of the thyroid gland. The diversity of symptoms is due to the extent to which the defect bears on the whole or part of that mechanism.

I should, perhaps, once more emphasize the fact that the conclusions have been drawn from data afforded solely by the Gilgit and Chitral districts.

I desire to express my indebtedness to my assistant, Sirdar Sahib Kehar Singh, whose untiring help and whose knowledge of the Chitrali and Gilgiti languages rendered possible the accumulation of material on which this paper is based.

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APPENDIX OF ILLUSTRATIVE CASES—TABLES I AND II.

Cretinism in Chitral and Gilgit.

No.	Sex	Age	Goutre in patient	Age at which goutre developed	Degree of cretinism	Associated deaf-mutism	Age at which cretinism developed	Goutre in parents	Consanguinity	Protracted labour	Fright, illnesses, or impressions received by the mother during pregnancy	Ordinal number of child in family	Ordinal numbers of children in family	Ordinal numbers of children in family	Remarks
5	F.	15	Yes	12	3rd	Yes	Infancy	Mother	No	No	Anxiety to have son, and seeing other cretinous daughter	4th	1st, 2nd	3rd, 4th	Reported to be able to hear and speak a little since goutre developed
6	F.	20 (sister to a boy)	No	—	1st	Yes	Infancy	Mother	No	No	Anxiety to have a son	3rd	1st, 2nd	3rd, 4th	Has appearance of child aged 10; "nervous cretin"; head very narrow laterally
23	M.	22	No	—	2nd	No	Infancy	Mother	Yes	No	Went to live in haunted house, which preyed greatly on her mind	3rd	1st, 2nd	3rd, 4th, 5th	First two sons healthy; then changed house; next three sons cretine; this case is very swollen and myxoedematous
25	M.	18 (brother to above)	Yes	7	3rd	No	10	Mother	Yes	No	Ditto, same mother	4th	1st, 2nd	3rd, 4th, 5th	Very swollen and myxoedematous; cretinism developed after an attack of smallpox when aged 10
24	M.	15 (brother to 23 and 25)	Yes	10	1st	Yes	Infancy	Mother	Yes	No	Ditto, same mother	5th	1st, 2nd	3rd, 4th, 5th	Very swollen; fatty folds under chin; of the two healthy sons of this mother one had goutre, and his son, or the mother's grand-son, is a cretin (No. 26)
26	M.	3 (nephew to 23, 24, 25)	No	—	1st	Yes	Infancy	Father; are Syeds; mother not seen	Yes	No	Ill with "bad eyes" for eight months before birth of child; lived with cretinous brothers-in-law	—	—	—	Very swollen; this boy's two brothers are healthy

27 F.	7	No	—	2nd	No	Infancy	Father: are Syeds; mother not seen	Yes	No history available	—	—	—
83 M.	24	Yes	(?)	1st	Yes	Infancy	Both	No	No	—	—	" Nervous cretin "; strutting gait, falls easily, fatty pads under chin, very swollen and myxomatous; has two healthy brothers
35 F.	13	No	—	2nd	Yes	Infancy	Mother	No	Yes	—	—	Helpless, cannot stand, very swollen, fatty pads under chin; at birth of child mother had no milk and child was very badly fed artificially; his younger sister is a cretin
45 M.	3	No	—	3rd	Yes	Infancy	Both	No	Yes, 4 days pains	—	—	Slight swelling, fatty pads under chin, nervous symptoms
47 M.	45	Yes	(?)	1st	Yes	Infancy	(?)	No	—	—	—	Can crawl only
48 F.	15	Yes	9	2nd	Yes	Infancy	Mother	Yes	No	—	—	Cannot stand or walk, very swollen pads in axilla; a notorious case owing to supposed connexion of case with invasion of Gilgit by Ghor Rhaman
50 M.	25	No	—	2nd	No	Infancy	Mother	No	Yes; Believed herself haunted during pregnancy; " Paradise had come to her " (lit. translation)	—	—	Nervous type, cannot walk
55 F.	7	Yes	6	1st	Yes	Infancy	Mother	No	No	—	—	Very swollen and small for age
57 M.	8	Yes (congenital)	—	3rd	Yes	Infancy	Neither (?); mother not seen	No	No	—	—	Very swollen, chin pads, cannot walk, spastic condition of limbs
63 F.	16	Yes	14	1st	Yes	Infancy	Neither (?); mother not seen (dead)	No	No (?)	—	—	Father married child's mother in old age; all children by previous wife healthy
										—	—	Very swollen, pads chin and clavicle, cannot walk; nervous type (mixed)

No.	Sex	Age	Goutre in patient	Age at which Goutre developed	Degree of cretinism	Associated deaf-mutism	Age at which cretinism developed	Goutre in parents	Consanguinity	Protracted labour	Fright, illnesses, or impressions received by the mother during pregnancy	Ordinal numbers of healthy children in family	Ordinal numbers of cretinous children in family	Remarks
69	F.	30	No	—	1st	Yes	Infancy	Both	No	No	Went to live in house believed to be haunted, where this child was born	1st, 2nd, 3rd	4th	Helpless, cannot walk, much swollen, pads chin and clavicle; her father was "daft"
180	F.	12	Yes	10	1st	Yes	Infancy	Father; mother dead	No	No	Phthisis, from which she died	None	1st, 2nd, 3rd; last two (dead)	Very swollen, pure myxoedematous case; mother married twice; this is family by second husband; always suffered from phthisis; had three children by first husband; reported all cretins, but dead
70	M.	18	Yes	16	2nd	Dumb; hearing defective	Infancy	Neither (?) ; mother not seen	Yes	No	Mother's first husband died, whom she loved; forced by Mohammedan law to marry his brother, whom she hated; child is son of second husband	—	—	This law is a very real trouble in many cases, where the woman is not willing; in this case, also, the woman was worried about the disposal of her first husband's property; children by first husband healthy
79	F.	8	Yes	6	3rd	Defective	Infancy	Both	No	No	Possessed of a spirit	—	—	Suffers from epilepsy
80	M.	7	Yes	3	2nd	Yes	Infancy	Both	No	No	No	—	—	Is half-brother to No. 79, same father; cannot walk, pads clavicle and chin
85	M.	15	No	—	1st	Yes	1 year	Mother	No	No	No	No others by this husband	—	Mother gets "fits" resembling tetany; patient nervous cretin, cannot stand; spastic condition of limbs (fig. 3); cretinism developed after fall from roof when aged 1; is first child of family referred to in text

86	M. 9 (Half-brother to 85)	No	—	Yes	Infancy	Both	No	No	"Constantly seeing her first son," this No. 85, and "fear husband that her child will be like him"	1st by this husband	None	1st, 2nd	Nervous type, waddles, small for age, granular eyelids; mother subject to "fits"
87	M. 5 (Brother to 86)	No	—	Yes	Infancy	Both	No	No	As above	2nd	None	1st, 2nd	Nervous type, waddles; as above
88	M. 11 (small tumour)	Yes	10	Yes	Infancy	Mother not seen; father no	No	No	Constantly seeing No. 86; rest as above	3rd	1st, 2nd	3rd	Helpless, very bad case, so bad parents do not even clothe him; lies where placed; very extreme nervous type, granular lids (No. 2 in group, fig. 4); was born after No. 85 came to reside in house; mother had two healthy girls before this
89	F. 11	No	—	Yes	Infancy	Neither; mother small, goitre now, but not during pregnancy (?)	No	No	As above	1st	None	1st, 2nd	Waddles; nervous type; father is brother to father of Nos. 86, 87, 88
90	M. 8	No	—	Yes	Infancy	Neither	No	No	As above	2nd	None	1st, 2nd	Helpless, nervous type, as above; mother gives, as an added reason, insufficient milk in this case
94	F. 5	No	—	Yes	Infancy	Both	No	Yes	Mother seriously ill during pregnancy (rheumatism)	—	—	—	Waddles; mother had no milk after birth of the child, who was consequently insufficiently nourished
99	M. 5	No	—	Yes	Infancy	Mother	No	Yes	Very ill during pregnancy (fever, type unknown)	—	—	—	Nervous type, cannot walk, pads under chin
100	M. 2	No	—	Deaf, too young to say if dumb	Infancy	Both	No	No	Very anxious to have a healthy child	9th (previous 1st child like this one, all died when aged 3)	None	9th	Enormously swollen (see fig. 2); mother myxoedematous, subject to "fits," small tumour in isthmus; father very large goitre

No.	Sex	Age	Goitre in patient	Age at which Goitre developed	Degree of cretinism	Associated deaf-mutism	Age at which cretinism developed	Goitre in parents	Consanguinity	Protracted labour	Fright, illnesses, or impressions received by the mother during pregnancy	Ordinal number of child in family	Ordinal numbers of children in family	Ordinal numbers of children in family	Remarks
103	F.	20	Yes	6	2nd	Yes	Infancy	Mother	No	No	Illness during pregnancy (nature unknown)	3rd	1st, 2nd (males)	3rd	Waddles; able to work; married, no children
124	M.	24	Yes	16	1st	Yes	Infancy	Mother	No	No	Was frightened by seeing a dead body while pregnant	—	—	—	Can waddle
The following cases are from Chitral:—															
136	M.	17	Yes	12	2nd	Yes	Infancy	Both	No	Yes	Death of three children and worry that unborn child would die also	6th (last child)	1st, 2nd, 3rd, 4th, 5th (died after birth)	6th	Mother was ill for a year after birth; child fed artificially; cannot walk, waddles a little
142	F.	5	No	—	2nd	Yes	Infancy	Mother	No	No	Haunted by spirit during pregnancy	1st only	2nd, 3rd	1st only	—
143	M.	7	No	—	1st	Yes	Infancy	Both	No	3 days pains	Jealousy of second wife	1st and 3rd	—	—	Nervous type
147	M.	7	Yes	6	2nd	Yes	Infancy	Mother	No	No	Grief at loss of first two children	3rd	1st, 4th, 5th	2nd (died when aged 7), 3rd	Mother "gets possessed of a spirit" and has "fits" and becomes unconscious
148	F.	12	Yes	10	2nd	Yes	Infancy	Both	No	Yes	Grief at death of her brother and his children, to whom much attached	3rd	1st, 2nd, 4th	3rd only	Strutting gait
156	M.	25	Yes	20	2nd	Yes	Infancy	Mother	No	Yes	Husband ill-treated her	1st	2nd, 3rd, 5th	1st, 4th, 6th	Can waddle
157	F.	40	Yes	30	1st	Yes	Infancy	Mother	No	Yes	Grief at loss of two first children in infancy; had "fever" for two months during pregnancy	3rd	4th, 5th, 6th	3rd only	Mother says "she mourned greatly for the loss of her first two children, always remaining crying and in grief; she went to the mullahs (priests), who gave her some charms, after which she had three healthy girls"

161	M.	40	No	—	2nd	De- fective	Infancy	Mother	No	No	Had bad abscess of jaw, could not eat	3rd	1st, 2nd	3rd 4th (?) 5th (?)	Mother believes her fourth and fifth children were also cretins, but these died in infancy and not seen
162	M.	8	No	—	1st	Yes	Infancy	Mother	No	Yes	Her fourth child died during this pregnancy; grief; had anemia and swelling all over body, which gradually got well after birth of the child	5th	1st, 2nd, 3rd, 4th	5th only	Helpless; typical nervous case; mother had insufficient milk for this child
164	M.	14	No	—	2nd	Yes, im- proving slowly	Infancy	Both	No	No	Had bronchitis, which became chronic during pregnancy; still very bad	6th	1st, 2nd, 3rd, 4th, 5th (3rd, 4th dead)	6th only	Very small, much swollen; mother had insufficient milk
165	M.	12	No	—	1st	Yes	Infancy	Mother only; father incom- plete myx- edema	No	No	Rheumatism which got well after delivery	2nd	1st (dead), 5th, 6th	2nd, 3rd	Very much swollen; pads chin, clavicle and axilla; cannot walk, helpless; nervous symptoms marked
166	M.	4 (Brother to 165)	No	—	2nd	Yes	Infancy	Mother	No	No	Rheumatism re- turned during this pregnancy	3rd	1st (dead), 5th, 6th	2nd, 3rd	Nervous type; cannot walk; pads chin and axilla; the father in these two cases is slightly myx- edematous
167	M.	4	No	—	1st	Yes	Infancy	Both	Yes	Yes	No	2nd	1st	2nd only	Nervous type, helpless
169	M.	13	Yes	10	1st	Yes	Infancy	Father; mother not seen	No	No	Ophthalmia; re- sultant total blindness	3rd	1st, 2nd (dead), 4th (dead)	3rd only	Very small, very much swollen; pads in clavicle, nervous type, flat-foot, granular lids; father partial deaf-mute and cannot walk properly
173	F.	9	No	—	1st	Yes	Infancy	Mother; father partial myx- edema, no goitre	Yes	No	Husband ill-looking and unfaithful; grief at having to marry him	1st	None	1st, 2nd, 3rd	Slight nervous symptoms, cannot walk, flat-foot; mother hates father, who is myxedematous; he is her first husband's brother; was forced to marry him by law; priests would not let her evade it; second and third children much swollen and myxedematous

No.	Sex	Age	Goutre in patient	Age at which goutre developed	Degree of cretinism	Associated deaf-mutism	Age at which cretinism developed	Goutre in parents	Consanguinity	Protracted labour	Fright, illnesses, or impressions received by mother during pregnancy	Ordinal numbers of children in family	Ordinal numbers of healthy cretins in family	Ordinal numbers of cretins in family	Remarks
201	F.	12	No	—	1st	Yes	Infancy	Mother; father partial myxedema	No	No	Jealous of second wife, whom husband married when pregnant with this child	2nd	2nd	2nd only	Very swollen and myxedematous
182	F.	20	No	—	2nd	Defective	Infancy	Mother; father myxedema	No	No	No	1st, 3rd, 6th boys all healthy	2nd, 5th, 7th girls all cretins	2nd only	Father very small, swollen, incomplete myxedema; all the boys are healthy and all the girls cretins
196	F.	13	No	—	1st	Yes	Birth	Mother; father myxedema	No	No	Jealous; husband married second wife at time of pregnancy	2nd	1st, 3rd (dead)	2nd only	Swollen and myxedematous (very); pads chin, clavicle and axilla; father stunted and swollen
190	M.	24	No	—	1st	Yes	Birth	Father and mother	Yes	Yes	No, but suffers from "fits," probably "tetany"	1st	5 dead	2nd, 1st, 8th, 9th	Fig. 3. Much swollen, typical nervous case, very small; mother slight myxedema, says she used to be more swollen than she is now; second child very swollen, eighth and ninth less so
159	M.	45	Yes	35	3rd	Defective	Birth	Mother	No	No	Bad "fever" during pregnancy	1st	2nd, 3rd	1st only	Very small, medium swelling, used to be much worse but getting less; hearing and speech also improved of late years since appearance of goitre

The degrees of cretinism noted in these appendices of cases are purely arbitrary; they have been adopted for convenience of tabulation.
First degree, most grave; second, less so; and so on.

Accidental Cretinism.

No.	Sex	Age	Goutre in patient	Age at which cretinism developed	Degree of cretinism	Associated deaf-mutism	Age at which cretinism developed	Circumstances which determined the development of the cretinism	Goutre in parents	Consanguinity	Protracted labour	Fright, illnesses, or impressions received by mother during pregnancy of cretinous child	Facts concerning other children in the family	Remarks
3	M.	20	Yes	17	1st	Yes	10	Not known	Mother	No	No	Mental distress; death of children	Brother also cretin and deaf-mute	The history in these two cases is very incomplete; were first cases met with at very early stage of inquiry, when points of importance were not fully appreciated
4	F.	6	No	—	2nd	Yes	2	Not known	Father; mother not seen	Yes	No	No	—	Roof about 4 ft. to 5 ft. from ground; houses always below level of ground
8	F.	8	No	—	2nd	Yes	1	Fall from roof	Neither; mother not seen	No	No	No	—	Considerable swelling and nervous symptoms; Syed family, consanguinity very marked
16	F.	15	Yes	12	1st	Yes	3	Fall from roof	Both	Yes	No	No	—	Swelling marked, myxoedematous type
25	M.	18	Yes	7	3rd	No	10	Smallpox	Mother	Yes	No	House haunted	Sister also cretin, one brother healthy	
28	M.	20	No	—	2nd	No	10	Fall from tree; injuries to head and spine; unconscious for one month	Father; mother not seen	No	No	No	—	Two first sons normal; then came to live in haunted house, next three sons cretins; this is fourth
41	M.	20	Yes	10	2nd	Yes	10	Fall from tree	Mother	No	No	No	—	Nervous symptoms marked, with myxoedema, hearing and speech defective since accident; symptoms might all be due to grave accident, apart from thyroid gland, but for myxoedema
49	M.	22	No	—	2nd	Yes	1	Fall from girl's arms who was carrying him	Mother	No	No	No	—	Very swollen, no nervous symptoms, can walk; pads under chin
														Swollen, clavicular pads, cannot walk

No.	Sex	Age	Goitre in patient	Age at which goitre developed	Degree of cretinism	Associated deaf-mutism	Age at which cretinism developed	Circumstances which determined the development of the cretinism	Goitre in parents	Consanguinity	Protracted labour	Fright, illnesses, or impressions received by mother during pregnancy of cretinous child	Facts concerning other children in the family	Remarks
64	M.	11	No	—	2nd	Yes	1	Abscess in head	Both	No	No	No	—	Child born with marked "bulging of brain" (literal translation); got gradually worse after abscess in head, swelling slight, can walk Cause of illness unknown; very small for age, idiot; swelling slight
65	M.	15	No	—	3rd	Yes	3	"Internal trouble for three years, for which he remained constantly crying"	Mother	No	No	No	—	Granular eyelids
75	M.	7	No	—	2nd	Yes	6 mos.	Fall; how not specified	Father; mother not seen	No	No	Possessed of spirit	—	Very swollen, pads, waddles; is said to be less after goitre appeared; hearing and speech worse; mother developed goitre when aged 30; before this she had three children, all healthy; afterwards this child was born
82	M.	20	Yes	7	1st	Defective	2	Convulsions	Mother	No	No	No	Three first children healthy; this is fourth	Child insufficiently nourished, typical cretinoid appearance
85	See Appendix I													
112	M.	25	No	—	Dwarf	3	3	Fall from roof	Both	—	—	Mother had attack of rheumatism; afterwards no milk	—	Can waddle only; swelling medium
117	F.	9	No	—	2nd	Yes	1	Fall from roof	Mother	No	No	Fright, causing unconsciousness (nature not specified)	—	Second child: first and third healthy, fourth a cretin
137	M.	35	Yes	25	2nd	Yes	5	Fall from roof	Mother	No	—	—	—	This child is believed by father and relatives to have been cretin from birth, but since fall from roof has become utterly helpless; hearing and speech also gone since then; upper part of body much swollen

146 M.	3	No	—	1st	Yes	1	Child at first looked well, but one year after birth both mother and child went to jungle with goats; "frightened by fairies"; mother brought back unconscious and remained so for three days; afterwards hysterical, after which child rapidly became a cretin	Both	No	No	First six children died in infancy seventh, eighth, and ninth healthy	Tenth child; Very marked case; nervous; also much swollen; cannot stand
149 M.	30	Yes	20	2nd	Yes	7	Fright while playing in graveyard; was carried home unconscious and feverish; he remained in this state for over two months	Mother	No	No	No	Elder brother is healthy
160 M.	30	Yes	24	3rd	Defective	10	Went to jungle with mother, got frightened there, brought home unconscious	Both	No	—	—	Has one younger brother healthy
163 F.	10	Yes	9	2nd	No	5	Smallpox, when she lost both eyes	Mother	No	No	Rheumatism and grief at death of child, her first-born	This is the second child; first, third and fourth died in infancy; fifth healthy; sixth, aged 27, looks normal
193 M.	10	Yes (congenital)	—	2nd	Yes	1	Child was "haunted in jungle by spirit"; cried out; got fever afterwards and was long ill	Father only	No	Yes	Severe eye trouble, confined to house; mullabs had told her all her children would be cretins, and the charms they gave her were useless	First died, second Can waddle; type mixed nervous and myxoedematous

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First degree most grave; second, less so; and so on.

DISCUSSION.

The PRESIDENT (Dr. Gee) remarked that the paper was a most interesting one, and although it related to India it referred to diseases which were not uncommon in England.

Sir VICTOR HORSLEY said that in response to the President's invitation he could only express his high indebtedness to the author of the paper, and would simply allude to the question of treatment. He understood from Captain McCarrison that operations were not looked on with favour in the Gilgit Valley. That was most unfortunate, because with such an enormous mass of material which he had at his disposal it ought to have been possible to have settled the question of the success or non-success of the grafting of normal thyroid in such cases. The small amount of grafting which he (Sir Victor) had done in this country had been entirely in relation to adults; he had not grafted the sporadic cretin, but treated it in the old way with dried tabloids. The rapid effects produced by grafting a piece of normal human thyroid was very striking, so much so that he thought all the sporadic cases, as soon as the disease was diagnosed, should receive a graft of normal human thyroid. The question naturally arose how such grafts could be obtained, because, although it was an extremely trifling operation to remove a portion of normal thyroid from a healthy person, still, people had a great objection to being operated upon. He suggested that the procedure he had adopted was quite good, and better than the procedure originally introduced by Bircher a long time ago, namely, grafting portions of goitres. That procedure had been carried out by Christiani to a considerable extent. He suggested that in a simple case of adenoma, when the adenoma was shelled out, a portion of the neighbouring gland could be removed, as was well known, without risk; and that could be used as a graft. He had done that with very marked success. Unfortunately, the subject of grafting, although it was long ago drawn attention to by von Eiselsberg, had never been followed out in this country, possibly because the effect of the dried gland had also been so striking. But he was sure that if anyone had a marked case of sporadic cretinism under his care he ought to get it grafted as soon as possible. Unfortunately, the question could not be answered from the experience in the Gilgit Valley, owing to the objection on the part of the natives to be operated upon. The Section was, however, under no less a debt of gratitude to the author for his paper. He did not know whether there was sufficient material in this country to compare with some of the author's conclusions, such as the relative frequency of deaf-mutism, which certainly did not accord with his own experience. He did not believe that deaf-mutism in this country bore any direct relation in degree to cretinism. The point, however, was a new one to him. With regard to the other matters, he did not know if the correlative influences of shock could have the importance which the author mentioned. But perhaps great importance was not attributed to it; the author might only have adduced the fact that any depressing agency might assist the condition.

Mr. JAMES BERRY desired to pay a tribute of respect and appreciation to the author for his valuable paper. Perhaps no one knew better than he did the amount of hard work which Captain McCarrison had been doing in connexion with the subject during the last three or four years in India. In the main, his conclusions concerning goitre in India bore out those which had been arrived at by many others, in Switzerland and other European centres notable for goitre. But the great advantage of the work was that the investigations had been carried out by one man, and for that reason he thought them in some respects more valuable than those of, for instance, the Sardinian Commission, which consisted largely of reports received from a number of people working on different lines. He thought the author had made a new point in regard to the type of cretinism which he called nervous. He was not sure whether, from the description, Captain McCarrison would include under that term most of the cretins met with in, for instance, a Swiss valley, in whom there was, as a rule, very little swelling, or whether he would include cases of what most people would call simply idiots, quite apart from cretinism. It was well known that in all goitrous districts idiots were by no means uncommon who were not supposed to be cretins, and the proportion of idiots to cretins in such places was often as high as 1 in 3. The ordinary cretin seen in European centres of goitre were by no means myxoedematous in the strict sense of the word. One rarely saw a cretin with much general subcutaneous swelling, such as was common in a myxoedema patient in a London hospital. He would like to know whether the author would include among the nervous cases those without swelling, but who had all the other ordinary features of cretinism, such as pallor, mis-shapen head, swollen ends of bones, and swollen abdomen. The author had mentioned the fatty tumours, and he supposed he meant the diffused tumours met with in cretinism, which were not lipomata. He regretted to hear the author, in speaking about parathyroids, apparently endorsed what he (Mr. Berry) regarded as a fallacy, namely, that there were two glands in the neck with different functions, the thyroid and the parathyroid. He did not believe that the functions of the parathyroid differed from those of the thyroid itself. Everyone was familiar with small masses of thyroid tissue, often undeveloped, which lay close to the main gland, and he thought that in the last few years a large amount of what he regarded as false teaching, especially with respect to exophthalmic goitre, had grown up around the idea of there being two bodies with different functions. He regretted that the author apparently lent support to that view. He had hoped that Dr. Forsyth's elaborate paper, published a few months ago, had dispelled that superstition.

Dr. GOSSAGE said there were several entirely new ideas in the paper for which members were indebted to Captain McCarrison. He (Dr. Gossage) did not propose to deal with the nervous type of cretinism, which was quite a new clinical type and of great interest, but with certain other points. The author did not agree with St. Lager's opinion that two healthy parents going into a goitrous district were liable to have a cretinous child, but said that in order to get cretinism it was necessary for two or three generations to have lived in the

district and to have suffered from goitre. Cretinism might then appear in the third or fourth generation. That was a very important point and needed fresh investigation. Anyone who had read St. Lager's book would feel inclined to be doubtful about many of the cretins which that authority accepted; he was certainly prone to exaggerate the occurrence of cretinism: It had to be remembered that cretinism and goitre were not necessarily associated, for it seemed to be proved that it was possible to find endemic goitre in a district without cretinism. Another fresh conception was that of thyroid deficiency, and the author advanced an original theory with regard to the production of cretinism on the basis of this thyroid deficiency: that thyroid deficiency in the mother allowed the circulation of toxins in her blood, and then through the placenta in the blood of the foetus, thus leading to the maldevelopment of the child's thyroid and so to cretinism. That theory of Captain McCarrison was very ingenious, but it did not seem to be quite supported by the facts. There was Edmunds's experiment in which he removed the thyroid of a bitch, and instead of that leading to a poor thyroid in her puppy, the puppy had an enormously hypertrophied thyroid. He (Dr. Gossage) thought it was possible to explain the facts brought forward by the author on another hypothesis, which had been advanced several times before, namely, that cretinism was an inherited condition, or rather that thyroid deficiency was an inherited condition. There was the old view brought forward by the brothers Wenzel that cretins could be divided into three classes: (1) what were usually called cretins, idiots incapable of reproduction; (2) semicretins, who, after reaching adult life, become capable of reproduction; (3) cretinous people who only showed certain slight signs of cretinism. He thought the author's cases of thyroid deficiency came into the third of those classes, but the class was more extensive than was usually supposed. Cretins almost invariably had a mother who showed some signs of thyroid deficiency. The author had pointed out that the condition was mainly derived from the mother, who usually had a goitre. But males were more frequently cretins than females, and in the male the condition was the more severe. The severer conditions appeared at birth and the less severe later in life; and the less severe conditions were particularly prone to be associated with goitre. Therefore one would expect that, as the woman showed it less severely than the man, a woman with thyroid deficiency would be particularly prone to have goitre. Assuming it to be an hereditary condition, it would be very much on the lines of inheritance shown by hæmophilia, in which the male was much more severely affected than the female, females often escaping; and when the female was affected it was more towards puberty or still later in life. In cretinism, as in hæmophilia, a large majority of males affected were incapable of reproduction, and so both conditions would be mainly transmitted through females. Assuming the condition was inherited, one came to the question of what were the chief features of the inheritance. First, it was mainly transmitted through the females, but, judging from the paper, it was sometimes transmitted through the male, as some of the fathers were myxædematous. Again, the condition was not passed on to all the children, but only to some, probably to half the children. If it were assumed that thyroid deficiency was passed on by inheritance, and passed

only to half the children, it was found to show itself in a severe form only in a certain proportion of the affected. It appeared that in a number of families sixty-five children were "normal" and forty-eight were marked cretins. He would have expected a smaller proportion of cretins, but he thought the explanation was that the author had naturally chosen for illustration those families which had the larger number of cretins in them. Included amongst the normal were all the slighter cases of thyroid deficiency. Assuming these points to be correct (and more research was wanted), they suggested that the inheritance was on Mendelian lines. If so, the majority of the affected people, however slightly or severely affected, would not be what was termed "pure." The condition would be dominant to the normal condition; *i.e.*, when present it would show itself more or less. In a small community one would expect a considerable amount of intermarriage among relatives, and so intermarriage amongst persons who were both carrying the factor of thyroid deficiency. Such intermarriage would give a certain proportion of people "pure" with regard to that particular deficiency, all of whose children should be affected. In the paper there were a fair number of families in which every child was affected with cretinism. That was very suggestive, especially when one considered that the vast majority of the children in the district were normal, not cretins. The paper was extremely suggestive as to the way in which cretinism arose in a goitrous district. Cretinism would probably be first introduced from the outside by intermarriage with a stranger from a cretinous district. Whether the factor of thyroid deficiency existed concealed amongst the general population was another question, and one which it would be difficult to solve. One point was peculiar with regard to this inherited condition, if it was inherited, namely, that it required, in order to properly show itself, that the parents should be living in a goitrous district, and that the child should be born in such a district.

Dr. DAVID FORSYTH said that one of the most interesting points raised by the paper they had just heard was the influence of a goitrous mother on her children *in utero*, some of whom might be cretinous but others were healthy. The question had been referred to by Dr. Gossage, and it would be instructive to hear something further from the reader on that point. The experiments of Mr. Edmunds in this country and of Professor Halsted in America, who removed the thyroid from pregnant animals, had shown that thyroid secretion passed between the foetal and the maternal circulations. Neither mother nor child could monopolize the whole supply. If, therefore, cretinism was the result of a deficient secretion of maternal colloid we would expect that the goitrous mother would herself show signs of thyroid insufficiency during her pregnancy. Since the close connexion between goitre and cretinism had become more generally recognized, the pathology of cretinism, and therefore of myxoedema, had been brought into the domain of antenatal pathology and fresh light might be expected from an examination of the embryos and stillborn foetuses produced by goitrous mothers. He asked whether the author had had opportunities of ascertaining the number of stillborn children among goitrous women, and whether he had examined the thyroid glands in young foetuses. In the case which the author examined

microscopically no parathyroid gland was present. Had Captain McCarrison found any accessory thyroid glands, and, if so, did they show any microscopical changes? What was the condition of the brain and of the pituitary gland?

Mr. EDMUNDS said one point which had been brought out was that the cretinism came on in many of the children when they were 6 months old, which would be about the time when they were weaned. If the blood of the mother contained thyroid secretion, and the latter passed through the mammary gland, the child when weaned would lose that supply; therefore it was desirable that these children should be fed with cow's milk for a long time.

CAPTAIN MCCARRISON, in reply, said he thought shock had a very marked influence on the thyroid gland. He had known a strong, healthy young man develop a goitre purely from fright—there was no other reason. Mr. Rushton Parker had sent him a reprint of his article entitled "A Goitrous Cretin under Thyroid Extract," which appeared in the *British Medical Journal* of February 8, 1896. That illustrated certain points brought out in the present paper. The patient was born in November, 1877, the mother had goitre in 1870, and a neighbouring family presented three instances of the same complaint. The patient's mother, when three months pregnant with her, experienced a great shock from the wounding in the neck of one of her older girls by the accidental firing of a gun. There was no history of consanguinity, of disease in the mother, of prolonged labour, or of any of the other factors which had a determining influence on the production of the disease, and there were eighty cases in the present paper with a similar history. In this case also a goitre had appeared at the age of 13 years, or subsequently to the appearance of the cretinic symptoms. In answer to Mr. Berry, idiots were not included—they were always found largely in goitrous localities. But he included those who, with other symptoms of cretinism, might show practically no signs of myxœdema. Dr. Gossage had considered the subject from the point of view of heredity. In the series there were some instances of myxœdema or permanent thyroid defect in one or other of the parents, and in them hereditary transmission of the defect was possible, but the majority were cases in which the thyroid deficiency in the mother had originated during pregnancy, and in which it was only a temporary defect during that state. He did not think the question of heredity arose in those cases, but more detailed pedigrees were necessary. He had had no opportunities of ascertaining the number of stillborn children, and owing to the objection to operative interference and post-mortem examinations, he had not been able to dissect any cases; he was not allowed to open the skull in the case from which the section of the thyroid gland was shown. It had been objected that his application of Halsted's experiment was incorrect, that so far from the puppies of the thyroidless bitch being born with a congenitally defective thyroid they were born with "compensatory hypertrophy." He was of opinion that such hypertrophy was a step in the direction of incompetency, in much the same way as the compensatory hypertrophy of the heart was a forerunner of its incompetency. The hypertrophy caused by excessive functional activity was followed in time by atrophy.

Medical Section.

November 24, 1908.

Dr. SAMUEL GEE, President of the Section, in the Chair.

The Resistance of Arteries to External Pressure: Experiments on the Value of the Sphygmomanometer as a Test of the Blood-pressure.¹

By W. P. HERRINGHAM, M.D., and F. WOMACK, D.Sc.

A GREAT deal has been written of late on the measurement of blood-pressure, and a considerable amount of reliance is now placed upon the instrument recommended by Martin—an air-bag 10 cm. to 12 cm. wide, forming a band round the arm, and distended by a rubber air-pump in connexion with a mercury manometer. So far as we know, no direct experiments have been made to show the influence of the vessel wall in resisting the external pressure of the arm-bag and thereby increasing the mercury reading. Von Basch states that the resistance of the empty radial only varies between 1 mm. and 5 mm. of mercury. Potain does not seem to have experimented with arteries removed from the body, but he thinks the resistance of the wall is inconsiderable.

At the suggestion of Sir Clifford Allbutt, we began a series of experiments on arteries removed from dead male bodies. In various cases the common carotid, the external iliac, and the brachial were tested. The method adopted was the following:—

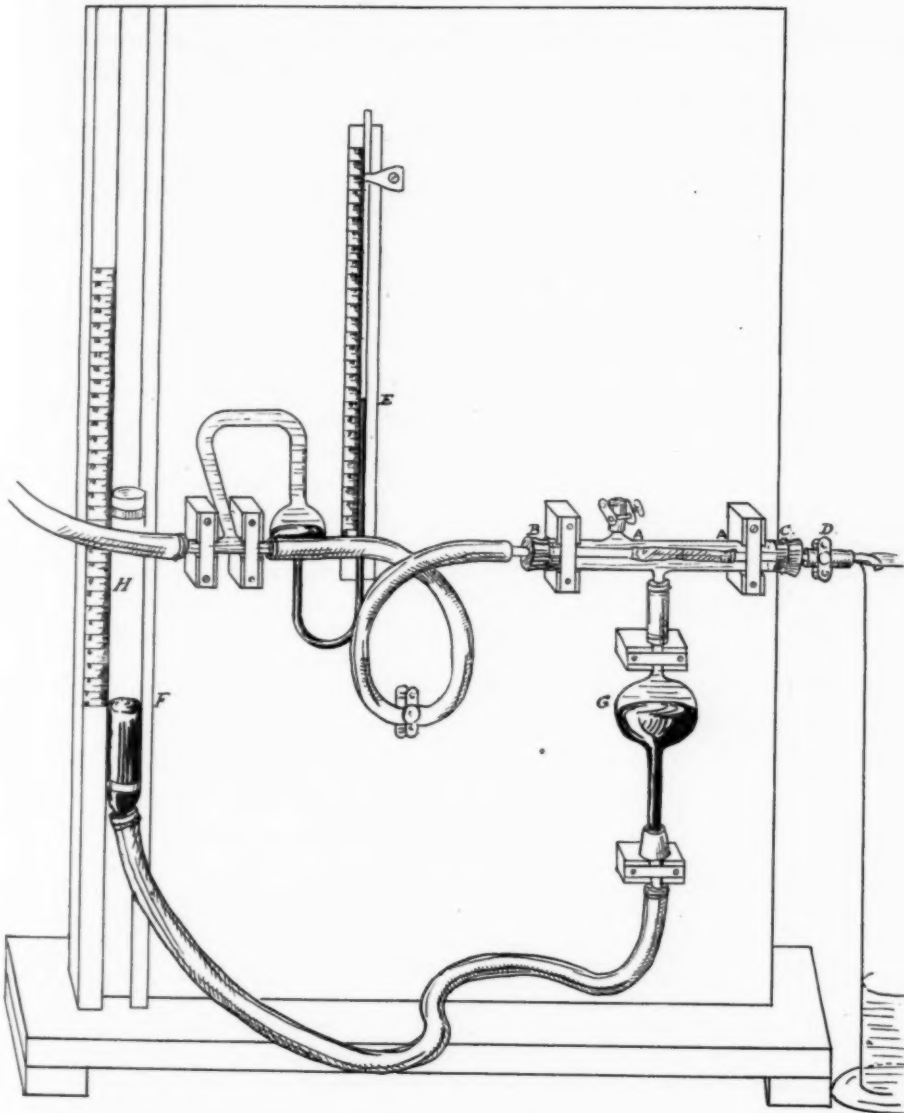
The artery *AA* (*see fig.*), about 2 in. to 3 in. in length, was tied at its ends to two pieces of glass tubing, and placed in the interior of an outer glass tube *BC*, held horizontally by two supports. Through the artery a stream of water was made to flow from a reservoir at a higher level (not shown in the sketch), a pinchcock being applied at the further end, *D*,

¹ From the Physics Laboratory of St. Bartholomew's Hospital.

of the glass tube, and tightened up so that the stream of water flowing through the artery issued slowly, drop by drop. (This pinchcock is unnecessary, and was afterwards omitted.) The internal pressure forcing the water through the artery was measured by means of a mercury gauge *E*, and was throughout the experiments maintained at 93 mm. of mercury, or 1,365 mm. of water, *i.e.*, about one-eighth of an atmosphere. (To this a small correction, 3 mm. of mercury, may be added, due to the narrowness of the tube *E*.) Pressure was then applied externally to the artery by means of the column of mercury *FG*, there being water above *G* in contact with the external surface of the artery. The height of the column at *F* is capable of variation by the glass tube sliding in a vertical groove. The external pressure was then steadily increased by raising *F* until the flow of water through the artery ceased; and the external pressure necessary to occlude the flow was read off on the vertical scale *H*. In the case of the brachial the small muscular branches given off had to be found and tied, so as to render the artery water-tight. The carotid and iliac have no such branches.

In the forty-nine cases on p. 40 we tested the brachial artery thirty-six times, and in five cases both brachials were tested. Nine cases were under 30 years of age, and seven of these were tested. In one both brachials were tested. The average obliterating pressure in six cases was 9.5 mm. Hg. One case (No. 8) of heart disease was exceptional, and required a pressure of 33 mm. If this be included the average was 12.5 mm. Eight cases were under 40 years of age, and five of these were tested. In one both brachials were tested. The average obliterating pressure was 18.8 mm. Thirteen cases were under 50 years of age, and ten of these were tested. In one both brachials were tested. The average obliterating pressure of ten arteries was 13.9 mm. The exceptional case (No. 27), a case of gastric ulcer, required a pressure of 31 mm. If this be included the average was 16.3 mm. Seven cases were under 60, and five of these were tested. In one both brachials were tested. The average obliterating pressure was 13.5 mm. Twelve cases were over 60, and nine of these were tested. In one both brachials were tested. The average obliterating pressure for nine arteries was 15.3 mm. The exceptional case (No. 42), a case of fractured spine, required a pressure of 34 mm. If this be included the average was 17.2 mm.

These cases seem to show—(a) that the resistance does not vary with age; (b) that at every age exceptional cases occur; (c) that the extremes vary greatly and irregularly; and (d) that the two brachials may differ by as much as 10 mm.



D-14a

The following is the table of cases and measurements:—

No.	Name	Age	Disease causing death	MILLIMETRES OF MERCURY REQUIRED TO OBLITERATE LUMEN OF		
				Carotid	Iliac	Brachial
1	H. P.	18	Chronic nephritis	18	4	4
2	S. L.	20	Cerebral abscess	25	11	19
3	H. D.	21	Tuberculous peritonitis	—	—	7, 11
4	J. W.	21	Phthisis	—	—	13
5	H. T.	28	Gastric ulcer	12	6	—
6	F. P.	28	Septic endocarditis	21	18	—
7	A. M.	28	Phthisis	15	18	13
8	W. A.	29	Heart disease	18	30	33
9	P. H.	29	Tuberculosis	—	—	6
10	J. S.	30	Septic endocarditis	—	—	8
11	A. B.	32	Phthisis	14	12	—
12	E. L.	32	Chronic nephritis	16	14	14
13	R. S.	33	Septicæmia	18	14	21
14	R. C.	33	Chronic nephritis, heart disease	16	26	12
15	C. S.	34	Fractured skull	25	4	—
16	G. C.	34	Tuberculosis	—	—	22, 12
17	W. C.	36	Aortic aneurysm	20	8	—
18	J. G.	40	Hepatic cirrhosis	23	15	13
19	J. C.	42	Pneumonia	13	11	7
20	R. P.	42	Fractured skull	17	4	17
21	H. H.	42	Cancer of pylorus	—	—	12
22	W. B.	43	Hepatic cirrhosis	14	18	—
23	F. E.	43	Cerebral hæmorrhage	24	12	9
24	A. W.	44	Hepatic cirrhosis	16	3	—
25	J. H.	44	Pneumonia	—	—	8
26	J. F.	46	Poisoning	8	—	10
27	W. W.	47	Gastric ulcer	47	—	31
28	A. D.	49	Pneumonia	16	11	—
29	F. E.	49	Lymphosarcoma	16	—	16
30	F. V.	49	Cancer of œsophagus	—	—	23, 24
31	W. N.	52	Peripheral neuritis	13	12	10
32	G. W.	52	Pneumonia	—	—	8, 13
33	J. D.	54	Cancer of pancreas	—	—	7
34	J. B.	55	Chronic nephritis	19	36	—
35	H. M.	56	Arterial sclerosis	19	20	—
36	C. T.	58	Cancer of pancreas	—	—	22
37	A. B.	59	Pneumonia	35	22	21
38	J. C.	61	Gastric ulcer	5	15	—
39	G. B.	61	Cerebral hæmorrhage	—	—	21
40	W. S.	61	Empyema	—	—	4
41	C. B.	63	Cardiac failure	—	—	22
42	J. H.	64	Fractured spine	39	36	34
43	C. P.	64	Collapse following accident	—	—	16
44	E. B.	66	Cancer of bladder	—	—	12, 17
45	C. K.	67	Fractured ribs	15	—	—
46	J. T.	67	Cut throat	47	56	—
47	J. C.	67	Chronic nephritis	—	—	24
48	W. O.	68	Cellulitis	22	18	10
49	J. E.	74	Chronic nephritis	—	—	12

We in many cases tested the carotids:—

Under 30 the average resistance was 18.1 mm. ; extremes 12 mm. and 25 mm.										
"	40	"	"	18.1	"	"	14	"	25	"
"	50	"	"	19.4	"	"	8	"	47	"
"	60	"	"	21.5	"	"	13	"	35	"
Over	60	"	"	25.6	"	"	5	"	47	"

These averages show a certain progression with age, but the extremes are so wide apart and so irregular that the average cannot be of much value.

In others we tested the external iliacs:—

Under 30 the average resistance was 18.5 mm. ; extremes 4 mm. and 30 mm.										
"	40	"	"	13.0	"	"	4	"	26	"
"	50	"	"	10.5	"	"	3	"	18	"
"	60	"	"	20.2	"	"	12	"	36	"
Over	60	"	"	31.2	"	"	15	"	56	"

These averages are quite irregular and the extremes very wide. Their numbers show that as regards these two arteries—(a) there is no gradual increase of resistance with age, and (b) that the carotid and iliac, though analogous in their relation to the aorta and usually of much the same calibre, do not vary together, nor either of them with the brachial. The same conclusion will appear if the cases are examined individually.

One of us happened to be at the same time examining the resistance of the carotid and external iliac to internal pressure. These experiments will be published separately, but in a few instances it is possible to compare the variation of the two resistances:—

CAROTIDS ARRANGED IN ORDER OF RESISTANCE TO EXTERNAL PRESSURE.

No. of case	Obliterating external pressure	Expansion under internal pressure of 250 mm. of mercury	No. of case	Obliterating external pressure	Expansion under internal pressure of 250 mm. of mercury
26	8 mm.	113	29	16 mm.	106
19	13 "	137	8	18 "	162
31	13 "	84	13	18 "	105
11	14 "	138	35	19 "	114
22	14 "	156	18	23 "	118
7	15 "	118	15	25 "	109
45	15 "	68	37	35 "	61
12	16 "	135	42	39 "	83
14	16 "	150	27	47 "	110
24	16 "	171	46	47 "	86
28	16 "	105			

It should be explained that if the two forms of resistance were correlated the expansion under internal pressure would vary inversely with the resistance to external pressure. A thin-walled or soft artery, easily obliterated, might be expected to dilate easily when pressure was

applied to its internal surface. Evidently no such relation exists. We need not give the numbers for the iliacs; the same result occurs.

It might, perhaps, be expected that the disease causing death would affect the condition of the artery. No such connexion can be made out in the present series. In another series of cases, in which Dr. Herringham and Dr. Wills tested the elasticity of the aorta,¹ it was found equally impossible to show any connexion between the fatal disease and the condition of the artery.

To sum up, then, it appears that when using the sphygmomanometer we must bear in mind that the resistance of the wall of the brachial artery when removed from the body may vary from 4 mm. to 34 mm. of mercury, that the readings of the instrument represent the sum of the blood-pressure together with the resistance of the artery, and that if the resistance of the artery when out of the body corresponds at all to its resistance during life, we have as yet no clue which will enable us to analyse this total into its two component parts.

DISCUSSION.

Mr. LEONARD HILL, F.R.S., said that recently, at the London Hospital, Mr. Martin Flack and he had been carrying out a series of experiments on this subject, but by a method which they applied directly to the arteries of living man. He believed Dr. Charles Martin had carried out experiments by the same method as that of the authors, and found that the amount of pressure required to obliterate the wall was only a few millimetres of mercury. He (Mr. Hill) had carried out some experiments of the same kind, and so had Dr. Otto Grünbaum. Professor MacWilliam had carried out some very important and valuable experiments, which the authors of the present paper were perhaps not aware of: they were published some years ago in the *Transactions of the Royal Society*. These experiments were on arteries of bullocks and sheep obtained from the butcher. If such an artery were irritated by being beaten with the handle of a scalpel, or rubbed between the finger and thumb, it would greatly contract, and, if frozen, it relaxed. On comparing the carotid artery of one side which had been frozen with that on the other side which had been beaten with a scalpel the results showed an extraordinary difference. Mr. Hill demonstrated two pieces of the same carotid artery; one of them was 5 mm. or 6 mm. in diameter, and the other had contracted down to 2 mm. It seemed as if some pressure would be required to obliterate the wall of the contracted piece, while the other needed very little. But that was a post-mortem difference produced by irritation. The same sort of contraction occurred when an artery

¹ *Trans. Med. Chir. Soc.*, 1904, lxxxvii, p. 489.

was torn or wounded. One had no right to assume that that kind of contraction occurred in the living artery in the body, through which blood was flowing in the normal way. Therefore, soon after he began experiments with post-mortem arteries he gave it up, and devised a means of applying his tests to living arteries. He would be glad to have the opportunity of demonstrating his methods on his laboratory assistant. In the first method two armlets were taken and one placed on each arm. The manometers were independent of one another, and the obliteration pressure had to be read simultaneously by two persons. One arm was held up and the other down, and the distance between the top of the armlets was measured. The two blood-pressures were then taken. It must be done simultaneously, with skilled observers at each wrist. The slightest psychical disturbance made considerable differences to the readings; even the turning of the head of the subject to look at the manometer might make a difference of 10 mm. They took the obliteration pressure of the two, and the difference between them came out equal to the gravity difference of the column of blood which separated the two arteries. It was well known that an artery contracted when the pressure was raised in it and dilated when the pressure was lowered, therefore one artery ought to be more contracted than the other. If the artery wall came significantly into the reading it was very strange that the gravity difference should always come out just about right, *i.e.*, within 1 mm. or 2 mm. of mercury. While not conclusive, it was suggestive that the arterial wall did not come into play. In the second method they found out what the obliteration pressure of the brachial artery was—suppose it were 200 mm. of mercury; the pressure in the armlet was then lowered down to 195 mm., so that the blood could just get through and distend the veins. One hundred and ninety-five millimetres of mercury was then the blocking pressure, and the blood could not get through the veins out of the arm until it exceeded that pressure. A second armlet already placed round the forearm was raised up to over 200 mm. Hg. and a small vein was selected above the armlet—small, because its valves were more efficient than those in a large vein. After stroking the vein and emptying it upwards, the pressure was quickly relaxed in the second armlet until the vein filled. It was found the vein filled at 195 mm. The venous pressure reached the blocking pressure if time enough was allowed for it to do so. Now the vein was quite a superficial thing, and no one would say that the vein could support any material part of the pressure, and thus the proof was obtained that the arterial obliteration pressure was correct within 5 mm. Hg. It was difficult to get patients with high pressures to stand this test as the blocking pressure caused so much discomfort. They had done the first, or gravity, method on a man whose systolic pressure was 232 mm., and the method came out perfectly on him. They could not do the second method on him because all his venous valves leaked backwards. They had done both methods on a man whose arteries were very much degenerated indeed—they were coated with large calcareous plaques; and by both the gravity method and the venous method the arterial pressure was proved correct. The highest pressure they had tested

by the second method and found correct was 190 mm. Hg. Another interesting point was that, when one had an arterial pressure, say, of 150 mm., and the veins blocked by 145 mm., if one pushed a hollow needle underneath the tissues of the arm and connected that with a glass tube filled with water, but containing an air-bubble index, which in its turn was connected with a mercurial manometer and a pressure bottle, they found the pressure required to just drive the liquid in under the skin and overcome what they took to be the capillary pressure was very low. For example, the liquid could be got in by a pressure of 45 mm. H_2O when there was a pressure of 900 mm. of water in the artery and vein. The explanation was that there were wide passages between the arteries and veins in certain highly vascular parts through which the veins filled up, while the capillaries in other large areas got little blood owing to vaso-constriction, and distended very slowly when the arm was obstructed by a bandage. There was a condition of very high pressure in a vein, very high pressure in an artery, and very low pressure indeed in the capillary system. That, physically, seemed at first sight impossible, but physiologically it was explicable by reason of wide anastomoses between arteries and veins which histologists recognized, vaso-constriction of the arteries and the valves in the veins. The continual change of posture and muscular movement, which was a feature in one's daily habits, prevented the capillary pressure rising. Immediately the pressure in any part became uncomfortable, that part was moved. The muscles had as large a share as the heart, if not larger, in maintaining the circulation.

Dr. GEORGE OLIVER said he thought the Section was as much indebted to Mr. Leonard Hill for his demonstration as it was to the authors of the paper, and doubtless those interested in scientific methods would appreciate that demonstration. There were one or two points in connexion with the method of observing and measuring the compressibility of excised arteries which occurred to him as likely to somewhat modify the results recorded in the paper. In practice one compressed 5 in. of artery, and arteries were not of uniform texture throughout such a length, the differences not being, moreover, so evident to the eye or the finger as to such a delicate instrumental test as the present one. The weakest part of the wall would give first, and he thought a longer portion of artery would be more likely to furnish such less resistant areas than a shorter one, and would therefore yield a lower range of variation for closure than the latter. He believed that this was one reason, if not the main reason, why armlets of less than the standard depth furnished higher readings of the arterial blood-pressure; he therefore held that the wide armlet now in general use not only reduced the disturbing effect of the arterial wall on the readings of the blood-pressure in all cases, but also diminished variation due to differences in the walls of individual arteries. Again, he did not think that the closing of an artery was so much a question of overcoming the general resistance of the wall as to occlude crinklings and resistant foldings. He therefore thought the final stage of closure might require much more compression than the earlier, and the extra pressure required to close up the irregularities might account for the

observed differences between the two forms of resistance—internal and external. During the past few years he had been working a good deal on the variations of blood-pressure furnished by the armlet applied to different areas, *e.g.*, the two arms, the forearm, and arm, and he had observed differences such as 10 mm., 15 mm., or 20 mm., or more, which he could only ascribe to variations in compression required to close individual arteries. His clinical observations, therefore, in a general way supported the results observed by the authors of the paper. He had observed that the differences were more marked in cases of atheroma and arteriosclerosis than in the subjects in whom arterial thickening was not apparent. In making these observations he, of course, took the greatest care to exclude the psychological element, and he regarded the different readings obtained as affording internal evidence of the disturbing effect of the arterial wall.

Sir LAUDER BRUNTON, F.R.S., said that the subject of the blood-pressure in man was of such extreme importance that anything which would help towards its more accurate understanding must be very welcome to every physician. He thought the Section was much obliged to Dr. Herringham for having brought the question forward. He (Sir Lauder) had only made one experiment in the same way as the author had done, and that he did in the laboratory of Professor Kronecker, in Berne. He took the artery from a freshly slaughtered animal and found that the amount of pressure required to obliterate it over and above the pressure inside the artery was not above 10 mm. of mercury. One thing which struck him about the experiments of the authors was the great differences in the arteries where one would not naturally expect those differences; and that led one to ask the question whether those arteries were all in the same condition of rigor mortis. On one occasion he went down to Cheapside and had a steak; it was as tough as leather, so that he could scarcely masticate it. He asked the waiter why it was, and the reply was that they had run out of their stock of beefsteak and had been obliged to send to Smithfield for some. The explanation was that in the beefsteaks ordinarily supplied rigor mortis had passed away, while in the one he ate it had not passed away. That made all the difference between a tender beefsteak and one which was practically uneatable. It seemed to him possible that differences in the amount of rigor mortis in the muscular coat of those arteries experimented upon might have had something to do with the difference in the pressure required to obliterate them. He thought that question was worth attending to, but he was sure that there was a difference in the living artery also, because he knew that in his own temporal arteries, which he was able to feel under the finger, the arteries varied enormously, being sometimes twice as wide as the normal, whilst at other parts they contracted almost to a thread when he had severe migraine. Those variations occurred in a less space than 5 in. He had little doubt that if there were alterations in the contraction of an artery and consequent variations in its calibre there would be differences in the amount of pressure required to obliterate it. He did not doubt that Dr. Oliver's experiments were quite correct. He had himself tested the blood-pressure in a number of patients,

both with Potain's instrument and with the Riva Rocci instrument, and with the band over the arm, and he had found considerable differences in the pressure as taken at the wrist and that taken in the arm; and he had found differences of at least 20 mm. in the two arms taken at the same time. Thus his observations corroborated those of Dr. Oliver. He had made another curious observation, namely, that in the same arm one did not always find the same amount of pressure at different times during the observation. He had raised the pressure in the armlet and found that a certain amount of pressure was required to obliterate the artery; but he had found that sometimes the pressure required to raise the pulse again was not, as in Mr. Hill's experiment, only 5 mm. below what was necessary to close the artery, but was as much as 30 mm. below. He had found that if it required 150 mm. to stop the pulse at the wrist when pressure was first applied over the arm, a lower pressure seemed sufficient to abolish the pulse at the wrist a second time just as the effects of the first were passing off. He thought this might be due to contraction of the artery of the arm induced by the first pressure, and which aided the external pressure in extinguishing the pulse. He agreed with the conclusions of the authors that the readings of the instrument represented the sum of the blood-pressure, together with the resistance of the artery; but he could not agree that they had no clue which would enable them to analyse or separate that total into its two component parts, because he thought it could be analysed. He was accustomed to take the pressure in the two arms, and to take the lowest pressure in the arms, because one could not obtain with the instrument a pressure over the pressure in the artery. If one took the lowest pressure one was sure to be getting nearly the pressure which existed in the artery. Another method had suggested itself to him, namely, that one might also take the pressure in the finger by Gaertner's tonometer, and if that were done along with the other reading one was pretty sure to avoid error. Another method was that described by Mr. Hill, namely, having a differential manometer. For clinical work it was out of the question to have two skilled observers, and he thought it should be easy to arrange a differential manometer, so that one observer could record exactly the difference between the pressures on the two armlets; and that might be a practical way of obtaining that exact estimation of pressure which Mr. Hill had found by means of having two observers. He thought the Section must feel very grateful to the authors for their paper.

Mr. P. LOCKHART MUMMERY said he had done some experiments with the object of ascertaining how accurately the ordinary sphygmomanometer with a broad cuff worked as compared with the manometer tied in the artery in registering arterial blood-pressure. The animals used for the purpose were large dogs, when they could be procured. The cuff was fixed round the animal's thigh over the femoral artery on the left side, and the femoral artery on the right side was dissected out and connected by an ordinary cannula to a mercurial manometer. The sphygmomanometer cuff was connected with the Riva Rocci manometer, the two instruments having previously been tested, so that they were known to be accurate. The artery down in the leg on the left side

was then exposed so that one could be certain of the exact point at which the pulse was obliterated. A screen was placed round the manometer so that the person taking the observation could not, even unconsciously, be biased by seeing the other reading. One person was observing the pressures on the manometer connected with the artery and another took observations with the sphygmomanometer. What was found surprised him very much; the readings between the two manometers were so extraordinarily accurate that they never varied by more than a few millimetres of mercury, except once, when it was subsequently found that the cuff had slipped. For more than ten observations there was not a variation of more than 2 mm. The conclusion came to, as a result of a considerable number of observations, was that if the instruments were properly constructed and the cuff properly adapted to the limb, the sphygmomanometer gave the actual pressure in the artery correctly. The only error which they could ascertain was that due to personal observation; it was obvious that by feeling a pulse one could not get as accurate a record as by watching the manometer connected to the interior of a vessel. They could not find any error which was apparently due to resistance in the artery wall. The variations that did occur between the readings in the two manometers occurred in both directions. That is to say, sometimes the sphygmomanometer reading was the higher and sometimes the arterial manometer reading. It was obvious that this could not be the case if the error was due to resistance of the arterial wall, and this was a very strong argument against the arterial wall having any effect upon the readings. The figures obtained in these experiments were as follows: sphygmomanometer readings—108, 107, 107, 107, 110, 110, 84, 65; arterial manometer readings—110, 106, 106, 108, 108, 110, 86, 64. Dogs of various ages were used, some of which appeared to have very thick arterial walls, yet there was nothing to show a pressure variation caused by the arterial wall. With regard to Dr. Herringham's and Dr. Womack's experiments, he could not help thinking that it was dangerous to draw conclusions from arteries which had been removed from the body and treated in the way those gentlemen had treated them. In face of Mr. Leonard Hill's methods, which proved that in a person with considerable arterial sclerosis the pressure was accurate within 5 mm., he could not think that arterial sclerosis or any form of thickening of the artery could possibly make any error of a serious account in the reading, because as long as the sphygmomanometer would work and give a pressure which was within 5 mm. of mercury, that gave as near a reading as could be obtained by other clinical instruments, such as the thermometer.

Sir CLIFFORD ALLBUTT, F.R.S., said, in response to the President's invitation, that he had come to learn, and had not intended to speak. As a clinical observer he had found that those problems were so difficult that he was seeking for assistance, and was pleased when Dr. Herringham was good enough to undertake these experiments. He often took blood-pressures in both arms together; or in the case of people coming frequently, first in one arm and then in another, as might be convenient. It was not his experience that *ceteris paribus* there was much difference; but he never pretended to accuracy of

record within 15 mm. Taking the question of a thickened artery, if in the area of one arm an artery contracted, and in this way became thicker, it might be that the artery might thus offer more resistance to the closing of it by pressure; yet this contraction in an area comparatively so small would signify a contraction down upon a less volume of content and would signify dilatation in other areas; thus the blood going into the arm would be at lower pressure, and this fall would be more than any thickened coat could amount to. The two opposite factors could not be discriminated in the total. If, however, the contraction of the artery were part of a rise of blood-pressure, the rise must be over the two arms together, and a much larger area still, involving far more than the two arms, and again would override any increase attributable to thickening of coats. What Mr. Leonard Hill said about the perpetual oscillation of the blood-pressure had struck him very strongly in private work; the pressures were always in a swim; and this made it very difficult to get true readings. For instance, a lady who had consulted him from time to time for some years, and who had a very intelligent physician at home, showed such variations very widely. Her pressures had been formerly 220 to 230, but under venesection, which had been systematically used for two or three years, her pressure had fallen substantially, so that when quietly at home her physician got exceedingly interesting evidence of reductions to 150 or so; but when the patient was at his (Sir Clifford Allbutt's) house and in her chair awaiting the record for better or for worse, up went the pressure to about 200, and he could not get it down within any time that could be given. Even in the laboratory assistants, who were accustomed to experiments and regarded them with apparent indifference, when put on to a couch and the blood-pressure taken, Dr. Dixon had shown that it followed a descending gradient for about twenty minutes, until, that is, the interest and attention had flagged. How, then, were they to make estimates of this kind with any accuracy in the course of consultation? Some patients were more excited in the consulting-room, others less so. For such reasons as these he never attached much weight to precise figures, and was usually content with approximate estimates. With regard to Dr. MacWilliam's valuable help in those matters, when he (Sir Clifford) was working at arteries with Professor Roy, they arrived at certain well-known conclusions. Then Professor MacWilliam fired a bomb into the middle of their observations by comparing post-mortem arteries under variable conditions of rigor mortis, mode of death, and so on. From that time Dr. MacWilliam had kindly written to him occasionally on the subject, and Dr. Herringham also had taken him into his confidence; and the present attitude seemed to be that the conditions of an artery outside the body were subject to so many variables that Dr. MacWilliam did not encourage Dr. Herringham and himself to rely too much upon these results. It was not merely a question of the age of the patient, but of the particular time at which he died, and indeed of the disease of which he died. And he believed Mr. Hill also was disposed to insist upon such possible fallacies. His own impression, after many years of clinical experience, was that, within the limits possible in practice, the arterial coats

counted for very little in pressure records. For instance, in the class of arterial sclerosis which he had named "decreascent," where there was not and had not been high pressure, one would find the blood-pressure low enough for the time of life. On the other hand, one would find a man with very much better arteries, but of the class he had named "hyperpietic," in which the pressures were persistently very excessive. He did not think, therefore, the arterial coats entered for much into these problems. Dr. Herringham had been kind enough to send him the arteries, or portions of them, from all the cases tested for his paper, and they were being cut in the Cambridge laboratories; so that, as Dr. Herringham desired, they would be investigated independently of the pressure records, and conclusions drawn by subsequent comparison of the schedules.

Dr. HERRINGHAM, in reply, said that during the discussion various new lights had been thrown upon the question. Professor MacWilliam's work was well known to him, and he had quoted it in his previous work on the aorta. The reason he thought he was safe in regard to the present arteries was that aortæ, similarly taken from the post-mortem room and tested by the character of the curve of extension laid down by MacWilliam, showed no remaining contractility or rigidity. But in an article which appeared ten days ago in the *British Medical Journal* Professor MacWilliam stated that, in smaller arteries similarly collected contractility occasionally persisted. The arteries which Mr. Leonard Hill spoke of were not to the point, as they were cut from an animal immediately after death—fresh arteries. What he now wanted to do was to take more post-mortem arteries and make sure that he had removed contraction and rigidity by relaxing them in sodium fluoride, as MacWilliam suggested, and try again. But other experiments had been quoted which, if they were accurate, seemed to do away with the pressure of the artery as a factor. He spoke of the contribution of Mr. Mummery, which was the most important thing they had heard that evening. It seemed to be quite inexplicable, except on the basis that the artery had no effect. But he could not understand that, and at the same time receive the clinical results given by Dr. Oliver, who was a leading authority, and those of Sir Lauder Brunton. Though he did not want to say his experiments were right or those of others wrong, he was of opinion that it was necessary to continue the investigation still further. With regard to Mr. Leonard Hill's first experiment concerning the different levels, he did not think that conveyed any information as to the point in question. He did not understand the second experiment; before he attempted to criticize it he must think it carefully over.

**On an Excitant for the Leucocytes of Healthy Persons found
in the Blood-plasma of Patients suffering from Carcinoma.**

By CHARLES MACALISTER, M.D., and HUGH C. ROSS.

THERE are some diseases which present constant pathological changes in one or another organ of the body to which most of the symptoms, whether subjective or objective, are referred, and the name by which the disorder is known is often associated with the lesion which chiefly characterizes it. In gastric ulcer, for instance, we have a very definite and constant morbid condition which gives rise to well-known clinical signs, and yet, as was pointed out by one of us some three years ago,¹ there are reasons for believing that the ulcer is the result of a more general systemic condition, often presenting prodromal as well as collateral symptoms which are not in any way connected with the stomach. And so in the case of cancer we have a disease called by a name derived from the character of a local growth which causes pain, and distress, and mechanical difficulties varying in degree according to the part which happens to be attacked, and these throw into the shade some less prominent general symptoms which may in the course of time prove to be the key to the situation and explain much that is at present problematical.

We wish to direct attention to the possibility that cancer is the local manifestation of a disease which is dependent upon changes in the blood, difficult to account for, but yet so constant that their association with the neoplasm cannot be disregarded. The histology of the carcinomata has unfortunately failed to throw much light on the causes of the new growth, and, so far as we are able to tell at present, the changes which have been described in the cells in the way of the asymmetrical mitoses, &c., may be regarded rather as effects than as causes; indeed, the very essence of the explanation which is being sought after in every cancer research is the reason why these hitherto normal epithelial structures adopt this reproductive or vegetative character and give rise to cancer formation. We do not gain much help by considering the localities which are mainly affected by carcinomata. There are those who still hold to the teachings of Cohnheim and Virchow, that the primary growth

¹ *Liverpool Med.-Chir. Journ.*, 1905, xxv, p. 112.

results from an embryonic tissue-inclusion which, after a long period of quiescence, undertakes active growth. To accept such a view shuts out the hope of preventing or of curing cancer otherwise than by surgical procedure, but fortunately there are strong reasons for believing that in the case of the carcinomata at all events this explanation is incorrect. There is just one point in this relation, however, which is worthy of remark, namely, the fact that the growths are most apt to occur in organs which are tidal in character, such as the breast and uterus, and in parts also which are subjected to irritation, whether by the passage of normal contents, as happens at the narrower parts and flexures of the alimentary canal, or by foreign sources of irritation, and we must not forget the cases of malignancy which follow, sometimes immediately, sometimes remotely, after traumatism.

Here we have factors which have been explained in general terms as being the result of nutritional changes in the cells following upon irritation, but they do not tell us why only a fractional proportion of some large number of such cases becomes affected by cancer whereas all the rest escape, and this circumstance, we think, in itself suggests that some special condition of the blood must exist to start the grave morbid process in those people who become the subjects of the disease. We have to remember how selective the bodily tissues are with reference to the elements which they derive from the blood. The kidney draws through its blood-vessels much more blood than is required for its nutrition when the amount of waste products present stimulates it for its removal. The breast and uterus at the menstrual periods become turgid, and their contained blood is no doubt rich in some material which stimulates their activity. Professor Starling has described how an extract obtained by crushing foetal rabbits, when added to the blood of a non-pregnant doe, causes the breasts to enlarge and lactation to take place;¹ another evidence of the chemical character of a stimulus and of the selective power of a gland.

If we pass from the normal to the abnormal, we must observe the fact that a part which is chronically irritated or inflamed or which has been injured, like the physiologically turgid breast or uterus, contains much more blood than is required for its normal nutrition, and that the vascular conditions of an injured or irritated part are different from those elsewhere owing to the processes of repair requiring some special nutrition. If, then, a general blood-condition is present which one

¹ Croonian Lecture, 1905, *Lancet*, 1905, ii, p. 582.

might suppose to characterize a cancerous tendency or diathesis, it becomes quite within the realms of feasibility that an irritated part or a part undergoing repair, each of which is drawing towards or through itself a blood containing special essentials (a normal reproductive blood, so to speak), might by its selective influence derive from the blood those abnormal elements which, if present, promote abnormal reproductive changes instead of those of normal repair.

One is reminded of the well-known experiment of Loeb, showing how the place of the normal organic stimulus to cellular activity may be taken by a chemical one in the case of the unfertilized eggs of the sea urchin, and of the further observations made by Benjamin Moore, Roaf, and Whitley, proving that in the fertilized eggs of the Echinoderms both the rapidity and the regularity of the growth of the cells are greatly influenced by alterations in the alkalinity of the sea water in which they normally segment and proceed to the development of embryos.¹

The hereditary or constitutional tendency to cancer which no doubt exists in some families is much more easily explained on this chemical hypothesis than by any cellular theory, and it seems much more likely that it will turn out to be a disease of metabolism, or one depending upon some altered blood-condition for which there may be a family predisposition. Idiosyncrasy plays an important part in the production of these types of disorder, and it is more than possible that the causal factors which originate the trouble in one person or family may be impotent in others, so that an immunity may be said to exist in this as in some other diseases which are not known to be infective in the proper sense of the term.

An interesting point in connexion with the cause of cancer is the frequency with which some mental shock immediately precedes its incidence. In one of the very first cases which we investigated in connexion with the blood-changes which we shall shortly describe, the growth in the breast speedily followed the grief occasioned by the sudden death of the patient's husband. There are many such cases recorded, and Paget directed attention² to the mental antecedents to the onset of the disease, found in "deep anxiety, deferred hope, or disappointment." We all know how other metabolic or blood-conditions occasionally arise in the same way; for example, we might cite certain cases of diabetes or of Graves' disease, or of those cases of general interference with nutrition which cannot be well explained in the present

¹ *Proc. Roy. Soc. Lond.*, B, 1905, lxxvii, p. 102.

² "Lectures on Surgical Pathology," 1863, p. 780.

state of our knowledge other than by their being due to nervous influences which occasion an altered bodily chemistry.

Similarly, certain diseases of the influenza type have been thought to be the precursors of carcinomatous growths, and it is remarkable how frequently they have arisen in cases of rheumatoid arthritis, and thus we have abundant suggestiveness that conditions affecting the chemistry of the body may give rise to a predisposition which results in the onset of the malady we are now considering.

There is one more very important pathological reference to be made, indicative that blood-changes are constant in cancer cases, namely, the diminished acid-secreting function of the stomach which is present, not only when the disease attacks the stomach but, as Professor Moore has shown, in every case of cancer "wherever the growth may occur in the body." It is not necessary to go into the elaborate explanations of this fact which are set forth in Professor Moore's papers,¹ communicated to the Royal Society, and elsewhere. Suffice it to say that he concluded that "the absence of acid- or reduction of acid-secreting power seen in cases of cancer of all regions points to the change being due to a change in the plasma and lymph"; "that the alkalinity of the inorganic constituents of the plasma is increased in cancer"; that "increase in alkalinity up to a given degree leads to increase of cell growth and pathological nuclear divisions in organisms which are unable to protect themselves against artificially increased alkalinity," and that "irritation and inflammation are accompanied by increased alkalinity of the lymph, which is a factor in stimulating cell growth, and when long continued may lead to the excessive nuclear division and cell growth of malignancy."

Such, then, are some of the facts which direct attention to the blood as being possibly the primary part affected in cancer cases, and no doubt workers in chemical pathology will be able to report later on some interesting facts as to the early chemical changes which may be discovered leading up to the local outbreak. There are some clinical facts, however, which are worthy of note, be they cause or effect of the carcinoma, to which we desire to direct attention. In the Liverpool Home for Incurables and in the Royal Southern Hospital, where so many opportunities of watching these cases have presented themselves, the presence of the disease has almost always been well established when the patients have come under observation, so that it has not been possible to note any early constitutional changes, if such exist. In the

¹ *Proc. Roy. Soc. Lond.*, B 1905, lxxvi, p 138.

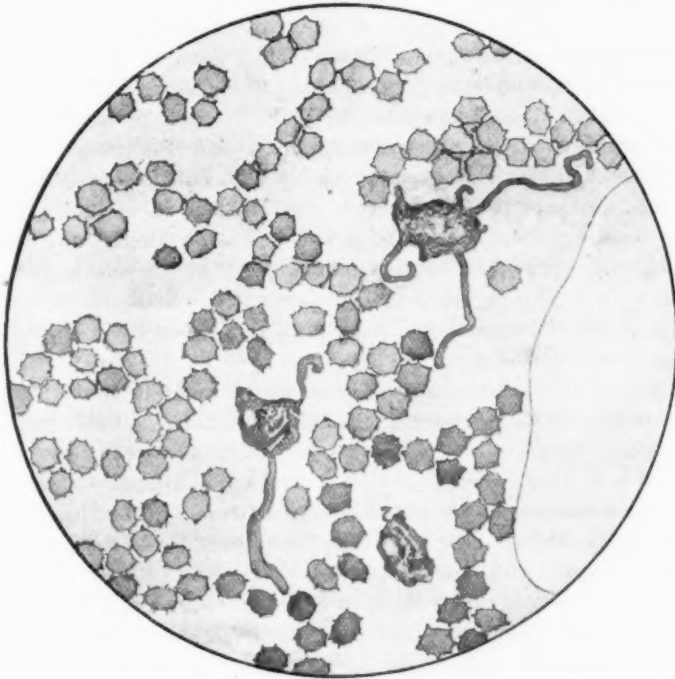
course of the cancer, however, no matter where it may be situated, it has often been remarked that the sufferers have presented a toxæmic appearance. Mental apathy, amounting in some cases to slight stupor and even mild delirium, has been noted, and these conditions quite unrelated to the use of narcotics. These symptoms and their variants, in fact, constitute part of the cancerous cachexia. On more than one occasion the very words used when referring to this state have been "that the patients have looked as though suffering from an alkaloid-like toxæmia."

One of us collaborating in this paper (H. C. Ross) has made extensive observations, extending over a period of three years, concerning the diffusion of stain into living leucocytes. After ascertaining that the cytoplasm of leucocytes exhibits jelly-like properties, and knowing that the rapidity of diffusion of hæmoglobin into agar jelly is hastened by the presence of *heat* and *alkalies*, he made experiments¹ which demonstrated that the diffusion of methylene blue into leucocytes is influenced by the same two factors. He then turned his attention to the diffusion of substances other than stain into the cells, experimenting especially with alkaloids, and he ascertained that if the cells are resting on jelly (agar), which contains besides neutralized sodium citrate and sodium chloride a certain proportion of methylene blue and atropine sulphate, the leucocytes will exhibit exaggerated amœboid movements, consisting in the extrusion and retraction of long curling pseudopodia (*see fig.*). The stain was found to be the important constituent of this excitant. If the jelly is neutral the excitation will occur without the addition of the alkaloid, although it is not constant. If, however, an alkali, such as sodium bicarbonate, is added, in amount proportionate to the temperature of the room at which the experiment is made, so as to produce diffusion of the stain into the cells, very marked and constant excitation takes place, *only provided the alkaloid is present*; whereas, if no alkaloid is present, no excitation takes place on the alkaline jelly. In other words, the *alkaloid is an essential factor* for the production of excitation on an alkaline stain-containing jelly.

This fact having been practically demonstrated, a conversation took place between us concerning the alkaloid-like toxæmic symptoms above referred to in cases of cancer, and one of us (C. J. Macalister) suggested that there might be in the blood of cancer patients some substance which, in the presence of the alkaline plasma, acted as a stimulant to the

¹ *Journ. of Physiol.*, Camb. and Lond., 1908, xxxvii, p. 327.

normal cells, thereby in some way giving rise to the new growth. Attention was directed to the fact that a patient was then in the wards (under the care of C. J. Macalister) suffering from carcinoma of the pylorus, who, not taking any narcotics, presented toxæmic symptoms resembling those which might be produced by an alkaloidal poison. We together saw the man and were struck by the fact that he was mentally dull and his pupils contracted.



Drawn by P. Nairn from a slide which had been kept at a room temperature of 20° C. for half an hour. The slide was prepared from the contents of a capillary tube which had been incubated for half an hour, and the tube contained 1 volume of a healthy person's blood and 8 volumes of the citrated plasma of a patient suffering from cancer.

One of us (H. C. Ross) had previous to this been making some measurements of the lives of healthy leucocytes when bathed in the plasma of different persons (the method employed will be published shortly). He had ascertained that the plasma of persons known to be

suffering from carcinomata did not comparatively shorten the lives of the leucocytes of healthy persons, in contradistinction to the effects produced by the plasma of persons suffering from several infective diseases, this fact indicating that cancer is not an infective disease. This was an important point to eliminate before proceeding to discover whether, on the other hand, anything in the nature of an excitant existed in the blood of cancer patients.

Consequent upon the clinical fact which we have referred to, and since we had observed the effect of the artificial excitant on leucocytes, we determined to ascertain whether the plasma of cancer patients contained any excitant pathological in character for the leucocytes of healthy persons, the following being the procedure adopted: The patient's citrated blood was centrifugalized and the citrated plasma mixed with one-eighth of its volume of the blood of a healthy person. The whole was kept at a temperature of 37° C. for half an hour and then a drop no larger than a pin's head was placed on a slide and covered, and examined at 20° C. Within half an hour after removal from the incubator some of the leucocytes showed the remarkable stimulated movements which were more marked than those seen when the artificial atropine excitant was used.

Since leucocytes mixed with the citrated plasma of healthy persons had been examined a very large number of times in the course of earlier researches, when exaggerated movements were never seen, it appeared as if there actually was a pathological excitant in the blood of this cancer patient as suggested by one of us (C. J. Macalister). The matter was therefore put to further test, nine other cases of cancer affecting various situations (breast three, uterus one, sigmoid one, pylorus two, jaw one, rectum one; and since this paper was written a very large number of cancer cases in which the disease affected various situations) being similarly investigated, and the interesting fact was elicited that every one of them responded in the same way.

In spite of the circumstance that the plasmata of large numbers of persons not suffering from cancer had already been examined without any sign of an excitant being present, fifty special control experiments were now conducted, the plasmata being taken from house surgeons, nurses, and hospital patients, both medical and surgical (cancer patients being alone avoided). In two only of these did the plasma act as an excitant to healthy leucocytes. Both of these were seamen suffering from malaria, who had recently returned from the West Coast of Africa, and they had both been taking large doses of hydrochloride of quinine.

Since it was known to us that the hydrochloride of quinine will act as a substitute for atropine sulphate in the artificial excitant and cause stimulation of leucocytes in the presence of an alkali, it was resolved to test whether the quinine in the blood of the two sailors had occasioned the stimulation. Thus, one of us gave one patient out of a series in a ward three doses of hydrochloride of quinine, the other being afterwards asked to examine the bloods of the whole series; and he correctly singled out the patient who had taken the alkaloid by the presence of the excitant in his plasma.

Although the number of patients suffering from carcinoma in whose plasma we have found the presence of this excitant for healthy leucocytes is comparatively small, it has been present in all of them, and it would appear as if the suggestion made by one of us is correct, in view of the number of control experiments which we have carried out with negative results. In fact, it is reasonable to suppose that there may be in these cases some substance, resembling an alkaloid in its action, which in the presence of the alkaline plasma excites the amoeboid movements of healthy leucocytes.

We desire to direct attention to the fact that methylene blue is a coal-tar derivative, and it has been found by one of us that both pyridin and quinolin, also coal-tar derivatives, can take the place of the alkaloid and cause stimulation. This point is the more interesting when we consider the frequency with which workers in coal tar suffer from cancer.

Since writing the above our attention has been directed to the interesting pathological mitoses (described by O. and R. Hertwig and Galleotti) induced by quinine, chloral, nicotine, antipyrine, cocaine, and potassium iodide, and to the statement of Galleotti that the asymmetrical mitoses produced in the epithelial cells of salamanders by treatment with dilute solutions of antipyrine, cocaine, or quinine are exactly like those seen in carcinoma.

These observations tend to confirm the possibility that the excitant which we have found to be so constantly present in patients suffering from cancer, and which reacts like the alkaloids with which we have experimented in the artificial excitant, may play some part in the production of the asymmetrical mitoses found in carcinomata.

One case of sarcoma and one of lymphadenoma have been investigated by us, and no excitant for healthy leucocytes has been found in either of their plasmata.

As an addendum to what we have said, may we refer to the possibility

that this reaction may prove serviceable as an aid to diagnosis? The pitfalls of research are numerous, and we desire to point to this with all caution in view of the circumstance that, although we have examined the bloods of patients suffering from numerous other diseases, there may yet be some in which the plasmata have this excitant effect upon the leucocytes of a healthy person. The fact remains, however, that medical men have sent us doubtful cases to investigate. Some have given a negative reaction and have afterwards proved to be innocent growths or no growths at all, whereas in other cases the presence of cancer, indicated by a positive reaction, has been confirmed by the subsequent examination of the tumour or by the history of the case.

DISCUSSION.

Mr. ROBERT JONES (Liverpool) said there was a possibility of the method being a very valuable aid to diagnosis. Mr. Roswell Park, of Buffalo, who was an authority on cancer, especially as it affected the breast, went round the wards of the hospital with him (Mr. Jones), and they saw a comparatively young woman with a very small nodule in the breast. They examined it carefully. His (Mr. Jones's) opinion was not a very valuable one, but Mr. Park's was; and the latter said it was an innocent growth. Dr. Macalister said that, from his point of view, it was malignant disease. Mr. Jones removed a small portion of that breast; it was examined and found to be malignant. It recurred in the scar in less than a fortnight, and a fortnight later he removed the whole breast, which was the seat of marked carcinoma.

Dr. H. C. ROSS said that three papers should have been published before the present one, but a description bearing on the matter would soon appear. An ordinary capillary tube was employed; 3 per cent. sodium citrate and 1 per cent. sodium chloride solution were mixed with an equal volume of the patient's blood, and the mixture was centrifugalized. The portion of the tube containing the corpuscles was discarded, and the remaining citrated plasma was mixed with one-eighth of its volume of blood from a healthy person's finger. It was incubated for half an hour and a drop of the contents was then placed on to a clean slide, covered with a cover-glass, and in fifteen minutes in a room at a temperature of 23° C. the extraordinary movement described could be seen. It consisted of the extrusion and retraction of long feeler-like pseudopodia. The movement would sometimes go on for eight hours, during which the snake-like processes were continually being thrown out from some cells. His brother (Professor Ronald Ross) said it was quite unusual to see such a thing in leucocytes. The word "stimulant" in the original title of the paper might lead to the conclusion that a stimulant for the phagocytes was meant; that was not so—an "excitant" was the right word.

Medical Section.

January 26, 1909.

Sir LAUDER BRUNTON, Bt., F.R.S., Vice-President of the Section,
in the Chair.

Ulcerative Colitis.

An Address introductory to a Discussion on the Subject

By Sir WILLIAM H. ALLCHIN, M.D.

THE morbid condition concerning which I have been invited to open a discussion is known as "ulcerative colitis," to which the prefix "general" or "simple" is frequently applied, and sometimes also the term "sporadic." The designation is sufficiently precise, so far as it expresses the structural changes met with, but is clearly open to the objection that there are several other affections of the large intestine, quite distinct from the one to be considered, to which the name might on anatomical grounds be given with equal propriety. It is a provisional title therefore, and its future retention or rejection will be largely determined by a clearing up of certain points in connexion with the natural history of the disease that will come up for consideration in the course of my remarks. The term "dysentery," it may be observed, is open to the same criticism on the clinical side.

So far as I am aware, attention to the condition was first drawn with any degree of completeness by Dr. Hale White in a paper in the *Guy's Hospital Reports* for 1888, although previously, in 1885, I had exhibited at the Pathological Society a well-marked specimen of the colon showing the characteristic appearances of the disease; and, as having been the first of the kind brought before the Society, it found a place amongst "the chief pathological lesions of more general interest" selected for exhibition at the Jubilee meeting of the Society on October 20, 1896. To Dr. Hale White further belongs, in great measure, the credit of having differentiated the malady from other forms of ulceration of the colon, and it would seem that since the publication of

his paper the name of "ulcerative colitis" has by general usage become attached to the disease. How far the extent of his differentiation has been justified by subsequent knowledge is the chief question at issue, towards the settlement of which it may be hoped this discussion may contribute.

That the affection is one of no great rarity, the result of the appeal to the large general hospitals of London for information on the subject in connexion with the present occasion amply shows. (I would, if I may, take this opportunity of expressing to those gentlemen who at much labour have furnished these reports the cordial thanks of the Society for the valuable information that they have furnished.) Thus, during the twenty-five and a half years, from 1883 to the midsummer of 1908, 80 cases were admitted to St. Thomas's Hospital; during the twenty years previous to 1908 there were taken in at Guy's Hospital 55 cases; and from 1884 to 1908, 42 cases were admitted to the Westminster Hospital. The incidence in successive years shows they are pretty evenly distributed at about the average rate of two or three per annum at each institution. On several occasions the numbers were somewhat larger at all of the three hospitals, but in other years a rise at one place was accompanied by a fall at the other two, so that there was nothing indicative of epidemic occurrence. It should be remembered that epidemics of dysentery have been unknown in Great Britain for more than fifty years, excepting such as have occurred in asylums, and a small outbreak in 1901 among the troops at Aldershot, which was supposed to have been introduced from South Africa. At other of the London hospitals considerably fewer cases were admitted; thus, during the twenty-five years previous to the present only 20 cases are recorded at St. Mary's, all of which were distributed over eleven years of that period; whilst in the records of the London Hospital during the past fifteen years there were only 22 cases in which ulcerative colitis was found post mortem.

From this same mass of well-arranged information may be ascertained with sufficient accuracy the age- and sex-incidence of the disease. By far the greater number of cases occur during early adult and middle life, and whilst a few are met with over 60 years of age, youth appears to be singularly free. Only very occasionally is the disease recorded as arising in children and infants, and it is a question whether it is of the same nature as that set up in older persons. Information on this point from those whose experience is derived from children's hospitals is much to be desired, for although a diarrhoea characterized by the

passage of blood and slime is far from uncommon in infants and children and is frequently fatal, I have never seen post mortem the appearance in the large intestines in any way resembling that which is constant in the disease we are now considering. In passing it may be observed that "all ages, from the nursing to extreme old age, pay their tribute to dysentery. In some of the European epidemics nearly half the attacks, and rather more than half the deaths, have been among children."¹

Of the total number of 177 cases of all ages reported from the three hospitals mentioned, 89 were males and 88 females. The great preponderance of mucous colitis in the female sex is in striking contrast.

The collective information now laid before the Society enables us also to appreciate what significance the previous history of the patient may have as to the real nature of the subject of our consideration, and among the data therein included is the fact of previous residence in a hot climate, in view of the relation of the malady to tropical dysentery. In this connexion, however, it is to be observed that the disease known as dysentery and presenting the essential clinical, anatomical and bacteriological features of that malady is by no means confined to tropical regions. To quote from the latest and most authoritative account contributed by Dr. Davidson to Allbutt and Rolleston's "System of Medicine" (1907), whilst "endemic dysentery is met with in warm climates only, dysentery, in the fullest sense of the word, is a ubiquitous disease, being met with in every inhabited region of the globe, from the Equator to the Arctic circle." As a predisposing or even a determining cause of sporadic ulcerative colitis previous residence in a hot country has not, therefore, so great an importance as it is sometimes supposed. This is fully borne out by the statistical information before us, for of the 177 cases already referred to only 7 had ever been out of England, and not one of these had previously suffered from dysentery. Of the 28 cases, however, reported from St. Mary's Hospital as many as 9 had been abroad (India, South Africa and Texas), 5 of whom were stated to have had dysentery and 2 typhoid fever. The occupations of the patients seem to exert no influence whatever on the determination of the disease. And almost the same statement may be made with respect to the occurrence of previous illness, a very large proportion of the cases having been well-to-do, well-nourished persons in excellent health. Thirteen of the 42 recorded at Westminster Hospital had suffered from previous intestinal trouble, chiefly of a diarrhoeal character that had persisted for years, and chronic constipation was the

¹ Dr. Davidson, Allbutt and Rolleston's "System of Medicine," 2nd ed., ii, pt. 2, p. 487.

preceding state of 3 of this group. Neither at St. Thomas's, Guy's nor St. Mary's Hospital is any reference made to such a condition. That a brother and sister were among the cases and that the father of one patient, and father and sister of another were said to have died of colitis cannot be regarded as other than coincidences, and there does not appear to be any form of prevailing disease in the family of any case sufficient to suggest any influence in bringing about the ulceration of the colon.

It is somewhat remarkable that in the reports before us there is so little evidence of the connexion of sporadic ulcerative colitis with chronic interstitial nephritis or with gout, an association that Dr. Hale White affirms has long been known, for of 23 of his cases of the colon affection "chronic interstitial nephritis was present in 6, and in at least 4 of these the renal change was advanced, and 2 of the 17 whose kidneys were healthy had urate of soda in their joints." Among the cases at St. Thomas's Hospital 5 were stated to have albuminuria, in 2 of which slight chronic nephritis was found post mortem, and 4 of the Westminster Hospital cases were reported as exhibiting albuminuria. With these exceptions no reference is made to the condition of the kidneys. The subject is an interesting one, although it has only engaged intermittent attention since Dr. Dickinson's paper on "Ulceration of the Bowels in Albuminuria," read before the Royal Medical and Chirurgical Society in 1894. So far as our present information goes the association between the two morbid conditions would appear to be of a twofold character. In the one group the ulcerative colitis develops, so to say, by chance in the chronic renal patient, presumably as an infection, runs its usual course, and is most commonly the cause of death; in the other group the ulceration of the mucosa of the colon is a terminal incident in the progress of the kidney affection, such as may develop in other mucous membranes, and if not actually overshadowed by the manifestations of the primary malady is subordinate in importance, death being due to uræmia or some other concomitant of the kidney changes.

The malady under consideration is one that is characterized clinically by well-marked symptoms that occur, on the whole, with such collective constancy as to render the diagnosis in any given case more than probable, the resemblance to conditions other than ulceration of the colon being but slight.

Only sometimes are there any symptoms preliminary to those connected with the intestinal lesion, such as malaise, headache and pain in the back and limbs, which occur in other acute febrile complaints. By

far the most frequently a sudden looseness of the bowels or passage of blood marks the onset of the disease. In the case of two lads aged about 18 who came under my notice the actual colitis was preceded over some months by occasional attacks, lasting a few days each, of partial incontinence of faeces, the motions escaping from the bowel without power of control, although no blood was then observed.

Undoubtedly the disturbances connected with the action of the bowels afford the most constant and characteristic symptoms, and as they are usually the first to appear so do they persist throughout the course of the disease. Although occasionally there may be some constipation at the outset a diarrhoea of varying degrees of severity is soon, if not at once, established. There may be but two or three motions, or as many as a dozen or twenty in the twenty-four hours, but there is never that constant and distressing desire to go to stool which is so marked a feature of tropical dysentery. Nor are short periods of confined bowels interrupting the course of the diarrhoea unknown, a circumstance which I am not aware is ever noticed in the epidemic disorder.

Except perhaps at the very commencement of the illness, or in the course of any intervening period of constipation, the motions are fluid, dark from abundant bile-derived pigments, singularly fetid, though scarcely of the putrid odour so common in tropical dysentery, and containing much or little blood, altered or almost pure, and a variable excess of mucus. It may be noted that the presence of blood in the stools is not to be taken as absolutely indicative of ulceration, although this is most likely, since a hæmorrhage from the mucosa may occur from the hyperæmia of inflammation before the actual destruction of tissue takes place. As regards the blood that is voided I do not know that it offers any distinctive feature either in quantity or in extent of admixture with the other constituents of the motion from what is observed in tropical dysentery, but very seldom does the stool consist of blood and mucus only, as is so common in the epidemic malady, and still less is it constantly composed of only a small pellet of clear or blood-stained mucus passed with agonizing straining and tenesmus which is so distinguishing a manifestation of the latter disease. The records before us, however, show that now and then undoubted ulceration of the bowel may exist and yet no appreciable blood appears in the dejecta, a circumstance that is not infrequently to be noted with similar lesions in other parts of the alimentary canal. Fragments and shreds of slough cast off from the bowels are sometimes recognizable and pus occasionally.

Associated with the diarrhoea is pain, but, as so commonly happens

in regard to the sensory symptoms of abdominal disease, and forms one of the strangest of the as yet unexplained phenomena of these affections, the constancy of the association is most uncertain. Without doubt pain, often very severe and of a paroxysmal griping character, is the more usual, but equally cases are described in which there is little more than a scarcely appreciable discomfort. When present it is but slightly affected by the taking of food, but is apt to be induced and intensified by the action of the bowels. Whilst generally diffused over the front of the abdomen the pain is sometimes distinctly perceived along the course of the colon, and in this respect may be accompanied by tenderness. It has, I think, hitherto been generally supposed that tenesmus, which is such a distressing, prominent and distinguishing feature of tropical dysentery, is altogether wanting in these cases of sporadic ulceration of the colon, but the information which has been collected for us shows that this is not so, the symptoms having been noticed at several of the hospitals in more than one case—indeed, *a priori*, tenesmus might have been expected to occur, since the anatomical changes, even to involving the anus, are identical with those of the dysentery of hot climates. It may be remarked, however, that Dr. Gemmell, in his classical account of asylum epidemics, says “tenesmus may or may not be present and is often slight.” Except that the symptom is less frequent in the sporadic affection it does not seem that any distinction can be drawn.

Among the other symptoms presented by the sufferer from this complaint, none are of sufficient constancy as to be of diagnostic value, nor with the information before us does it seem that any individually contribute much towards prognosis, however significant they may be in combination. Nor is the severity or otherwise of the symptoms generally to be regarded as an index of the extent of the intestinal lesion. Thus vomiting is of most uncertain occurrence, sometimes very severe at the outset of the disease, oftener only slight, and oftenest of all is absent throughout; an associated nausea has been noticed. It seems that the administration of enemata sometimes determines the sickness or intensifies it if present. “Hiccough,” says Dr. S. Phillips,¹ “occurs in the case of ulcerative colitis so frequently that it is almost to be regarded as a symptom of the disease, and its severity, frequency and persistency for many days or weeks constitutes in itself a very dangerous condition.” I cannot say that this is in accord with my own experience, nor, except as a terminal symptom in two cases at St. Thomas’s, do I find any reference to it in the records I have had the opportunity of seeing; but

¹ *Brit. Med. Journ.*, 1907, i, p. 1355.

it may be that ignorance of the occurrence of this symptom has caused a less careful observation of its frequency. All conditions of the tongue have been noted, from the clean and normal to one that is dry, cracked and glazed. Parotitis and parotid bubo have also been noticed more than once. The abdomen may be normal in contour, distended or retracted, and intestinal peristalsis is sometimes visible.

Equally variable is the general condition of the patient, for although in all cases he is obviously ill and often gravely so, it is not very uncommon for the appetite to be good and the nutrition to be well maintained, though some loss of weight, even to emaciation, is the rule. Many exhibit a febrile state from the first and throughout the illness, others manifest pyrexial exacerbations in the course of the complaint, whilst in many there is no rise of temperature at any time. The curve is frequently of the intermittent type and may reach 104° F., although a lower maximum, 102° F. or 103° F., is more usual.

Profuse sweating occasionally happens, and due to this and to the diarrhoea thirst may be complained of. Anæmia from loss of blood or the severity of the illness may be so profound as to suggest that it is of a pernicious type, but although, as would seem from the report on the patients at St. Thomas's, this state may exist when the hæmorrhage was not marked, in other cases the blood is normal. The leucocytosis, regarded by Dr. S. Phillips as so frequent as to be a feature of the disease, does not seem to have been so prominent to other observers—indeed, at St. Thomas's among the recorded estimations there were as many with a normal or low white cell count as with a high.

A review of the records shows the greatest variety in the duration of the symptoms, from one that was fatal in three days from the onset, mentioned by Dr. Hale White, to others that have lasted continuously for upwards of two years, or with intermissions of more or less complete health for ten or even eighteen years. The malady from this aspect appears to be separable into groups—those which may be termed acute, continuing for the most part for six or eight weeks, and others of a more chronic character where the symptoms may extend over years, broken by acute outbreaks to which the termination may ultimately be due. The intervals during which the manifestations of the disease subside may be marked only by a long-continued anæmia, or by an easily induced diarrhoea, or there may be no evidence of ill-health, and then after months a relapse may occur, again to pass off and perhaps again to recur. The liability to relapse is undoubted, and in this resembles also the tropical affection, and hence considerable hesitation must be expressed as to the future of any case that may seemingly be cured.

The majority of cases run their course to death or recovery uncomplicated by association with other forms of disease, which are of accidental occurrence when present. The malady is a distinctly marked out one, and, so to say, self-contained. At the same time it is recognized that ulceration of the colon, with the general features, both clinical and anatomical, of the specific affections, is liable to develop as a terminal event in other morbid states, one of which—chronic interstitial nephritis—has already been referred to. The records from Guy's give some interesting figures in this connexion, including fifteen cases of the renal disease, three of infective endocarditis, and nine of septic infection from the female pelvic organs, as well as single instances of glanders, pneumonia, morbis cordis, &c.

Incidental to the ulceration peritonitis may be set up consequent upon perforation of the gut, though in view of the extensive nature of the lesions it is remarkable that this complication is not of more frequent occurrence. It has been noticed by several observers that the peritonitis sometimes gives singularly little evidence of its existence during life. A liability to embolism in the pulmonary vessels and elsewhere has also been noticed.

Hepatic abscess is certainly a rare event in connexion with ulcerative colitis, only one case being recorded at St. Thomas's and one at Guy's, and none at Westminster Hospital. The two cases at St. Mary's in which this was found had suffered from dysentery in South Africa. A like infrequency of association of liver abscess with the epidemic dysentery of temperate climates and of asylums is well known. Even when there is reason to suppose that the ulceration of the intestine has been severe in any case and recovery subsequently follows, there seems to be but little liability to obstruction of the bowels from the cicatrization.

Sporadic ulcerative colitis in its outlook is most grave. Of the 80 cases at St. Thomas's, where special care was taken to exclude from the record all but those that strictly conformed to the condition understood by the term, 40 died, whilst at Guy's 40 of the 55 similarly selected cases were fatal; at Westminster Hospital a somewhat lower rate ruled, 19 dying of the 42 cases admitted. The mortality is about equal in the two sexes, and no special liability to death or recovery seems to be conferred by any age. These figures exhibit a distinctly higher percentage of fatal cases than obtains in tropical dysentery. Quoting from Dr. Davidson's article (*loc. cit.*), the mortality at Hong Kong in 1902 was 37 per cent. and 34 per cent. at Selangor, whilst the case mortality in six epidemics in France and Germany ranged from

9 per cent. to 14.9 per cent., and in different epidemics in Japan it varied from 16.5 per cent. to 30 per cent. In considering these statistics, however, it should be remembered, as Dr. Davidson observes, that the cases in the military and jail hospitals probably included a number so mild that recovery would follow from rest and dieting only. But after making allowances for this the preponderant death-rate of the sporadic cases is striking. But although the expectation of death is so high, recovery may follow even when the disease has run a comparatively acute and severe course; an interesting example of this nature was brought before the Harveian Society by Dr. H. Caley in 1893.¹ This circumstance makes the prognosis very uncertain in any individual case, but the severity of the hæmorrhage and of the diarrhœa, the presence of tympanites and extreme prostration and the typhoid state are manifestly ominous. Exhaustion is the most frequent immediate cause of death, brought about by the diarrhœa or by hæmorrhage, or by both, and more rarely by peritonitis; pneumonia and purpura have been observed as terminal fatal indications.

Such are the clinical phenomena presented in varying degrees by the subjects of ulcerative colitis. In the course of the account I have indicated those features which should suffice to distinguish the disease at the bedside from such specific intestinal ulceration as is determined by the typhoid or tubercle bacilli, by syphilis or actinomycosis, from ulcers consequent upon hæmorrhage into the mucosa, and from the ulceration of new growths of the bowel, or from like lesions caused by the bursting of an abscess into the gut. In such affections the intestinal symptoms, definite as they may be, are in the majority of cases quite subordinate to those of a general character, or to local manifestations elsewhere, and even when considerable in exceptional cases are markedly distinct if only in the nature of the diarrhœa and of the stools. On such an occasion as this I need not further formulate a differential diagnosis. I have also drawn attention to those differences, partial though they be and mainly differences of degree rather than of kind, that would seem to exist between these sporadic cases and those of epidemic occurrence. It would be most helpful were it possible to have the views of observers who are conversant with the manifestations of tropical dysentery, and have also had experience of these isolated cases, who would thus be in a position to estimate at their real value such differences as exist and at the same time to appreciate to the full the resemblances.

The post-mortem appearances which are met with in ulcerative

¹ *Lancet*, 1893, i, p. 795.

colitis and that clearly underlie the symptoms of the malady are as constant as they are characteristic. They are essentially referable to the intestines, and in a minor degree to the peritoneum and liver, though, of course, they may be supplemented by changes due to previous or co-existent disease in other organs that are only accidentally associated with the affection of the colon which is our present concern.

The condition observed is one of intense inflammation of the mucosa proceeding to ulceration, but the area of distribution of these states and their degree of intensity vary from an involvement of the entire gut from cæcum to anus, occasionally even extending into the ileum, with a complete destruction of the mucous membrane over large areas, to merely a few discrete ulcers in the lower part of the bowel with indications of a simple catarrh of the inner coat. Between these extreme conditions it is impossible to draw a dividing line; all grades of change are to be met with. No doubt the mildest cases in respect both to symptoms and structural alterations have long been recognized and described as catarrhal colitis, but the difference in degree in all aspects between such cases and those exhibiting the grave conditions connected with what we are describing as ulcerative colitis are sufficient, at any rate for the present, to warrant our regarding the latter as a distinct disease.

If the examination of the bowel be made sufficiently early in the course of the attack, whether this be after death or by the sigmoidoscope, or by operation wound during life, the mucous membrane will be seen to be swollen, soft and pulpy, of a deeply congested appearance, restricted perhaps to certain regions, or diffused more or less throughout the length of the large intestine. Exceptions to this description are met with, for Dr. Sidney Phillips, in an account of cases that came under his notice,¹ says that "the noticeable feature in one case was the dryness of the colon; it was desiccated and fragile rather than sodden, and in another in which operation exposed the colon during life its dry, pale condition afforded a great contrast to that of the glistening moist small intestine." Such appearances, however, I take to be most unusual, and I find no record of anything similar among the reports before us.

In exceptional cases nothing beyond this state of extreme inflammation is to be found, and so far as can be judged the characteristic symptoms may be present with a severe colitis only, but in the vast majority of cases some amount of ulceration is to be seen, whether this be only in the nature of a few minute points or definite ulcers involving the whole thickness of the mucosa. It is this latter condition, of course,

¹ *Brit. Med. Journ.*, 1907, i, p. 1355.

which justifies the name, and when existent to an extreme degree undoubtedly offers a most characteristic appearance. Over large tracts scarcely a shred of mucous membrane is to be seen, the muscular coat being wholly exposed, whilst here and there may be left patches of the membrane of varying size, some of which, being partially loosened, hang into the lumen of the bowel like polypi, or it may be that bridge-like processes, formed by undermining of the membrane, traverse the colon from side to side; or, again, the destruction of tissue may be limited to a few spots and of a size from a few lines across to 1 in. or more, with fairly clearly defined edges or with shaggy borders that overlap the underlying muscle. It is clearly by the coalescence of these individual ulcers that the large areas of necrosis of the mucosa are formed, though this is mainly effected in a molecular manner, since sloughs are but seldom to be seen. According to the extent of the mischief intervening portions of the bowel may be almost or quite healthy in appearance, and all stages of the process, from a simple colitis to a widespread destruction, may be visible in the same specimen. Although the entire length of the colon may be more or less ulcerated there are certain regions that are preferentially implicated, these being the cæcum, descending colon, sigmoid flexure and rectum, sometimes even involving the anus, but now and then each of these situations may escape and the transverse colon, which is oftenest free, may suffer; occasionally also the disease may invade the appendix and sometimes the small intestines. The information that has been collected for us fully bears out these statements in respect to the structural features of the malady, and further enables us to say that perforation of the bowel arises in about a fifth of the fatal cases. Sometimes this perforation is multiple and may constitute a rent in the wall of considerable size, but the extremely friable and, indeed, rotten state of the structures readily permits of post-mortem tearing. Very frequently the intestine is much thickened and hypertrophied, and the bowel very considerably dilated, a condition that has been commonly regarded as indicative of lengthy duration; but certain observations recorded from the Westminster Hospital show that this inference is not always valid, since the thickening was noted in certain of the acute cases and as wanting in some of the chronic. A very few times has pus been observed. From the appearances presented by the condition which has been described it has generally been supposed that the destruction commenced in the submucosa, and that the solitary follicles were not primarily concerned. From what we hear from Guy's Hospital, however, it appears that in five cases these structures "were involved in such a way

as to suggest that the ulceration had started in them." In passing, it may be remarked that true follicular ulceration, which is on the whole more frequent in children than in adults, presents a wholly different picture, both clinically and post mortem, to that we are now considering.

Inasmuch as a large number of patients suffering from ulcerative colitis recover, it is clear that repair of the intestinal lesions must take place, but the records from the hospitals go to show that even in fatal cases no inconsiderable proportion manifest evidence of cicatrization of the ulcers.

Unless peritonitis has resulted from perforation, the serous membrane gives little indication of change beyond perhaps some adhesions in the more chronic cases, and only occasionally are the mesenteric glands swollen.

The liver shows some liability towards fatty change, which was recorded in eight of the thirty-two post mortems made on adult patients who died of ulcerative colitis at St. Thomas's Hospital, and the same has been remarked by observers elsewhere.

It is apparent that this description of the morbid appearances is in complete accord with that of tropical as it is also of so-called "asylum dysentery," and thus sporadic ulcerative colitis is anatomically indistinguishable from epidemic dysentery. It is also interesting to observe that the structural changes closely correspond to those found in swine fever, as was shown to the Pathological Society of London in 1895 by the late Mr. Leopold Hudson.

The prima facie resemblance of sporadic ulcerative colitis to epidemic dysentery at once points to the infective nature of the disease, and raises the question of the specificity of the causal organism. It would, I presume, be generally accepted that some type of the *Bacillus dysenteriae*, whether the Shiga, Flexner or other, stands in this relation to the tropical disease, whatever may be the influence of the amoeba, although Dr. Hale White considers "that at present it is difficult to establish the claim of any disease to be dysentery on bacteriological grounds only."¹ Similarly also the work of Dr. Mott and others, particularly Dr. Eyre,² has demonstrated the same microbe as being tolerably constant in the evacuations and scrapings from the surfaces of the intestinal ulcers in the acute asylum dysentery, provided that the material be examined fresh, but that in chronic cases of the same malady the "*Bacillus dysenteriae*, if present, is so outnumbered by *Bacillus coli* and other intestinal

¹ Allbutt and Rolleston's "System of Medicine," 2nd ed., 1907, iii, p. 826.

² *Brit. Med. Journ.*, 1904, i, p. 1002.

saprophytes as to render its isolation a matter of extreme difficulty" (Dr. Eyre, loc. cit.). Moreover, the blood-serum of some of these acute cases possesses a specific agglutinative action when tested against *Bacillus dysenteriae* isolated from the stools of other similar cases, and also against other strains of *Bacillus dysenteriae* isolated from cases of dysentery in tropical countries" (*ibid.*).

If we now turn to bacteriological evidence connected with sporadic ulcerative colitis we find far greater contradiction than obtains in respect to either the tropical or the asylum variety of dysentery. To take an illustration of this, Dr. Sidney Phillips, in the course of the discussion on a typical fatal case of the sporadic form which he brought before this Section of the Royal Society of Medicine in February last, explicitly affirmed that "competent pathologists" had examined his case, as also others of the same disease which had been under his care, and had not found the Shiga bacillus. In contrast to this Dr. Saundby records¹ an undoubted case of ulcerative colitis—which, however, recovered—in which Shiga's bacillus was found by Dr. Hewetson, who carried out the investigation, though the agglutination test was not performed. Unfortunately, the information that has been collected for the present occasion does not afford much assistance towards the settlement of the question, though, so far as it goes, it is adverse to microbic identity of the sporadic and epidemic diseases. In four of the cases at St. Thomas's, Shiga's bacillus was searched for in vain; "in three cases there was no agglutination-reaction with this bacillus; in one case it was agglutinated at once (dilution 1 : 100); and in one case a dilution of 1 : 20 produced good diffuse clumping in one hour, 1 : 50 gave slight clumping in one hour, and 1 : 100 gave no reaction." At Guy's the blood of one patient was examined for the agglutination-reaction with Shiga's bacillus with negative result. Clearly the numbers are too few to draw definite conclusions from. As bearing on this point, Dr. Davidson (loc. cit.) states that in several instances of dysentery occurring as a terminal disease in adults the subjects of chronic Bright's disease, cirrhosis of the liver, or chronic heart disease, the *Bacillus dysenteriae* was obtained in cultures.

When such opposite results are obtained by experts under conditions that from a clinical point of view would appear to be the same, it is not for me to venture on an opinion, and my duty at the moment ends with placing the facts before you so far as they are known. At the same time, however, in the condition of uncertainty in which the subject for the time being stands, there arises to my mind that question which so frequently

¹ *Brit. Med. Journ.*, 1906, i, p. 1325.

presents itself in connexion with investigations of this character: Are we not apt in the search for a specific causal organism to overlook somewhat the contributory part played by the individual's own tissues? a factor in bringing about the result which even should be reckoned with when the actual infection is clearly ascertained. Surely there must be something more than the invasion of the microbe concerned when we find the specific *Bacillus dysenteriae* responsible, on the one hand, for the severe and fatal malady which we call tropical dysentery, and is also "found in almost all children who suffer from diarrhoeal diseases, in many of whom the symptoms are mild and transient, and clinically almost of no consequence. Moreover, this diarrhoeal affection does not present the appearance of an epidemic disease."¹ The same organism has also been found as a terminal affection in children the subject of wasting disease,² and, further, the *Bacillus dysenteriae* has been met with in healthy children (Dr. Flexner, loc. cit.). So wide a range of distribution of the organism coupled with such differences in clinical manifestations suggest most forcibly that the tissues of the individual, by their degrees of vulnerability or otherwise, are to be reckoned with in assigning the entire causal antecedents to the resulting morbid state.

This same consideration finds support from another point of view. It is well known that the administration of certain salts of mercury, even by other channels than *per oram*, is likely, when in excess, to set up an ulceration of the intestine, and especially of the colon, leading to symptoms closely resembling those of dysentery. It is generally accepted that the poison is eliminated, at least in part, by the intestinal mucosa, and thus leads to colitis and ulceration. Of the excretory functions of the alimentary canal we know as yet but little, but that such functions exist is undoubted, and analogy would suggest that modifications and perversions of the metabolic changes underlying the normal processes would be associated with alteration in the resisting power of the tissues to pathogenetic organisms, even if the toxic agent be not one originating in the course of the abnormal nutritive changes. It may thus be that the *Bacillus coli* on occasion excites an ulcerative colitis, being enabled to do so by the very condition of the tissues it infects, or that the nature of the contents of the bowels determine a virulence on the part of the microbe that it does not ordinarily possess. To conclude, however, that the causal organism may be of the *Bacillus coli* type, as would follow from the observations of some competent

¹ Dr. Flexner: Allbutt and Rolleston's "System of Medicine," 2nd ed., 1907, ii, pt. 2, p. 501.

² Dr. Howland's "Studies from the Rockefeller Institute," 1904, ii.

observers, is not to carry the question far, since, apart from the fact that the same organism has always, I believe, been found in association with Shiga's bacillus, it is well known that, like that other inhabitant of our bodies the pneumococcus, its range of power of doing injury is extremely wide, from being a harmless parasite to one that becomes responsible for toxæmias of a most virulent and fatal character. It is therefore a matter of extreme difficulty to affirm with certainty what is the precise cause of the malady we term ulcerative colitis, and so long as this side of the natural history of the disease remains incomplete we are unable to speak with certainty as to its specificity. What, it may be asked, are the criteria by which any given symptom-complex is to be regarded as a separate and distinct disease? In the case we are considering there is an identity in the structural features of the sporadic and epidemic maladies, together with a clinical resemblance, that is strikingly close, leaving only the question of cause to be considered. If the bacillus and the bacillus only be looked upon as the essential antecedent without any reference to the possible, if not probable, co-operation of two or more organisms being concerned in the result, then we must say at present that the two maladies are not the same. But if it be admitted that under modifications of its environment the harmless *Bacillus coli* of the intestines may assume a virulent and destructive character, or that the resisting power of the tissues is so depraved as to allow of the development of such changes as are characteristic of these several forms of dysentery, we approach a step further towards regarding the two conditions as essentially the same. When facts are wanting the judicious suspend a final judgment, though each observer will, from his own way of looking at an unsettled question, seek to explain the phenomena observed, and group them according to his own interpretation, and so long as this is conditioned by what is known or generally accepted this is both legitimate and desirable. As fresh data are ascertained these provisional views are confirmed or discarded, and new hypotheses are set up, in their turn to be accepted or rejected.

Lastly, there remains to be referred to the methods of treatment, which at present, having regard to the high mortality of the disease and the long-continued ill-health of many who do not actually succumb, must be looked upon as far from satisfactory. It has been towards controlling the two prominent symptoms, diarrhœa and hæmorrhage, that efforts have been mainly directed, but whatever success may have attended these efforts in individual cases, a consideration of the extensive and serious intestinal lesions that underlie these manifestations gives

but little ground for anticipating any frequency of cure by these methods only. Numberless have been the drugs, astringents, antiseptics, and sedatives that have been administered by the mouth with little or no assured benefit; frequently repeated small doses of calomel or grey powder combined with opium are certainly most to be recommended, and recovery has no doubt followed such a line of treatment. Ipecacuanha does not seem to have been as useful as it has been found to be in certain cases of tropical dysentery, nor have salines proved as beneficial, although here and there cases may be pointed to as justifying their use. Much greater success was reported by Dr. Gemmell as having followed full doses of quinine in the epidemic disease in asylums. With a view to treating the local condition of the bowel, and so lead to abatement of the diarrhoea and bleeding, rectal injections have been extensively employed—some, small in bulk, such as starch and opium, or tincture of hamamelis, or more recently adrenalin; others, larger in quantity and slowly introduced, with the object of bringing the drug in contact with as large a surface as possible of the inflamed bowel. Boracic acid in saturated or dilute solutions has been employed either alone or preparatory to the injection of some other medicament, such as nitrate of silver, a dram or a dram and a half of which to three pints of warm water was advocated by Sir Stephen Mackenzie so long ago as 1882.¹ Solutions of the perchloride or of the perntrate of iron have been used in the same way, and lately large enemata of a 1 per cent. solution of argyrol have been highly extolled. Excellent as these measures may have been in intention their frequent failure to effect any improvement has prevented their being regarded as other than very uncertain remedies. As a means, however, of bringing applications of this character more effectively to the ulcerative surface—and what is of equal, if not of greater, importance to divert the intestinal contents with their irritant and toxic ingredients—the cæcum or ascending colon have been opened and an artificial anus established. Such procedure permits a very satisfactory irrigation of the colon from above by antiseptic solutions, such as permanganate of potash (1 in 10,000), boracic acid, creolin, &c. Scattered throughout the medical journals are accounts of cases thus treated most successfully, but although offering the prospect of considerable benefit it cannot be said that the records are sufficiently numerous to allow us to estimate its exact value as a therapeutic measure. More recently appendicostomy has been recommended as doing away with the undoubted objections to colotomy, and as affording an equally

¹ *Pro. Med. Soc. Lond.*, 1884, vi, p. 159.

convenient means for irrigation, although it obviously does not prevent the passage of the bowel contents over the ulcerative surface, which appears to be very desirable to avoid.

The growing knowledge of the bacteriology of the complaint has suggested fresh lines for the pursuance of treatment, but until that knowledge has been more precise such courses must be tentative. To this end both antidysenteric serum and coli vaccine have been tried, and, as I can testify from experience of the latter, with marked benefit. It is not, however, until a larger body of evidence on this point is available that any conclusions can be drawn; the few cases hitherto recorded are quite insufficient to base any opinion on. It need scarcely be added that the lately introduced preparations of lactic acid ferments have found their advocates as remedies for this particular affection.

Whatever plan or combination of plans may be adopted it is obvious that the greatest attention must be paid to the general treatment of the patient. Complete rest and warmth are essential, and where pain is considerable fomentations or other forms of hot application are matters of routine; vomiting may be allayed by hydrocyanic acid in an effervescing draught repeated every few hours, by minim doses of tincture of iodine or other recognized measures, and for the hiccough opium is to be regarded as most efficacious.

Since the appetite is often good and the digestive capability of the upper parts of the canal often appear but slightly if at all impaired, a small quantity of digestible solid food is to my mind preferable to a wholly slop diet, and that as much food as can be well borne is desirable is evident when the exhausting nature of the malady is remembered. Anæmia and other complications require their appropriate treatment, and strychnine, alcohol or other stimulants will most probably be required.

The CHAIRMAN (Sir Lauder Brunton) said the Section had listened with the utmost pleasure to a very full and clear paper, admirably adapted to the needs of the Section. The subject was one which interested them all; it was difficult to treat, and so was wearisome alike to patient and to the doctor, and therefore any addition to our knowledge would be welcome.

Mr. G. H. MAKINS, C.B., said his experience of the surgery of the condition was neither very extensive nor highly satisfactory to relate at that early period of the discussion, but, in answer to the President's invitation, such as that experience had been he was glad to give it. His

experience of the treatment of ulcerative colitis surgically was distinctly in favour of colostomy, the object of such surgical treatment being a complete diversion of the fæces. The advantage attained of treating the disease locally from the colostomy opening was a minor one. The operation of choice was, he thought, the establishment of an artificial anus in the ascending colon. Appendicostomy did not attempt to divert the fæces, and was for that reason manifestly inferior for the treatment of this condition to colostomy. The diversion of the fæces by ileosigmoidostomy he also considered unsatisfactory for two reasons: first, even if a unilateral exclusion was made, a certain amount of reflux flow of faecal material into the colon above could not be helped, which was objectionable; secondly, in a large number of cases both the sigmoid flexure and the pelvic colon might be the seat of ulceration, and therefore a short-circuiting to either of these levels was not of material use. The necessity of complete diversion of the fæces was first impressed upon his mind by a case which he had to treat some years ago. The patient was admitted to the hospital suffering with ulcerative colitis, and had an attack of collapse resulting from very severe pain. The degree of pain and the general condition of the patient suggested that perforation of the colon had already occurred. The pain and tenderness were mainly in the line of the descending colon, and an abdominal exploration showed that to be the portion of the bowel most involved. Consequently a transverse colostomy was performed. It was then his custom to make a lateral opening only in the bowel wall when there was reason to believe that the artificial anus would only be needed temporarily, in the hope that spontaneous closure would eventually take place. In three months this colostomy did close spontaneously, but the closure was accompanied by a recurrence of the symptoms and the patient got into the same condition as before the operation. The colon was again exposed at the same point, completely divided, and the two ends brought up, and as a result the patient was relieved. He thought three months later that the time had arrived when the artificial anus should be closed, and as a preliminary to this measure he made a lateral anastomosis between the two limbs of the colon within the abdomen and re-established the current of the fæces. The passage of fæces along the excluded portion of the colon was followed by a fresh recurrence of symptoms, for which reason the further step of closing the artificial anus was not proceeded with. As ordinarily happens under these circumstances the anastomosis gradually contracted with a correspondingly free escape of fæces by the artificial anus, and at the end of a month the patient left

the hospital comparatively well. He did not know what had since become of her. She was operated upon in 1899; he saw her again in 1902, when she was in fair health, but, of course, with an epigastric anus. That case impressed upon him the importance of diverting the fæces. He had had further experience of the condition since. In cases where he had performed ascending colostomy, and later introduced a Mitchell Banks's tube to temporarily divert the fæces, there had been a recurrence of symptoms in several. With regard to the situation of the colostomy, no doubt the ascending colon was the best. It was true that the cæcum was often the seat of ulceration in the disease, but the existence of a small extent of ulceration in the proximal segment of the bowel was not a matter of very great moment compared with the advantage gained by making the colostomy at a point where the fæces escaped in a more solid form than higher up. The increase of comfort to the patient was very great from location of the artificial anus in the ascending colon and not in the cæcum. The colostomy should be a complete one; no doubt in this disease there was often a difficulty in this regard; if the portion of colon exposed was diseased the wall was very friable and the difficulty and danger of its manipulation might oblige one to revert to the cæcum as the more mobile part. With regard to the results of colostomy he had few cases to bring forward; he had performed the operation in six. One patient was ostensibly cured, the artificial anus had been closed after functioning for seven months, and her condition at the end of six months remained good. That was the only patient in whom he had been able to close the colostomy. In one case in which a transverse colostomy of the lateral kind was made—the case not being a very severe one—the anus closed spontaneously and the patient was improved and was now getting about. He did not think that the colostomy in that instance had any very great influence in the treatment, because it failed to do what he believed to be necessary—namely, to properly divert the fæcal current.

If such cases were treated by colostomy, he thought it was important to raise the question of when the artificial anus could be closed. That was a very difficult matter to decide. Cessation of symptoms must be awaited, and when the closure had to be considered he thought it was best to temporarily divert the fæces into the portion of the colon which had been excluded. That was best done by inserting a rubber tube across the colostomy opening, which usually ensured the passage of about half the fæces along the normal channel and allowed a judgment to be formed as to whether the re-entry of fæces into the

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colon caused fresh symptoms. If so, there was no object in trying to close the colostomy. That raised another point; a long time might have to be waited, and in the majority of cases if this period extended to more than seven or eight months the opportunity of closing the opening had been allowed to pass and the colostomy must remain a permanent one. Under those circumstances the colon which had not been used became contracted, as is always the case with bowel under these conditions. But in the case of ulcerative colitis the contraction was a more serious matter than when the colon had been excluded for some other cause and when the wall of the bowel itself was healthy. Sometimes after exclusion for ulcerative colitis the colon at the widest part was no larger than his finger, it was also extraordinarily pale and bloodless, and the appendices epiploicæ on the sigmoid flexure assumed a very large size, much bigger than the enlarged appendices observed on the bowel over a malignant growth. This contraction of the bowel, together with the large masses of fat, rendered it very difficult to do anything in the way of an anastomosis, so that if a colic anus had existed for some time an anastomosis could not be relied upon to aid in the closure of the opening. Another point which might be raised was that of the actual danger of attempting to close the opening in this disease; it was apparently a much more serious proceeding than the closure of an artificial anus which had been established for the relief of other conditions. He did not know why this was so, and he had no actual personal experience of this accident, but in his hospital several patients in whom an artificial anus was made for the relief of ulcerative colitis had died from peritoneal infection as a result of operations for their closure. In other diseases the closure of an artificial anus might be looked upon as a fairly safe procedure. In performing colostomy for ulcerative colitis the fact must be faced that the patient might have to maintain the artificial anus possibly for life. He did not know that these patients would necessarily be very uncomfortable even if they had to do that. He would mention only one such case. A youth aged 20 suffered from very severe ulcerative colitis with great hæmorrhage and consequent anæmia; he believed that this patient would have died had a colostomy not been done. After the operation the patient at once improved, and at the end of six months appeared to be in fair health; he was then anxious to have the artificial anus closed, and Mr. Makins was anxious to do this. A Mitchell Banks's tube was inserted and left in position for a week, after which time symptoms redeveloped, diarrhœa and the passage of a considerable amount of blood and mucus

occurring. The idea of closure was abandoned at that time, now some 2½ years ago, and since then the patient has lived a comfortable life, has been at the University, where he took his degree, played golf, and rode a bicycle, sometimes as far as 40 miles, and has danced. If anything upset him he still passed blood and mucus *per rectum*. He (Mr. Makins) was of opinion that were any attempt made to close the anus more harm than good would ensue, and the patient himself had made up his mind to put up with it. In proposing this operation too much weight should not be attached to the probability that the artificial anus was to be temporary only; if that were done the patients naturally began to worry and ask for the opening to be closed, and yet the closure might be inadvisable or impossible. If patients insisted and the attempt were made, it might end in recurrence of the symptoms, or even worse. Of the six cases he had mentioned as having been treated by colostomy, one died. The fatal case was one of very severe and extensive ulceration, and the patient died on the twenty-third day after the operation.

Dr. HALE WHITE said he thought the account given by Sir William Allchin and the gentlemen who kindly analysed the cases at the various hospitals was so full that but little was left to be said about the disease. He could bear out what Sir William Allchin said about the confusion which existed regarding colitis. It was not uncommon to find membranous colitis spoken of as ulcerative colitis; but those maladies were distinct from each other. The introducer had mentioned that there were many causes of ulceration of the bowel; growth, dysentery, tubercle, typhoid and so forth, but the speaker was surprised not to find mention of instances of vascular ulcers of the intestine. Fifty years ago Wilks and Moxon stated that the intestine was ulcerated in very various conditions, patients dying generally of feeble circulation. He (Dr. White) had seen it, and he did not think it was uncommon to meet with it in the post-mortem room. One such case was given in the series from Guy's. As Sir William said, if the case was to be regarded as ulcerative colitis, there must not be any of the obvious causes of ulceration which were known to all, and the patient must present a definite assemblage of symptoms from which one could predict that after death the colon would be found ulcerated in the way Sir William had described. As our knowledge of the causes of ulceration of the colon widened, the number of cases reported simply as ulcerative colitis would get less. For example, cases due to the pneumococcus

could now be excluded, for it was known that colitis going on to ulceration may occur in the course of pneumonia, and so one learned that the pneumococcus could cause ulceration of the intestine. The same could be said of the cases in which ulcerative conditions of the bowels occurred in the course of septicæmia, of which, happily, few cases were now seen, though the post-mortem records of fifty years ago contained many such cases. Cases occurring in glanders could similarly be marked off. But with regard to the residue of cases which we call ulcerative colitis, as the author said, there was nothing distinctive in the colitis itself. The association of chronic interstitial nephritis with the disease was of great interest—indeed, of historic interest—because if Fellows would look at Bright's original "Tabular view of the Morbid appearances occurring in 100 cases in connexion with Albuminous Urine," one of the fatal cases, viz., No 23, was a very excellent instance of extensive ulceration of the intestine. The figures which had been handed round showed that that association existed, and he supposed we must assume that a person with chronic interstitial nephritis had a lowered resistance in such a direction that the micro-organisms which were the cause of the ulcerative colitis obtained a hold; though it must be remembered that it had been suggested that some of the cases associated with interstitial nephritis were due to hæmorrhages in the course of the intestine. That question was raised many years ago by Sir William Gull.

Much attention would, no doubt, be directed to a discussion of the relationship between ulcerative colitis and dysentery. From the facts that were available, each would form his own opinion according to his interpretation of them. He did not regard the case as proven either way. He had had only a short experience of the tropics, but as far as the dysentery he saw there was concerned, it was unlike ulcerative colitis clinically. And all would admit that the bacteriology of the subject was yet in a very uncertain condition. No bacillus was mentioned in the definition of dysentery in the last edition of Clifford Allbutt's "System of Medicine." The author mentioned the case recorded by Professor Saundby in the *British Medical Journal* as a case in which Shiga's bacillus was said to have been found in ulcerative colitis. But from a correspondence which subsequently appeared in the *British Medical Journal* it seemed that there was much doubt whether the bacillus described was Shiga's bacillus. And none of the cases narrated on the present occasion supported the identity of the two diseases; rather they seemed to separate the two conditions. He

hoped someone with a large experience of asylum dysentery would enlighten the Section, in the course of the discussion, because it was one of the most interesting aspects of the question. Dr. Knobel pointed out that the condition of the nervous system must have something to do with it; for certainly asylum dysentery was far commoner in asylums than was any similar disease in other establishments, so that the cause could not be the mere herding of large numbers together. And the same authority gave instances in which there had been widespread epidemics in new asylums. Possibly the lowered resistance of lunatics might have something to do with it. In the Guy's series got together on the present occasion there were four cases of nervous disease associated with the ulcerative colitis: two of tabes and two of chronic disease of the cord. Considering that in a general hospital post-mortem examinations on patients with diseases of the cord were not frequent, and ulcerative colitis was a rare condition, the association could hardly be a mere coincidence, and many other instances of the association had been described; three or four in the Pathological Society's "Transactions," one by Mr. Targett, and another by Dr. Acland. It was curious that occasionally ulcerative colitis was associated with a single abscess of the liver; but he did not think it ought therefore to be assumed that this was of much value as showing that ulcerative colitis was the same as dysentery, because the more one thought of why a man with dysenteric disease of his intestine should get a solitary abscess of his liver, the more difficult it was to understand. One would have thought that the amœbæ would have been distributed about the liver in such a way as to make multiple abscesses. He believed they were still in the dark about the pathology of single abscesses of the liver. A rare variety of ulcerative colitis was that in which the symptoms of ulcerative colitis were due to the bursting of a hepatic abscess into the bowel. He had known of one or two such cases. He had seen difficulties occur in the recognition of appendicitis from ulcerative colitis, and in the latter condition the appendix was frequently involved. The practical point in connexion with that was, that on no account should the appendix be removed when such a condition as ulcerative colitis was suspected: operation then could scarcely be expected to do good, because it would remove only a small area of the ulceration. Examination *per rectum* of all cases of suspected ulcerative colitis was most important. From his post-mortem experience he could confirm what Mr. Makins said as to the inadvisability of short-circuiting, because frequently the disease reached down to the anus, or the bowel involved was so rotten that no such operation

could be done. His experience of rectal irrigation of any sort had not been very favourable; he had not been able to discover that washing out did those patients very much good, and for that reason he had not done it recently. Nor had he obtained much benefit from drugs. He had been much interested in what Mr. Makins said about opening the ascending colon, and if surgery was demanded in a case, that seemed to be the best operation. He (Dr. Hale White) had very little experience of it, but he could remember one man whose life certainly seemed to have been saved by it. He was so bad that the wound was not closed. That was several years ago. He still had an artificial anus, but was going about perfectly well. For a long time after the operation he suffered much from irritation of the skin owing to the very irritating nature of the fæces. He had had scarcely any experience of opening the appendix for the condition; and Mr. Makins's argument appealed to him very much: that one was hardly likely to get much good from that because the fæces would still pass over the ulcerated surface. It was difficult to decide in any case whether surgery should be undertaken, because mild cases would probably get well, and severe cases would die, whether operated upon or not. Probably there were few conditions in medicine which required greater judgment in estimating whether the time had come for operation. Opening the ascending colon should not be decided upon lightly, because it was not a pleasant thing to have artificial anus on the right side. But he thought he had advanced his knowledge of treatment by the experience of a single year, because in the last twelve months he had had three cases of ulcerative colitis, all of whom had been treated by coli vaccine; they had all been put upon milk which had been soured by a reliable lactic acid preparation, and all were at the present time well. He admitted that the cases were very few, and that sufficient time had not elapsed to know whether the cure was permanent, but as three successive cases had got well on a definite line of treatment, that was sufficient justification for continuing that treatment. The sour milk should be given in large quantities. An adult took three pints a day of it, in addition to his ordinary milk. Of course, the patients were kept in bed, and were fed upon slop diet as well.

Medical Section.

February 10, 1909.

Dr. T. HENRY GREEN, Vice-President of the Section, in the Chair.

Discussion on Ulcerative Colitis.¹

SIR PATRICK MANSON, K.C.M.G., said that he accepted with considerable diffidence the task of reopening the discussion on ulcerative colitis. He had listened with great interest to what Sir William Allchin, Dr. Hale White and Mr. Makins had said at the previous meeting. He was sorry, however, that the term ulcerative colitis had been adopted as the basis of discussion, for his belief was that ulcerative colitis was merely a name for a phase of a class of diseases which had hitherto been included under the term dysentery. A morbid anatomy basis for classification was not a satisfactory one. To use the term ulcerative colitis as the name for the disease was likely to give rise increasingly to the impression that ulceration was a feature more or less peculiar to this condition, and that ulcerative colitis represented something new and fresh that had not been recognized hitherto in pathology. Moreover, the use of the term had the disadvantage of excluding important phases of the disease of which it was only one feature, and that a temporary feature, for the condition that gave rise to an ulcerative colitis in one patient might give rise to gangrenous colitis in another, and to catarrhal colitis in a third. Patients who in former days would have said they were suffering from dysentery often came to him now believing that they had a disease which was much more romantic in character and calling it ulcerative colitis. The proper basis for classification of any disease was the etiology—the germ cause—and until they applied that principle to the classification of the dysenteries they would be apt to be misled and progress in knowledge be delayed.

¹ Adjourned from January 26.

He had written on the blackboard an attempt at what he conceived to be a scientific classification of the group of diseases which they indicated by the word dysentery.

Dysentery	Bacterial	{ Bacillary dysentery <i>Bacillus dysenteriae</i> (Shiga, &c.) " <i>pyocyaneus</i> Durham's micrococcus
	Protozoal	{ Amœbic dysentery (<i>Amœba hystolitica</i>) Balantidium dysentery (<i>Balantidium coli</i>) Kala-azar dysentery (Leishman body) Malarial dysentery (malaria parasite)
	Verminous	{ <i>Schistosomum japonicum</i> " <i>hæmatobium</i> " <i>mansoni</i> <i>Esophagostomum brumpti</i>

Dysentery was not a disease; it was merely a word standing for a group of symptoms indicative of an inflamed condition of the colon. Until they ceased to think of dysentery as one disease, and one disease only, they would be likely to be misled in their ideas as to its pathology and treatment. Practically, dysentery was an expression with little more significance as regarded disease of the bowel than the word cough possessed as indicative of disease of the lung. The attempt he had made at a classification consisted of dividing dysenteries into three categories according to the known or assumed causes: (1) The *bacterial* form of dysentery, including first that produced by *Bacillus dysenteriae*; second, that produced by *Bacillus pyocyaneus*, and third, that supposed to be produced by *Durham's micrococcus*. (2) Another category of dysenteric diseases might be called the *protozoal*, caused in the first place and most frequently by the *Amœba hystolitica*. Other well-marked types of protozoal dysentery were produced by the malarial parasite and the Kala-azar parasite, and another that peculiar form of dysentery now known to be produced by the *Balantidium coli*. (3) Lastly, there was a group of dysenteric diseases produced by worms—the *verminous* type. This type of disease had only recently been recognized. Dysentery was now known to be produced by the *Schistosomum hæmatobium* and the *Schistosomum mansoni*. Undoubtedly these gave rise to a disease clinically indistinguishable, save by the microscope, from forms of ordinary dysentery. Other types of verminous dysentery were produced by the *Schistosomum japonicum* and by yet other and more insignificant worms. He considered that this was a proper basis for the classification of the dysenteries, and until some such

system was adopted they would wander about in their conceptions of the subject in an indefinite, unsatisfactory manner. His main reason for speaking was to submit this classification. But he wished to make one or two comments on the remarks of preceding speakers. An important point was the application of surgery to the cure of dysentery. The surgeon was gradually invading, not only the abdomen, but the details of the abdomen, including the colon. He thought it was a very dangerous thing to hang the word "operation" in front of a patient. As soon as the word operation was pronounced some patients were never satisfied until they had had it performed. He believed that if in these cases operation became popularized it would in the long run kill more than it would cure. In the hands of competent surgeons the operation could do good, but in the hands of general surgeons, especially in the hands of unpractised surgeons, who were just those most likely to meet the disease in the tropics, attempts at operation would prove disastrous. In his experience he had only seen two or three cases of dysentery in which an operation was required. In one case the patient was passing little masses of organized tissue, evidently polypoid excrescences, and suffering intense pain. He got Mr. Godlee to open the bowel, more with the idea of mitigating pain than of effecting a cure. Another case was that of an elderly gentleman suffering from chronic dysentery which had been neglected for many years. When the patient came to him he was evidently in the last throes of chronic dysentery. The case went from bad to worse. I got Mr. William Turner to open the bowel, and the patient eventually recovered. In a third case with which he had to do some time ago the patient was suffering from apparently hopeless chronic dysentery. Finally it was resolved to perform an operation. The patient at first demurred, but finally consented. Before the operation could be performed, however, the patient began to improve and ultimately recovered. In the vast majority of cases he believed that if proper treatment, food, &c., were given, the necessity for a dangerous operation would be obviated. In dysentery imported into this country—as distinct from dysentery arising here—the vast majority of cases were amœbic, and would yield to proper treatment. The patient should have proper food and rest, and be submitted to a course of treatment with ipecacuanha. But it was essential that this medicine should be properly administered. In the case of any disease for which they possessed a specific, if they gave one or two doses only and then left off they would fail to cure. In the case of malarial fever, for instance, if only a dose or two of quinine were given they might see a little improvement, but

presently the case would relapse. In the treatment of amœbic dysentery in ipecacuanha they had a specific that would act, he believed, as surely as mercury did in syphilis or quinine in malaria. Therefore in cases of chronic dysentery coming to this country from abroad he strongly urged the persistent use of ipecacuanha. The treatment should be followed up for weeks and months in exactly the same way—although, of course, in different doses—as the mercury treatment in syphilis or the quinine treatment of malaria. In cases in which the ova of the worms causing the third or verminous type in his classification of the dysenteries were lodged in the bowel, treatment was practically hopeless, and such cases, he thought, might be handed over to the surgeon. Malarial dysentery was not uncommon; if one met with a dysentery of some duration in which the temperature was very much higher than usual and the spleen enlarged, one might be almost sure either that it was hopelessly complicated or else that it was malarial. If the blood was examined in such cases the malarial parasite would probably be found. If, in such circumstances, one gave a full dose of quinine one might await the result with confidence.

Dr. THOMAS CLAYE SHAW said that owing to the fact that Sir William Allchin in his address spoke about ulcerative colitis, more especially from the point of view of the asylum question, he (Dr. Shaw) would also speak of it so far as he had found it in the insane. More than twenty-five years ago he published in the *St. Bartholomew's Hospital Reports* some cases of what was now recognized as ulcerative colitis or asylum dysentery, and he was led to give these reports by the particular pathological appearances which he found. At that time bacteriology was in its infancy, but he then attributed the cause of these lesions—the lesions which were now described as ulceration—not to bacterial origin, but to a nerve degeneration, and he formed that conclusion by reason of the constant symptoms of nerve degeneration associated with gastric symptoms to be met with in the insane. He ventured to think at that time that there was an alteration of the “periodical times” of digestion, and that fact had come recently more into prominence. Instead of the regular contraction of the muscle of the stomach and the pouring out of the stomach juices, there appeared to be a general derangement, and the functions became irregular. He concluded that the altered nervous conditions, together with the altered gastric conditions, combined, of course, with the altered chemical relations, had a good deal to do with the bringing about of the condition

which formed the subject of the present discussion. There was no diarrhoea in a great many of the cases and no question of an epidemic sort, and as the condition concerned only the insane patients, one felt that one was justified in attributing it primarily to nervous degeneration. Afterwards, but in rare instances, some of the staff of the asylum became affected. Later the Claybury Asylum was opened and became overcrowded, upon which there was a severe epidemic of colitis. The authorities called upon Dr. Mott and another to investigate. They came to the conclusion that although no special bacillus could be discovered, still the condition was infectious and probably microbic in origin. They absolutely scouted his idea that it was in any way due to nervous causes. Dr. Mott and Dr. Durham rightly insisted that the cases should be isolated, and that isolation was very carefully done, but it did not stop the colitis. He understood that there had recently been an outbreak of the disease in one of the new asylums under the County Council. No doubt in some of the cases the condition was infectious, and its origin might be bacterial. Were it due to nerve degeneration it might still be infective, because the lowered resistance of the tissues gave the organisms their chance, even under the perfect hygienic conditions found at the large asylums. There seemed to be no obvious cause of infection, and the intelligent attendants were not attacked by it. He therefore did not see how any other supposition than that of nerve degeneration could account for some of the cases. Where, then, was the nerve degeneration? Dr. Mott said he had not found in the Meissner plexus any signs of degeneration, but was it likely that such a change would be found? The conditions he spoke of had been ulcerations, with a distinct punched-out appearance, sometimes with considerable erosion of the mucous membrane, and it was chiefly in the duodenum and the descending colon. Looking at those points, and the fact that affections of the nervous system caused alterations in the digestive system—in mania, melancholia, dementia, there was a great upset of the digestive functions—that in conditions of sclerosis there was gastric disturbance of an explosive character, there seemed to be made out a *prima facie* case for a nervous origin of the condition, so that the microbic sequel easily followed. That was the view which he strongly held, and which he had advanced also in writing. At first he was disinclined to accept Dr. Mott's conditions, but they were so clear and seemed to him so conclusive that he had to accept many of them, and his first view must be modified. He admitted that where there was overcrowding and insanitary conditions the communicability of the diseased was enhanced, but there were cases which, he contended,

could not be explained except upon the basis of a primary nerve degeneration. If ulcers could be produced in other parts of the body from nerve degeneration, why not in the digestive tract?

Dr. SIDNEY PHILLIPS said that although exception had been taken to the title selected for the subject under discussion, it was justified in that it had led to an expression of the views of Sir Patrick Manson and others; he believed it was meant to exclude mucous colitis from the discussion. No doubt there was no sharp dividing line between colitis and ulcerative colitis, the ulceration in some cases only occurring as a second stage.

Ulcerative colitis appeared to be much more common now in this country than formerly. There was no mention of it in any of the published reports of any of the London hospitals before 1888, when Dr. Hale White published cases in *Guy's Hospital Reports*. It was not mentioned in *St. Bartholomew's* or *Westminster Hospital Reports* before 1893, nor in the *London Hospital Reports* till 1897. And the textbooks used twenty or thirty years ago, such as Bristowe's and Hilton Fagge's, made no allusion to it. The speaker himself had seen many cases at St. Mary's Hospital and elsewhere since 1888, but not before then.

Possibly the cause of acute ulcerative colitis was connected with our food supply; tinned or preserved foods might have something to do with it, and he had had two cases coming on acutely after partaking of meals at restaurants. He did not agree that colitis was not met with between infancy and adult age, as suggested by the St. Thomas's Hospital records; he had published a fatal case at the age of 13 years, and had seen others at the ages of 12 and 15. He had been surprised that more mention of pain had not been made, because sometimes it was agonizing, and almost caused collapse. Leucocytosis, he wrote some time ago, occurred in most of the cases, and his subsequent experience confirmed this. Bushnell, too, recorded 5 successive cases in which leucocytosis occurred, and Dopter in his recently published work,¹ found that in bacillar dysentery polymorphonuclear leucocytosis was found, just as Dr. Spilsbury had found it in the speaker's cases of colitis. Sir William Allchin rather dissented from his statement that hiccough was a very frequent accompaniment of colitis, because out of 80 cases occurring at St. Thomas's Hospital, it was only mentioned as occurring in 2. But

¹ "Les Dysentéries," Paris, p. 253.

the other 78 cases occurred over a period of some twenty years, the notes of each being taken probably by different clinical clerks who would not be likely to mention the incidence of such a symptom as hiccough. Of 13 very acute cases under his observation hiccough was noted in 8, and in 2 of the others there was no inquiry made about it. Other writers had also noticed hiccough in their cases. No mention had been made during the discussion of acute hæmorrhagic colitis in which profuse melæna occurred, reducing the patient to a state of extreme anæmia; in 1 case where the colon had been opened by operation blood could be seen continuously oozing from the interior of the colon.

With regard to the identity of dysentery and colitis, if the latter was a form of dysentery it was in most cases that variety which was due to the *Bacillus coli*. Several of his cases examined by Sir Almroth Wright exhibited only *Bacillus coli* and streptococci. In some recent asylum cases he believed Shiga's bacillus had been found. There did not seem to be much difference in the naked-eye post-mortem characters found in the "dysentery" of asylums and in acute colitis occurring elsewhere.

The speaker agreed with Dr. Hale White that no drug had been found which had a curative effect in ulcerative colitis, but he thought mercury had a distinctly beneficial effect; cases doing badly under it did usually worse when it was discontinued, and it was surprising for how long patients with colitis took mercury without any signs of mercurialism; he thought probably mercury was not usually given freely enough in some cases. Adrenalin was of great use in the hæmorrhagic forms of colitis, controlling the bleeding better than any other drug.

Sir Patrick Manson had laid stress on the inexpediency of operation in colitis, but Dr. Phillips thought he spoke only of cases of chronic dysentery. Dr. Phillips had not found that opening the colon and irrigations cut short the disease, and thought the rapid cures reported after such procedure could not have been in the type of cases of severe acute ulcerative colitis. In several cases under Dr. Phillips's observation the disease had continued its course unchecked by operation, and mere arrest of the passage of the fæces over the colonic tract did not arrest the disease; the organisms producing the affection were in the tissues of the colon as well as inside it. But operation in some cases was necessary and certainly gave relief, and in one case was the only thing which prevented death from hæmorrhage. Appendicostomy was not always possible, the appendix being sometimes bound down from

old inflammatory attacks, and sometimes it was impossible or very difficult to irrigate through the appendix. Mr. Makins's experience of relapse following operation to close a colotomy wound had occurred in a case of Dr. Phillips's, the relapse being fatal. He thought the advisability of operation must be decided on the merits of each individual case, and it should be borne in mind that however ill the patient became in colitis, recovery might ensue even without operation.

Mr. W. G. SPENCER said that much of the surgical side of the question had been fully dealt with, but he would refer to a case mentioned by Sir William Allchin, which was in the medical wards for some time, and had been treated with various drugs, and also by high enemata containing different materials. The patient was afterwards handed over to him, and he did a colotomy. Later the nurse was able to syringe right through the bowel, and the resultant liquid was clear, although previously there had been a continual passage of blood and fibrinous material. But he was never able to close the opening, because on attempting to do so there was a recurrence of the condition. Some years afterwards the patient presented herself at hospital complaining of the colotomy opening, but she was otherwise quite well. She died seven years after the colotomy of alcoholism and cardiac conditions, but the post-mortem examination showed nothing abnormal in the colon. Probably the lesion had been a superficial one. In another case at the hospital apparently the disease was confined to the sigmoid flexure, and part of her trouble was due to extension outwards binding down peritoneal adhesions. Some time after irrigation the colotomy opening was closed, but the disease had been very limited. In a case of the opposite condition on which he operated, the patient probably died a few days sooner through being explored, but post mortem the whole colon was in a polypoid condition, like a mass of new growth. From the surgical point of view he agreed fully with the points mentioned by Dr. Phillips, that the disease was very deep-seated, and he would wait before operating for some more surgical reason than simple ulceration of the bowel. All must agree that if there were stricture or pockets containing large quantities of pus that appendicostomy should be done, but in superficial lesions of the mucosa and limited ulcerations high enemata would do as much good as appendicostomy.

Dr. NORMAN DALTON: As regards the pathological anatomy, I have met with in England a case of the typical diphtheritic type. It was a very acute case which died in about a week. The ulcerations were

typical and extensive, but pieces of the mucous membrane which had not entirely disappeared showed coagulation necrosis, and the connective tissue beneath was infiltrated by fibrin in strands.

As regards the serum reactions, I have had under my care a patient with multiple papillomata of the colon whose blood clumped Shiga's bacillus, and more recently there has been a fatal case of ulcerative colitis at King's whose blood did not clump Shiga's or Flexner's bacillus, but gave a strong and rapidly developing reaction with the typhoid bacillus (1:50).¹ I therefore do not think that the clumping test will help us much in diagnosis, and I believe that the variations in particular cases are due to the temporary virulence of the bacteria aided by the temporary or permanent predisposition of the patient rather than to any real specificity on the part of the organism.

As regards the relation of ulcerative colitis to insanity, I believe that the frequency of the disease in asylums is due to a predisposition on the part of the bowel rather than to any trophic action on the part of the nervous system. When we consider how nearly insane are many sufferers from enteroptosis and mucous colitis, it is not unreasonable to suppose that many lunatics have some functional or organic affection of the intestines which strongly predisposes to the supervention of ulcerative colitis.

As regards treatment, I think that in the very grave cases colotomy should be done as soon as the diagnosis is made, and I wish to record a case which proves that it is almost never too late to perform this operation. A woman, E. B., aged 30, after being ill with ulcerative colitis for about six weeks became suddenly much worse and appeared to be in a hopeless condition when the operation was done by Mr. Carless. After this she rapidly improved. At first we could not wash out the colon through the colotomy wound, as the lotion returned, but afterwards we were able to do so. By the way, in washing out I always place a tube through the sphincter and so that the fluid can escape readily. Considering the depth of the ulcerations in these cases, I always fear lest distension of the colon by the lotion should cause perforation. Later on faeces passed occasionally *per anum*, so we plugged the distal end of the opening in the colon. She left the hospital apparently well, but had slight relapses from time to time. In 1905, eighteen months after the

¹ In this case either the organisms which usually cause ulcerative colitis were producing an agglutinating agent usually produced by the typhoid bacilli, or the typhoid bacilli were really present, but were producing the symptoms and lesions of ulcerative colitis instead of those of enteric fever.

colotomy, she developed a fistula *in ano*, which was operated on successfully. At this operation it was observed that there were still ulcers in the rectum and much cicatricial contraction. In April, 1908, nearly five years after the colotomy, she reported herself as being quite well for a long time, although feces passed through the anus as well as through the colotomy opening. Mr. Carless accordingly operated and was successful in closing the colotomy opening. The patient has kept well for the past nine months. The time is too short for us to be certain that no relapse may occur, but this certainly appears to be a case in which colotomy was done when the patient was almost *in extremis* and in which the colotomy wound was closed after five years. The patency of the colon below the opening was probably maintained by the lavage and by the occasional passage of feces.

MR. LOCKHART MUMMERY said that cases of the kind under discussion were not infrequently seen at St. Mark's Hospital. A fact which had not received much notice in the discussion was the value of the sigmoidoscope in these cases. By its means one could see the type and form a fair estimate of the severity of the ulceration. There were probably almost as many types of ulceration in the colon as on the skin, and the treatment which would be correct for one kind would not be correct for the others. In those cases there was generally some ulceration in the sigmoid flexure which the instrument revealed. He showed drawings of some of the conditions he had seen with the sigmoidoscope. It might be supposed that the use of the instrument in ulcerative colitis was dangerous, but that was not so if it was used with care and skill, because the end of the instrument practically never touched the bowel-wall at all. The parts of the bowel just in front of the end of the instrument were dilated with little puffs of air from the bellows, and it was not necessary to touch the bowel-wall with its end, nor should sufficient air be used to produce tension. The instrument showed that there were many kinds of ulceration, some of them mild in degree, but extending over a large area, the mucous membrane being excoriated and red. Those cases in his experience got well often readily. Such patients should be kept in bed, and the bowel washed out with suitable irrigants. The type which did not get well from medical treatment was that in which there were large irregular-shaped ulcers exposing the muscular coat. He had collected records of 25 cases of ulcerative colitis treated by operation: 6 in his own practice, and the remainder from other sources. Of the 25 cases, 5 died and 20 recovered, giving a

mortality of 20 per cent. This was less than half the mortality resulting from medical treatment. Up to the present also the cases which the surgeon was called upon to treat were those which did not get well by medical measures, hence they were the worst cases. Out of 33 cases not treated by operation, 26 died, giving a mortality of 78 per cent. Probably that was higher than the general average, as the St. Thomas's Hospital figures gave a mortality of only 50 per cent. The nature of the operation was as follows: cæcostomy in 6 cases with 6 recoveries; left inguinal colotomy, 5 cases, 2 deaths, 3 recoveries; appendicostomy, 10 cases, 1 death, 9 recoveries; left lumbar colotomy, 1 case, 1 death; laparotomy, 2 cases, 1 death, 1 recovery; cauterization of ulceration through the sigmoidoscope, 1 case, 1 recovery. He believed that left colotomy was quite the wrong treatment in cases of ulcerative colitis, because by it one could not get above the disease. The choice of operation lay between cæcostomy and appendicostomy. The case which died after appendicostomy was found post mortem to have ulceration in the small intestine as well as in the colon. Of the remaining cases 1 died a year later from another operation, and the remainder were well at various periods up to two and a half years. Of his own cases, two on whom he performed appendicostomy were both cured and were still quite well over one year afterwards. They were cases of very bad chronic ulceration in the colon, with severe symptoms dating back one year and one and a half years. Another case was too recent for him to say anything about the result, as he only operated a fortnight ago. He examined the colon through the appendicostomy wound, and the whole of that part was studded with ulcers the size of a threepenny or sixpenny piece, feeling very like typhoid ulcers in the small intestine. The patient had had symptoms of ulcerative colitis for three years, following dysentery which was contracted in Singapore, and had been treated in Singapore, and also since returning to London, by ordinary ipecacuanha treatment, correctly administered; and he agreed that in the majority of cases that treatment was not correctly carried out. He believed the operation of appendicostomy was the operation of choice. But occasionally there were cases in which the appendix could not be brought up, and the opening could not be made for irrigating the colon. But in the majority of cases the appendix could be used satisfactorily, and he thought the treatment a reasonable one. An ulcer on the leg was treated by keeping it clean and preventing the accumulation of discharge. Appendicostomy was based on the same idea—namely, that healing was rapid when the bowel was properly washed out. It was an

operation with practically no mortality, and was easy to do. If it failed one could resort to cæcostomy, but the latter was a most objectionable operation, and one to be avoided if possible. Appendicostomy had none of the objections which were inevitable with cæcostomy. The opening was at no time objectionable, and could be closed without an operation if desired.

Dr. BERTRAND DAWSON said that at the London Hospital the more these cases were gone into the more difficult it was to decide whether a given case was ulcerative colitis or not, because men judged data so differently. Some regarded blood as important evidence of ulcerative colitis, while others did not. His own opinion was that blood should not be taken as a criterion of ulcerative as against mucous colitis. Hiccough he regarded as merely indicating that the disease was associated with a dilated stomach. Many people with colon disease had dilated stomachs. Sprue was a good example of that, and in a proportion of cases of colitis the stomach was dilated. Tenesmus he regarded as evidence that the rectum was the seat of disease. To show the influence of the personal equation he took two recent years at the London Hospital and found that the colitis cases were classified as follows: 6 cases as ulcerative, 39 of all other kinds. Of the 6 cases, 5 had blood, 6 diarrhœa, 4 mucus. Of the 39, 21 had blood, 21 diarrhœa, 38 mucus. He would personally have put some of the 39 cases into the ulcerative group. Also in reading through the records of other hospitals he doubted whether all the cases called ulcerative colitis were really such. The signs most indicative of ulcerative as distinct from mucous colitis were hectic fever and marked wasting. Jenner when he was working out the difference between typhoid and typhus practically excluded all cases which he did not confirm post mortem. To ensure reliability we found it necessary to restrict our records to cases which had actually been proved post mortem. The doubtful cases were difficult to distinguish from mucous colitis.

Dr. Dawson raised the question whether the cause of the ulceration might not be situated above the large intestine. Might it not be that a toxic substance is formed in the small intestine which either travels down with the intestinal contents or is carried by the blood-stream and excreted into the colon? Flexner found that the toxin of dysentery bacillus injected into the blood-vessels produced ulceration. That the colon acts as an excretory organ is shown in mercury and lead poisoning. In certain reported cases of the former no sulphide of lead has

been found above the ileocaecal valve. Further, the colitis which is found associated with cases of uræmia shows how poisons excreted into the large intestine can produce ulceration. If the causal poison enters the large intestine from above or through the blood-stream an adequate explanation is afforded why operation only partially relieves the condition. The interesting case recorded by Mr. Makins at the last meeting brings out this aspect of the problem in an interesting way. He opened the ascending colon in a bad case; the patient gradually lost his symptoms, put on weight and was able to lead a fairly active life as long as the artificial opening acted and the faecal stream was diverted. If, however, this patient had a bout of anxiety or worry mucus and blood would for a time come through the opening from the proximal end of the bowel. These symptoms could not have been due to ulceration, or the operation would not have cured the patient.

The same idea impressed itself on Dr. Dawson's mind with regard to mucous colitis. In several cases of this condition he had the faeces examined because he noticed that sometimes they were pale, greasy and fetid, and analysis showed that the fat digestion was sometimes so deficient as to point to the pancreas being affected. In cases of sprue it had several times been shown that the pancreas had undergone change.

Turning to treatment at the London Hospital, appendicostomy had been found to do good in some cases, but the patients seemed unable to return to anything like the rough and tumble of life; if they attempted to do so they seemed to easily relapse. One or two cases treated by coli vaccine seemed to give encouraging results, and this supported the relation between the coli group of organisms and ulcerative colitis. But it is only fair to add that in other instances coli vaccine failed.

Dr. ZUM BUSCH: The relatively small number of cases of ulcerative colitis which have been described until now encourage me to make a few remarks based on 11 cases which came under my observation since 1903. In three of these cases a very marked and distressing hiccough existed, a symptom which has been described as very characteristic in this disease by Dr. Phillips. Two of the cases, a man and a little boy, were sent to me for immediate operation, both being supposed to suffer from intussusception. The very acute onset of the illness, the frequent passage of mucus and blood (often without any admixture of faecal matter), the tenesmus, and in the case of the man a very distinct resistance (due to some pericolicitis, I think), made the correct diagnosis difficult, and one can easily understand that, especially in the case of

children, exploratory operations have been done when ulcerative colitis was found instead of the supposed invagination. The high temperature which existed in both my cases, and the absence of sickness and signs of complete obstruction, saved me from this mistake. The child died on the sixteenth day of his illness, and the post mortem showed that no operation would have done good. A very severe hæmorrhagic ulcerative inflammation was found chiefly in the cæcum and the whole length of the colon, but it extended to the length of about 1 ft. upwards into the spleen and ended here with a sharply-defined line. The man recovered after a very severe illness of over four months' duration, and has now been quite well for over eighteen months.

Of the 11 cases which I have seen 7 were women, 3 men, and 1 a little boy. Five cases were treated without an operation, and 2 died, whilst 3 recovered. Small frequently-repeated doses of calomel and opium seemed to be of use, as well as frequent irrigations of the bowel. After having tried many different antiseptic and astringent lotions, I think that irrigations with a solution of peroxide of hydrogen (which should be prepared freshly from Mercks' perhydrol each time), and large enemata of warm olive oil give the best result. To the latter one may add dermatol, or bismuth, or xeroform powder. The sigmoidoscope may be of use in the later stage, when the mucous membrane of the lower bowel may be treated locally by its help.

In 4 cases (all women) I performed colotomy in the transverse colon. In every case the bowel was cut right across, so that no fæces could get into the distal end. In all the four cases—and I may add that they were exceedingly bad cases which had been ill for a long time, and were in an almost hopeless condition, so that the operation had to be done twice under cocaine only—was the operation followed by a very distinct and quick improvement of the general symptoms. The severe pain, which in my cases seemed to be chiefly due to the tenesmus and the almost continual dribbling of liquid fæces, ceased, the high hectic temperature became normal, and the patients gained greatly in weight. One woman, the worst case I have seen, was operated on, on November 6, 1903, in an almost dying condition. Her weight was 6 st.; on December 1 she weighed 6 st. 13 lb.; on December 13, 7 st. 6 lb., and by the end of March, 9 st. 6 lb. This woman has remained perfectly well since the operation—*i.e.*, more than five years. The ulcers, which at the operation were seen to extend beyond the artificial anus, healed quickly under local treatment. It was very easy to irrigate the proximal end as well as the distal end from the colotomy wound. Of the other three women,

one got quite well, but refused to let me try and close the anus; she has been free from all symptoms for three and a half years; in one case I could close the anus after eight months, and the patient has been well since (two and a half years). The fourth case was greatly benefited as regards her general health; she gained 28 lb. in a little over two months; her temperature became normal, but from time to time bleeding would occur both from the anus and colotomy wound. As she was believed to have suffered from syphilis she was treated with mercurial inunctions and potassium iodide. This displeased her so much that she left the hospital. She returned three years later in a dying condition, and died a few days later from a perforating gumma of the œsophagus. The post mortem showed that the mucous membrane of the sigmoid and colon descendens was almost entirely destroyed. There were numerous ulcers up to the colotomy wound, but not beyond.

A fifth case, also a woman, came under my care in November, 1903, suffering from intestinal obstruction. This was due to a prolapse of an ileocæcal invagination through an artificial anus in the ascending colon, which had been made at St. Thomas's Hospital years previously for ulcerative colitis. The invagination was reduced by laparotomy, and the patient, who was a confirmed morphiniste, left the hospital three weeks later. She returned a few months later suffering from septicæmia, due to an enormous decubital ulcer over the trochanter. She died two days after admission, and the post mortem showed a very long and narrow stenosis, but no ulceration, of the rectum and sigmoid. This explains why various attempts which were made at St. Thomas's Hospital to close the anus had ended in failure.

One other case, a man whom I have seen twice lately in consultation, had been operated on shortly before I saw him by another surgeon. As an appendicostomy was impossible, a small opening was made in the cæcum. This patient is still dangerously ill, and it seems doubtful whether the operation has done much good.

Of vaccine treatment I have no experience, but having never seen any real good following its use in other diseases, I should not think that it would do much good in ulcerative colitis.

All the five cases treated by colotomy recovered from the operation, which in two cases undoubtedly was of a life-saving character. In four cases the disease was arrested, and all symptoms disappeared; in one, however, although the general condition was greatly improved, the bleeding continued and ulcers were not healed three years after the operation. In only one case was it possible to close the anus and restore the patient to complete health.

It is, of course, impossible to draw general conclusions from such a small number of cases, but I think that ulcerative colitis is one of those diseases in which the physician should give the surgeon a chance to see the case early and watch its progress. Operations done on patients who are *in extremis* give little satisfaction to all concerned. Whether in early cases appendicostomy will shorten the disease and lead to a speedy and complete recovery seems to be, at least, very doubtful.

In those very bad and chronic cases in which I deemed an operation necessary, colotomy and the complete diversion of the fæces from the ulcerated mucous membrane seemed to offer the greatest chances for success, but, of course, one always runs the risk of a permanent artificial anus.

Dr. H. D. McCULLOCH desired to mention a case of tropical dysentery which he treated two years ago in Bournemouth with Dopfer's serum, supplied from the Pasteur Institute. The serum eliminated the bacillary phase of the malady, but the amœbic condition continued a little longer. It was a chronic case, the patient having contracted the disease in Java two years before, and been sent home. He was eighteen months in the Seamen's Hospital, where he had had all the usual methods of treatment; he was then invalided to Bournemouth, where he partly recovered, and came under Dr. McCulloch's care after a relapse. Dr. McCulloch succeeded in isolating Shiga's bacillus, as well as the amœba hystolitica.¹ The cicatricial condition of the colon was at one time very serious, and massive strictures occurred in the length and breadth of the colon. He thought of surgical intervention and the necessity of extirpating the colon, but it occurred to him to try the X-rays over the anterior pelvic region, in order to bring about resolution of the fibrous hyperphasia in the submucous layers of the colon. He believed that it had not been tried previously in that condition; the results were very gratifying. The patient was now in London, and had made a tour in Canada in the interval of the last two years without any further relapse.

Sir WILLIAM ALLCHIN, in reply, said that the remarks which were made at the previous meeting and on the present occasion were very valuable, and added very generally to the interest of the subject, and enlarged their knowledge thereon. At the same time, those remarks were of such a character that any observations he might now make

¹ *Brit. Med. Journ.*, 1906, ii, p. 332.

could scarcely be looked upon as of the nature of a reply, since there was nothing to reply to. However interesting and instructive Sir Patrick Manson's remarks might be, he could scarcely accept Sir Patrick's objection to the term "ulcerative colitis" as a valid one. In his preliminary remarks on the subject he (Sir William) carefully stated that at the present time the term could only be regarded as provisional. And from the clinical standpoint he took it there was the same objection to applying the term dysentery as there would be to applying the term ulcerative colitis; one was symptomatic and the other was anatomical, and he submitted that the terminology in either case would be equally correct. He took it that there was ulceration of the colon, more or less, in all of these cases. But the point upon which additional information was required, as he pointed out in his opening, was the cause of the condition. While they recognized that the structural changes in ulcerative colitis and in the conditions grouped under the term dysentery were substantially identical the symptoms were much the same, differing perhaps only in degree. The only point requiring elucidation was which of the various organisms Sir Patrick Manson had referred to was to be looked upon as the causal factor in the cases they were now discussing. It was a matter of great regret that no one had come forward in the discussion to deal with the matter from the purely bacteriological standpoint. While some thought the Shiga-Flexner bacillus was responsible, others named the *Bacillus coli* as the cause. More information was needed before the diseases could be regarded as defined from a causal point of view. The practical outcome from such a discussion as the present one would naturally have reference to the treatment. They had been warned by those who could speak from experience on the matter that operation for the condition was scarcely to be entered upon in any light-hearted way, such as the reading of certain records would seem to suggest was the case. He had been pleased to hear Dr. Dawson's remarks, and also those of Dr. Hale White, that at the present time there was considerable encouragement in the outlook in regard to treating such cases with coli vaccine, since such an observation entirely coincided with the speaker's own experience.

*STATISTICS OF ULCERATIVE COLITIS FROM THE LONDON
HOSPITALS, PROVIDED TO FORM A BASIS FOR THE
ABOVE DISCUSSION.*

Guy's Hospital.

By H. CHARLES CAMERON, M.B., and C. H. RIPPMAUN, M.B.

WE have analysed the records of Guy's Hospital for twenty years (1888 to 1907), examining both the clinical reports and the post-mortem records. In all 55 cases have been dealt with, in 38 of which an autopsy was performed and recorded. The cases recorded were, for the most part, found among the Medical Clinical Reports. A few cases only, and those all fatal, were taken from the Surgical Reports of the same years. This total of 55 cases does not, of course, by any means represent the number of cases in which ulceration of the colon was found post-mortem, and which appear in the index of the post-mortem records under the heading, "Ulcerative Colitis." We have omitted all cases in which it is stated that the ulceration was confined to the lymphoid follicles, a condition frequently met with in infants and with extreme rarity in adults. In ulceration, which was more widely spread, it was often a matter of considerable difficulty to decide whether any individual case should or should not be included in the statistics. When the ulceration was obviously a terminal complication and subordinate to some other well-marked disorder, it was felt that it was useless or misleading to include it in a statistical table of symptoms. Thus 9 cases were found in which the ulceration occurred, presumably as a terminal event, in puerperal septicæmia or in general peritonitis secondary to septic infection of the female pelvic organs. Similarly there were 3 cases associated with infective endocarditis, 15 cases associated with nephritis, generally of some standing, and four cases associated with an ascending pyelo-nephritis. In a single instance ulceration of the colon was also noted post-mortem in each of the following diseases: glanders, lobar pneumonia, heart failure, with much venous congestion of the viscera and severe general lardaceous disease. In some of these cases the appearance described did not differ markedly from that recorded in the cases of simple ulcerative colitis included in these statistics. All cases of tropical dysentery have also been excluded.

Moreover, among the cases which recovered, it was at times difficult to decide whether to include or to reject certain mild cases of short duration, which were of fairly frequent occurrence. The 15 cases which are recorded as recovering all presented well-marked symptoms of the disease. For these

reasons it is not suggested that a mortality of 40 among 55 cases represents accurately the danger to life.

NUMBER OF CASES ADMITTED IN INDIVIDUAL YEARS.

Year	No. of Cases	Year	No. of Cases	Year	No. of Cases
1888	2	1895	1	1902	3
1889	4	1896	2	1903	4
1890	4	1897	6	1904	3
1891	2	1898	2	1905	1
1892	2	1899	2	1906	4
1893	3	1900	4	1907	3
1894	2	1901	1		

Mortality.—Of 55 cases 40 died and 15 recovered; of the cases which recovered 7 were females and 8 males.

Sex.—Twenty-four were females, 31 males.

Age.—The average age of all cases was 35·8 years. The average age of the cases which recovered was slightly less than the average age of cases which died. In those who recovered the average age was 30·8. The average age of the females was distinctly less than that of the males: Females, 30·3 years; males, 40 years.

FEMALES.		MALES.	
Age	No. of Cases	Age	No. of Cases
15 to 20 years	5	— to 1 year	1
21 „ 30	10	1 „ 5 years	1
31 „ 40	4	15 „ 20	2
41 „ 50	4	21 „ 30	5
51 years	1	31 „ 40	5
	—	41 „ 50	12
	24	51 „ 60	5
			31

Residence Abroad.—In no case had the patient resided abroad.

Occupation.—No single occupation occurred in more than two instances. Two men were painters.

Previous Diseases.—Two of the cases had tabes dorsalis (18 and 21), 1 had hæmatomyelia with softening of the cord (53), 1 had a fracture of the spine with pressure upon the cauda equina (8). In three syphilis is mentioned, in 2 gout, and in 2 typhoid fever.

Relatives, Similar Disease in.—Two of our cases were brother and sister. In no other case is it mentioned that there had been similar illness among relatives or associates.

Length of Symptoms.—In 10 out of 41 cases, in which it was clearly stated, the symptoms had been intermittently present for many years. In 1 of these the total duration was said to have been fifteen years. In the other 9 cases symptoms had been present intermittently for some two or three years. In 45 cases there was a history of continuous passage of blood and mucus and of diarrhœa for a period which averaged three and a half months. Many cases, however, were only of a few days' duration. On the other hand, the average stay in hospital of the 40 cases which died was noticeably short. It averaged

twenty-four and one-third days, but if 3 exceptionally long cases be excluded, the average stay of the others was only fourteen and a half days before death occurred. The average stay in hospital of those who recovered was fifty-one days.

MODE OF ONSET IN 55 CASES.

Symptoms	No. of Cases
Diarrhoea noted as main early symptom	40
Constipation alternating with diarrhoea	8
Vomiting	16
Abdominal pain present	21
Abdominal tenderness	18
Rectal pain or tenderness	10
Rectal tenderness	5
Mucus ¹ in stools	24
Blood ² in stools	35
Sloughs in stools	2
Pus in stools	3

¹ Mucus is said to have been absent in 12 cases.

² Blood is said to have been absent in 5 cases.

The stools are usually described as watery, offensive, and ill-smelling, containing nearly always a little faecal matter, some mucus and blood, which is often said to be bright red.

Delirium was a not infrequent terminal event in acute cases. Marked tympanites was occasionally noted, and loss of weight and progressive anæmia was the rule.

Pyrexia.—The majority of the cases were pyrexial, especially the fatal and severe cases. The fever was, as a rule, of a remittent character. In 15 cases which recovered the temperature averaged 100'2° F.; in 15 which died, 102'2° F.

Blood Examination.—Where this was done the result usually showed a simple secondary anæmia. In 2 cases the anæmia was so profound that a diagnosis of pernicious anæmia was entertained. Thus in one case the count showed only 1,200,000 red corpuscles, with poikilocytosis and a colour index greater than one.

Rectal Examination.—In 9 cases nothing was felt *per rectum*. In 11 cases an ulcer was distinctly felt.

Death, mode of.—*Exhaustion*: 28 cases died of exhaustion. *Hæmorrhage*: In no case was hæmorrhage the direct cause of death. *Peritonitis*: 11 cases died of peritonitis. In 6 of these there was obvious perforation. One of the latter was thought to have been produced by the passage of a sigmoidoscope (50). In one colopuncture had been performed (20). *Hepatic Abscesses*: 1 case died with multiple hepatic abscesses (19).

In addition four cases showed localized collections of pus, one in the connective tissue around the rectum, one around the cæcum, one in the rectovesical pouch, and one in the neighbourhood of the splenic flexure. In one case a rectovaginal fistula had formed.

Post-mortem Appearances.—A short description of the post-mortem appearances in the 38 cases in which an autopsy was made is appended. In 15 of

these the rectum was involved, in 21 the cæcum, and in 1 the appendix. The ileum was affected, as well as the colon, in 3 cases. In 5 the solitary follicles were involved in such a way as to suggest to the observer that the ulceration had started in them. In 7 cases the wall of the gut is noted as being so friable that it tore on the slightest manipulation. Sometimes it was widely adherent to the abdominal wall.

REMARKS.

Mortality.—For the reasons stated above it is not suggested that the figures given represent the true mortality.

Repair.—Obvious cicatrization was observed in two cases (30 and 38), while in the latter (38) a new development of epithelium was described. In Case 9, fourteen months before death the patient was in a very critical condition. When seen by Dr. Pye-Smith at that time there was severe diarrhœa with passage of blood and mucus. The temperature was 104° F. at times, and an ulcer was felt *per anum*.

Course.—The contrast between the long period during which the symptoms had been intermittent or persistent in most cases, and the rapidity with which death supervened after admission, suggests that the disease is one in which an acute exacerbation is apt to occur to complicate and terminate a chronic affection.

Rectal Examination.—In 11 cases a rectal examination was successful in finding actual proof of ulceration. In some of these an anæsthetic was used and an ulcer felt which had, without the anæsthetic, escaped detection.

Operation.—In at least 7 cases the gut was so friable that it would have rendered any manipulation during life almost certainly fatal. In one case the use of the sigmoidoscope appeared to precipitate or cause perforation. In one case of peritonitis colopuncture had been done. In 15 cases the rectum was involved in such a manner as to render it unlikely that ileosigmoidostomy could possibly have been of benefit. In 1 case only was the appendix involved, but the cæcum was ulcerated in 21 cases.

Diagnosis.—Where the nature of the case was in doubt the confusion was most frequent with typhoid fever. One case was considered to have had recurrent attacks of appendicitis. In 2 cases the anæmia was so profound that a diagnosis of pernicious anæmia was suggested.

In 4 cases the possibility of "trophic" ulceration was considered, in 2 cases of tabes dorsalis, in 1 case of fracture of the spine with pressure on the cauda equina, and in 1 case of hæmatomyelia from injury with softening of the cord.

DESCRIPTION OF BOWEL.

- (3) The whole colon was more or less affected with subacute ulcers, many of which ran transversely to the gut; the ulcers were free from induration; the bases were ragged and the ulcers quite superficial; there were many dozen ulcers; none were chronic, and yet from the distance they extended round the gut none were very acute. (1889; P.M., 278.)

- (4) The large intestine was one stretch of severe acute ulceration, most marked in the transverse colon, where very little mucous membrane was left, and the muscular and serous coats exposed; the gut was very rotten and tore on removal from the abdomen; where the ulceration was less marked the mucous membrane was swollen, injected, and with punched-out ulcers; the ulceration probably started in the transverse colon; in part the wall was much thickened. (1889; P.M., 133.)
- (6) The mucous membrane was congested, and five ulcers were scattered throughout the large gut, some exposing and almost penetrating the muscular coat; the appearance was "quite indistinguishable from dysentery." (1889; T., 253; P.M., 205.)
- (7) There was extensive ulceration of the entire large intestine; some of the ulcers were small and might have originated from solitary glands; others were much larger (1 in. or more) and in places reached the peritoneal coat; just above the anus was a large ulcer; the bowel was not thickened. (1890; Good, 297; P.M., 309.)
- (8) The whole of the large intestine presented, on its surface, superficial patches of coagulative necrosis of recent date; in many the epithelium had not separated, and there was a layer of adherent lymph; the patches were $\frac{1}{2}$ in. to $\frac{3}{4}$ in. in size, and about one-twelfth of the colon was involved; in many the epithelium was gone, with resulting superficial excoriation; all were recent, *i.e.*, perhaps a few hours old; they were probably trophic from the injury to the cauda equina; the colon was empty. The liver was riddled with gas, containing holes from decomposition, though only six hours had elapsed since death. (1890; P.M., 283.)
- (10) Innumerable ulcers were found from the cæcum to the anus; some were small and limited to the solitary follicles, while others were large and undermined. (1890; P.M., 379.)
- (12) The large intestine was very friable and congested, in some parts more than in others; one foot above the anus there were three small ulcers, rather superficial, about the size of a sixpence, perhaps originating in the solitary glands; at the anus, and for 3 in. above it, the mucous membrane was almost entirely destroyed by ulceration; the muscular coat was exposed, and in places the cellular tissue also. (1891; P.M., 479.)
- (14) The upper third of the large intestine and the lowest part were healthy; the rest of the large intestine showed recent acute ulcers, many reaching the peritoneal coat, while several had perforated it; the largest ulcer was 2 in. in length; the total extent of mucous membrane involved was perhaps one-eighth of the whole; the follicles were not, apparently, affected; there was general peritonitis. (1892; P.M., 309.)
- (16) The upper part of the large intestine was extensively ulcerated, three-quarters of the mucous membrane being gone; the lower part showed a number of follicles distended with pus, which could be dislodged by a little pressure; this was noted by Dr. Pitt as unusual; the rectum was not involved. (1893; Wash., 310; P.M., 305.)
- (17) The entire large bowel from the anus to the ileocecal valve, with the appendix, was involved; some of the ulcers were small, circular, and acute, and others large with undermined, prominent, polypoid edges; the mesenteric glands were much enlarged, and two were cretaceous. (1893; H. W., 320; P.M., 442.)
- (18) There was an almost complete destruction of the mucous membrane from 20 in. below the cæcum to 7 in. above the anus, with a few more recent ulcers above this area of complete ulceration; the bowel in the left iliac fossa was bound down by adhesions, and these, when separated, exposed a small quantity of extravasated faeces; the bowel had perforated and was extremely rotten, so that it could not be removed intact. This patient had well-marked *tabes dorsalis*. (1894; P.M., 223.)
- (19) There was old ulceration in the cæcum, the wall of which was extremely thickened; the rest of the colon was involved in an acute ulcerative process; the ulcers were small, round, and superficial, the largest the size of a pea; it is not stated whether or not they originated in the solitary follicles. Multiple pyæmic abscesses were present in the liver. No thrombus was found in the portal vein. (1895; P.M., 45.)

- (20) During life there had been colopuncture performed twice; there was general suppurative peritonitis; the colon was brittle and tore readily; during life the transverse colon had perforated, but it was not proved that this was due to the colopuncture; from the anus to the ileocecal valve there were many ulcers, some small and round, beginning in the solitary follicles, others large and irregular, and separated by undermined mucous membrane. (1895; Clin., 548; P.M., 455.)
- (24) There was ulceration throughout except 2 in. in sigmoid region; coats thickened throughout; the rectum was involved. (1897; P.M., 375.)
- (25) The mucous membrane of the colon in its transverse and descending parts had almost completely disappeared, leaving only isolated patches of necrotic discoloured membrane; numerous patches of ulceration were present in the cæcum; several ulcers had perforated; one at the splenic flexure had given rise to a localized collection of pus; one in the cæcum had set up generalized peritonitis; the rectum was healthy. (1897; Wash., 429; P.M., 347.)
- (26) The ulcers were found in the cæcum and ascending and transverse regions of the colon, and the submucous and muscular layers were exposed in different parts; the ulcers were transverse and mostly on the rugæ. (1897; Clin., 153; P.M., 138.)
- (27) Old phthisis and recent tuberculous broncho-pneumonia were found at autopsy; the colitis, however, did not appear to be of a tuberculous origin; the cæcum was adherent to the parietes to such an extent that it had to be torn in removal; there was deep ulceration in its mucous membrane, exposing the peritoneum in places; there was superficial ulceration in the transverse and ascending parts of the colon; several small deep ulcers were present in the ileum; there was general peritonitis without perforation. (1897; P.M., 164.)
- (28) The gut from the ileocecal valve to the anus was extensively ulcerated; the ulcers mostly tended to enlarge in a transverse direction and were covered with a sloughing false membrane; the wall was extremely thick, in one place $\frac{3}{4}$ in. in thickness; the appendix and rectum were involved; the mesenteric glands were enlarged. (1897; Clin., 320; P.M., 266.)
- (29) The cæcum and the whole of the large intestine showed thickened and infiltrated walls with innumerable ulcers measuring $\frac{1}{4}$ in. to 2 in. across; most of these exposed the muscular coat, one the peritoneal coat, but no perforation had taken place; the intervening mucous membrane was healthy but undermined. (1898; G., 242; P.M., 290.)
- (30) The whole colon was ulcerated; in the ascending colon the process had gone on to cicatrization in places; 10 in. from the cæcum was a very severe patch 1 ft. long, extending down to the muscular coat in irregular patches, most marked in transverse rings; the sigmoid and rectum were acutely inflamed, the inflammation being most marked towards the anus. (1898; P.M., 2.)
- (31) The whole of the large intestine except the cæcum was ulcerated; all that was left was a number of isolated islets of a bluish colour; in the rectum the ulceration was less intense, and there were larger areas of mucous membrane; the cæcum was unaffected; a sharp line separated it from the ulcerated part. (1899; P. S., 253; P.M., 262.)
- (34) The entire large intestine showed a condition of ulceration, including the rectum; throughout the wall was thickened; the capsule of the liver "showed a cyst containing blood." (1900; P.M., 350.)
- (35) The whole of the mucous membrane of the large intestine was extensively diseased; there were discrete ulcers, running in a transverse direction across the gut, covered with yellowish adherent sloughs; the lower part of the gut showed only small patches of adherent mucous membrane, of a blackish purple colour; the exposed submucous coat was swollen and gelatinous. (1900; P.M., 77.)

- (36) Extensive ulceration was present in both caecum and colon; the majority of the ulcers were the size of peas, and one was 2.5 cm. in length; the intervening portions of mucous membrane were deeply stained with faecal pigment; the ulcers extended to and involved the muscular coat; there was also extensive broncho-pneumonia present. (1900; P.M., 42.)
- (38) Death from a gangrenous appendicitis, of which he had had symptoms for thirteen days; there was a condition of healed ulcerative colitis present, with patches of new mucous membrane, which showed in marked contrast to the normal mucous membrane. (1902; P.M., 241.)
- (39) The large intestine was thick walled with patches of superficial ulceration in the transverse colon; there were many patches of membranous exudation varying in size from a pea to a pin's head. (1902; H. W., 51; P.M., 83.)
- (40) The entire colon involved in an ulcerative process, almost completely denuding it of mucous membrane: *c.g.*, one area 27 cm. long contained only two or three tags of mucous membrane; the wall was very friable; there was general peritonitis, but no perforation. (H. W., 1903, 393; P.M., 412.)
- (43) Twelve cm. above the ileocaecal valve was a single sloughing ulcer, extending to 8 cm. below the valve; the walls were thick and the condition chronic; great fibrosis; the caecum was converted into a small sacculus, admitting only the tip of a finger. (P., 1903, 121; P.M., 258.)
- (46) Extensive ulceration of the colon, destroying two-thirds of the mucous membrane; the wall of the gut was thick; the rectum was even more severely involved in two places and its walls had given way, and there were two collections of pus, one on either side, in the pelvic connective tissue. (H. W., 1904, 202; P.M., 270.)
- (47) The mucous membrane of the rectum was ulcerated for 9 cm. above the anus, and the bowel was gangrenous; there was a collection of pus between the bladder and the rectum. (1904; Pitt, 72; P.M., 66.)
- (48) There was much ulceration of the large intestine and caecum; there were five ulcers, the largest $\frac{1}{4}$ in. in diameter, in the small intestine; one of these had perforated, giving rise to general peritonitis; there was also broncho-pneumonia and pyelonephritis. (Clin., 1905, 111; P.M., 86.)
- (49) The entire large intestine showed advanced ulceration, exposing, in many places, the muscular coat; shreds of gelatinous mucous membrane surrounded the ulcerated areas. (1906; P.M., 276.)
- (50) There was marked ulceration of the bowel from the middle of the rectum to the ileocaecal valve, all infiltrated and friable. (T., 1906, 198; P.M., 453.)
- (51) General suppurative peritonitis was present; there was a perforated ulcer in the lower sigmoid, circular, $1\frac{1}{2}$ in. in diameter, with undermined edges; a second smaller perforated ulcer was near by; there were numerous other ulcers from the caecum to the rectum; indications of peritonitis of older date than the recent suppurative peritonitis were present. (1906; P.M., 381.)
- (52) The large intestine was extensively ulcerated; large areas of submucous tissue and the peritoneum were exposed; the mucous membrane was swollen and undermined; the highest ulcer was just below the ileocaecal valve; 3 in. above the anus there was a fibrous structure, and just below it a very large ulcer. (1906; P.M., 454.)
- (53) There were small ulcers in the large intestine; numerous small haemorrhages were seen in the mucous membrane, which was congested. This patient had injured his spine, and there was haemorrhage into and softening of the cord. (1907; P.M., 597.)
- (54) In the caecum and ascending colon were three large ulcers of recent date with well-defined edges; each was covered with a black slough; the heart was much hypertrophied and there was aortic incompetence. (1907; P.M., 129.)
- (55) The transverse and descending parts of the colon were much thickened and ulcerated; only a few islands of mucous membrane were left; it resembled the condition seen in the dysentery of adults of some duration. (1907; P.M., 483.)

London Hospital.

By BERTRAND DAWSON, M.D., H. L. TIDY, M.B., and the late
J. E. SPICER, M.D.

(1) *Review of Cases in which Death occurred in the London Hospital
in the fifteen years 1894-1908.*

(2) *Some Statistics of various Forms of "Colitis" occurring during
the years 1906 and 1907.*

In the records of the London Hospital during the past fifteen years we find only 22 cases in which "ulcerative colitis" was found post mortem. In 6 of these the total duration of the illness to death was, in each case, less than two weeks. Since the average duration of the remainder is ten months, these 6 cases appear to fall into a separate group and will be referred to subsequently. The statistics of the remaining 16 cases will now be dealt with.

DETAILS OF 16 CASES.

These occurred as follows:—

	1894	1895	1896	1897	1898	1899	1900	1901	1902	1903	1904	1905	1906	1907	1908
Number	0	0	0	1	0	2	1	3	2	0	3	1	4	4	2

Etiology.

(1) *Age.*—The average age is: males, 41; females, 33. Mean, 37. The age mortality is:—

	Age 20-30		Age 30-40		Age 40-50		Age 50-60	
Males	...	0	...	4	...	0	...	3
Females	...	4	...	4	...	0	...	1
		4		8		0		4

With regard to the 4 cases occurring in persons over the age of 50 one notes: (a) In 3 of the 4 the duration was less than five months. (b) One died with the signs of myocardial failure, a second had granular kidneys, and a third died three days after admission. One may observe, then, from the table: (a) That ulcerative colitis tends to occur between the ages 20 to 40. (b) That in persons aged over 50 it may occur terminally. It may be added that in the cases diagnosed as ulcerative colitis during life in 1906 and 1907 the average age is 28 and the highest 37.

(2) *Sex.*—The cases are divided thus: males, 7; females, 9. Thus they are practically equal.

(3) *Residence.*—In only 2 cases, both men, had there been any residence abroad. Both were seamen who had been to the East and also South America.

(4) *Occupation*.—Except for the two seamen, no occupation occurs more than once.

(5) *Previous Disease*.—(a) Of intestines: In three cases previous intestinal trouble is mentioned. In one there had been, nineteen years previously, a similar attack lasting one year. A second, a seaman, had had "dysentery" when abroad. A third, who was a diabetic, had had an appendix abscess three years previously, and an attack of ulcerative colitis "with hæmatemesis" two years previously. (b) Other parts. One had had scarlet fever. The previous health of these cases seems to have been exceptionally good. Syphilis is not mentioned in any.

(6) No similar affection is mentioned in any member of the same family or household.

Symptoms and Signs.

Mode of Onset: Early Symptoms.—(a) Diarrhœa was noticed by the patient before admission in all cases except one female. In three females diarrhœa alternated with constipation. (b) Pain was present in all but 3 cases. "Pain" is here taken to include tenesmus as well as abdominal tenderness. Other symptoms occur less constantly at the onset.

One may observe: (a) Diarrhœa and abdominal pain are the early symptoms noted by the patient. (b) Other symptoms mentioned by various patients are:—

	Males	Females	Total
(1) Blood in stools ...	2	5	7
(2) Wasting ...	2	3	5
(3) Mucus in stools ...	2	1	3
(4) Vomiting ...	3	0	3

being given in their order of frequency. It will be seen that none of these important points are commonly observed by the patient. In two females the onset is described as "after confinement."

Symptoms and Signs as Observed in Hospital.—Numerically these are as follows:—

	Males	Females	Total
(1) Diarrhœa ...	6	9	15
(2) Constipation only ...	0	0	0
(3) Diarrhœa and constipation alternately	0	3	3
(4) Pain—Abdominal ...	6	3	9
Tenesmus ...	0	3	3
(5) Tenderness ...	4	4	8
(6) Stools—Blood ...	5	9	14
Mucus ...	4	7	11
Sloughs ...	1	2	3
(7) Vomiting ...	4	4	8
(8) Pyrexia: average maximum ...	101.6°	102.6°	102.1°
(9) Blood, mucus and diarrhœa ...	3	8	11

Rectal Examination.—This is recorded in 10 cases. In 5 the rectum appeared natural. In 1 the rectum was ballooned. In 4, ulceration, foul discharge, thickening of mucous membrane and other pathological conditions are described.

With regard to the *symptoms* it may be noted: (a) Pain and diarrhoea are the constant symptoms. Tenesmus is by no means common. Vomiting occurs in half the cases; it is not related to any group of cases, being found in those of short or long duration. (b) Pyrexia is invariable. In every case it rose above 100° F.

With regard to the *stools*: (a) Frequent loose motions prevail, in many cases being thirty to forty a day. (b) Blood is almost invariable, but in some cases repeated search has only revealed it rarely or in small quantities. (c) The 3 cases in which periods of constipation occurred were all patients in whom rectal examination revealed ulceration and other changes.

Complications.

(1) *Disease elsewhere*.—These are: myocardial failure, bronchitis and emphysema in 1 case, glycosuria in 1 case, mental excitement, fits or delirium in 3 cases (1 male, 2 female). There is no history of alcoholic excess in any of these 3 cases. Septic changes in the lungs found post mortem in 3 cases (*vide* post mortem).

(2) *Perforation and Peritonitis*.—Males, 2; females, 5. Total, 7.

(3) *Hepatic Abscess*.—This only occurred in one case, a seaman who had been abroad and had had dysentery.

Duration.—Average duration: Males, ten months; females, ten months. Mean, ten months. The longest duration was two and a half years, but 1 case had had a similar attack nineteen years previously which had lasted one year.

Mode of Death.

The figures with the duration are:—

	Males	Females	Total	Months
(1) Exhaustion	3	4	7	13
(2) Perforation and general peritonitis	2	5	7	7
(3) Hepatic abscess	1	0	1	—
(4) Myocardial failure	1	0	1	—

It is noticeable that: (a) Exhaustion and perforation divide the cases equally. (b) Perforation tends to occur much earlier than exhaustion. Not only is the average duration shorter, but of 6 cases which died within two months of the onset 4 were due to perforation and peritonitis.

Post-Mortem Appearances.

Intestines.—The small intestine was only affected in 2 cases, in both of which ulceration had occurred in the last few inches only. The large intestine: The pathological appearances may be divided into: (1) ulceration: (2) hypertrophy.

(1) Ulceration in every case affected portions of the whole of the large gut from the caecum to the end of the sigmoid. In all but 4 cases the rectum was also involved. The lymphoid tissue and mucous membrane are involved without

distinction. In many cases no normal mucous membrane at all remains, but the ulcers may be of any size. In depth they may extend to the peritoneum without any thickening on the peritoneal surface.

(2) "Hypertrophy" is used to include thickening of the mucous membrane or polypoid fibrinous masses. The distribution agrees with that of ulceration.

Although thus diffuse the changes are more noticeable towards the sigmoid. This is also shown by the region of the perforations: cæcum 2, ascending colon 1, transverse colon 0, descending colon 2, rectum 2. In only 1 case is any healing process mentioned. In general these changes are characterized by their variety of form but combined extent. They are limited to no tissue and confined to no stratum. Though individually irregular in distribution, together they are universally destructive.

Other Parts.—Hepatic abscess in 1 case, liver fatty in 1. Septic changes in lung in 3 cases; in 1 of these there was also septic thrombosis of the hæmorrhoidal veins. No other changes mentioned.

Bacteriological Observations.—Stools and blood, none; bowel-wall, 1 case recorded as showing no amœbæ in sections.

Treatment.—(1) Medical: By the mouth, bismuth and opium almost invariably. By the rectum, opium and starch enemata; injection of AgNO_3 . (2) Surgical: Appendicostomy in 3 cases, followed by injection of AgNO_3 protargol or cyllin. Cæcostomy in 1 case (in 1904).

THE SIX ACUTE CASES.

In these cases death occurred within a few days of onset. The cases are as follows:—

(1) Acute croupous pneumonia, apparently typical. Post mortem: signs of ulceration and hypertrophy confined to lower sigmoid and rectum, with perforation and general peritonitis. Not diagnosed.

(2) Strangulated umbilical hernia; operation; radical cure; developed general peritonitis. Post mortem: ulceration only, in large intestine; rectum not affected.

(3) Similar attack of diarrhœa and blood occurred in a fellow-workman. Post mortem: ulceration and thickening of mucous membrane.

(4) Apparently a general septicæmia. Commenced with septic throat.

(5) Diarrhœa and vomiting (aged 40). Post mortem: ulceration superficial only. Death from exhaustion.

(6) *Bacillus coli* abscess of the liver; opened and drained. Similar attack six years previously. Never out of England. Post mortem: ulceration only in lowest 8 in. of descending colon.

Although ulceration of the colon was present in these cases they do not seem to be classifiable with "ulcerative colitis," with the possible exception of the last.

Royal Free Hospital.

By ADELINE M. ROBERTS, M.D.

IN the twenty-five years, from 1883 to 1907, there have been only 2 cases of ulcerative colitis in the Royal Free Hospital, both of which died. One case, a female aged 19, was admitted in 1897 with a history of diarrhoea with blood and slime for a month. Faeces were passed both through the anus and the vagina. Tenderness on left side of abdomen. Vomited once. Died of hæmorrhage four days after admission. Large intestine ulcerated throughout down to anus with very little mucus membrane left except in rectum, where the ulcers were discrete. An ulcer just above anus had penetrated through posterior vaginal wall. The other case was a male child, aged 5 weeks, with congenital syphilis. Admitted for wasting, diarrhoea, and vomiting, which had lasted two days. Died five days later. Extensive ulceration of large intestine and peritonitis were found post mortem. A male, aged 46, was admitted collapsed and died in a few hours; a single ulcer the size of a sixpence was found on the posterior wall of the transverse colon, which had perforated.

St. Bartholomew's Hospital.

By T. J. HORDER, M.D.

CONCEPTION OF THE DISEASE.

ALTHOUGH 69 cases of "ulcerative colitis" are recorded as having been treated during these twenty years in the medical and surgical wards of the hospital, it is obvious, in examining the records, that a great variety of different entities have been included under this heading—so much so that any statistics based upon all the cases met with could not possibly serve any useful function. Many of the cases are of great interest; but, though diagnosed as ulcerative colitis, they cannot be accepted as such for the purpose of this collection. The interest lies in the individual cases rather than in their forming part of a series. In order, therefore, to ensure that only those cases bearing definite affinities with each other have been selected, it has been necessary to observe certain criteria. It might seem at first sight that in the cases going on to post-mortem examination there would, at least, be no difficulty; but this is not so, for ulceration of the colon, of a multiple sort, may occur as part of a more general ulcerating process in the alimentary canal, such as typhoid fever and tuberculosis, and the clinical condition may have been strictly determined by

the disease in the colon—that is to say, the symptoms and signs may have been those of an ulcerative colitis. Such cases, it seems to the present writer, should not be included. In the cases which recover the difficulty is much greater, since the number of conditions which simulate ulcerative colitis rather closely are so great.

The criteria to which cases have been, in the main, required to conform, to be included in the list as true ulcerative colitis, are as follows:—

(1) More or less persistent diarrhoea, the stools being frequent in number and possessing "dysenteric" characters.

(2) Blood has always been present in the stools at some time or other, with or without mucus, sloughs, and pus.

(3) Abdominal pain, general or local, with or without rectal pain.

(4) Some degree of fever.

(5) Loss of flesh.

(6) A duration of more than one week.

These criteria have been applied chiefly to cases which have recovered or in which, being fatal, no post-mortem examination was made. Cases in which the post-mortem examination has revealed a state of ulcerative colitis have, of course, always been included, whether or not the condition was diagnosed or suspected before death. It may be objected that the observance of these criteria is too strict; but believing, as the writer does, that ulcerative colitis is an uncommon and very grave disease, the comparatively small number of cases which are found to remain after rigid exclusion of the more miscellaneous group does not create any surprise. Certainly the cases remaining form a fairly uniform class and are capable of consideration as such.

CASES EXCLUDED.

These amount to no fewer than 27 out of the 69 cases. They are made up as follows:—

(A) *Without Evidence of Ulceration.*

(i) *Mucous Colitis* (4).—No evidence of ulceration was present in these cases. A few streaks of blood were passed in 2 of the cases, membrane in 1; all 4 cases occurred in women between the ages of 19 and 30; all 4 were afebrile; constipation had been present in all 4 cases before admission and was a troublesome feature in 2 throughout. All recovered.

(ii) *Colitis* (6).—One male, 5 females. Diarrhoea present in all cases, blood in 2, fever in 4. All recovered, the duration of the illness never exceeding fourteen days.

(B) *With Evidence of Ulceration.*

(i) *The ulcer was single, or at most one to four ulcers were present.*

(a) *Pyæmic Ulceration* (3).—In 1 case the lesion was found in a fatal case of otitis media, with septicæmia. In another case it complicated puerperal fever.

(b) *Trophic Ulcers in Myelitis* (2).—In 1 of these cases symptoms of ulceration were present during life; at the post-mortem examination two large ulcers were found, one in the cæcum and one in the sigmoid.

(c) *Ulceration of Old Dysenteric Colon* (2).—In both of these cases there was marked post-mortem evidence of healed ulcers following dysentery many years previously; in 1 case a single ulcer, and in the other case three ulcers were present in the colon.

(d) *Associated with Malignant Disease* (2).—In 1 case there was extensive ulceration of the sigmoid and lower part of the descending colon, the growth being in the left ovary: suppurative pyelonephritis was also present. In the other case there was an epithelioma of the vagina; the ulceration of the colon appeared subsequently to an operation for removal of the growth.

(e) *Associated with Appendicitis, and following certain Operations* (3).—Fairly extensive ulceration of the cæcum is not an uncommon post-mortem finding in fatal cases of appendicitis; one such case was recorded as ulcerative colitis; a second case followed operation for strangulated hernia, and a third followed an operation for renal calculus.

(ii) *The ulceration was multiple.*

(a) *Ulceration of the Colon in Tuberculosis* (2).—These 2 cases were excluded because there was post-mortem evidence of active tuberculosis elsewhere in the body (lungs in both cases, kidney in 1 case), with obvious tuberculous ulcers in the small intestine. The appearance of the ulcers in the colon in such cases often suggests a mixed infection, and this is sometimes proved to be so by bacteriological examination. In 2 of the cases included as true ulcerative colitis lesions of quiescent tuberculosis were present.

(b) *Ulceration of the Colon in Typhoid Fever* (2).—Both cases were excluded on account of ulcers being present post mortem in the Peyer's patches of the ileum, and the cases, clinically, being compatible with the diagnosis of typhoid fever. Both cases occurred during the earlier years of this investigation, when the diagnosis of typhoid fever received no assistance from the agglutination reaction or leucocyte counts.

(c) *Ulceration due to Actinomycosis* (1).—The disease affected the cæcum chiefly, the ascending colon to a less extent.

CASES INCLUDED.

The above cases being set aside for the reasons given, the number of cases to be properly designated "ulcerative colitis" is reduced to 42. These fall into the following five groups, and may be classified thus:—

- (A) Acute ulcerative colitis of infants (6).
- (B) Ulcerative colitis occurring in chronic (infective) endocarditis (2).
- (C) Ulcerative colitis of chronic dysentery (3).
- (D) Ulcerative colitis occurring as a terminal condition in certain chronic diseases, especially nephritis (7).
- (E) The "innominate" form of ulcerative colitis (24).

It is, of course, the last group of cases which is of the greatest interest, both on account of the apparent primary nature of the disease and on account of the lack of much knowledge of the etiology of the cases. The name "innominate," suggested by Dr. Gee, has been used to designate them provisionally, serving to mark them off from the others and also from certain of the conditions present in the excluded cases.

(A) Ulcerative Colitis of Infants.

The ages of the 6 cases were 3, 4, 6, 8, 10, and 18 months respectively. In all the cases the symptoms were acute or subacute, and consisted of diarrhoea (all cases), vomiting (4 cases, severe in 1), fever, and abdominal tenderness; the stools contained blood in 3 cases only; in 3 cases the condition of the colon was unsuspected during life; only one case recovered; broncho-pneumonia was present in 3 cases and may have been the primary condition; two of the cases were admitted to hospital as cases of "summer diarrhoea." Post mortem, the condition differed from that in the adults in being much more acute and, in most cases, less extensive. In 2 cases the ulceration affected the transverse and descending colon only; in 3 cases the ulcers were multiple but discrete, showing no tendency to coalesce.

(B) Ulcerative Colitis in Infective Endocarditis.

Case I.—Female, aged 26. Previous rheumatic fever. Mode of onset: Rheumatic pains and loss of flesh; diarrhoea for one month, with marked abdominal pain and tenderness in left iliac fossa; vomiting present; irregular fever, with periodic rises to 102° F. or 102.5° F.; signs of old-standing mitral disease and of chronic nephritis. Death from exhaustion. Post mortem: Many ulcers in colon, with irregular thickening of mucosa and pigmentation old and recent mitral endocarditis, with fungating vegetations; adherent pericardium; cardiac thrombosis; infarcts in spleen and kidneys.

Case II.—Female, aged 32. No previous illness recorded. Two months' history of rigors, severe abdominal and rectal pain, and profuse diarrhoea (8 to 16 stools daily, containing much blood); much abdominal tenderness; slight vomiting; irregular fever, 99° F. to 102° F.; under an anaesthetic the ulceration felt high up in rectum. Death from perforation in ten weeks from onset. Post mortem: From the ileocaecal valve to within 6 in. of the anus, extensive ulceration; small tags of blackened mucosa are all that remain of this lining muscle exposed in large areas; great distension and thinning of gut; caecum fenestrated by perforations; faecal abscess in pelvis; adherent pericardium; recent fungating endocarditis.

It is difficult to gauge the influence of the endocarditis in these cases. In the second case it may, of course, have been a terminal infection, as it certainly was in one of the "innominate" cases. In the first case, however, it is more likely that the endocarditis preceded the bowel ulceration, which may, in the first place, have been of the nature of the "vascular" ulcers of some authors.

(C) *Ulcerative Colitis of Chronic (Recurring) Dysentery.*

Case I.—Male, aged 47. Had dysentery in China three years previously. Onset with bloody stools three and a half months before observation; constipation occasionally; abdominal pain, not rectal; tenderness in right side; blood and mucus in stools; no vomiting; no fever. Rectal examination shows much mucus and oedematous mucosa, with dilated veins, but no ulceration. Duration four months. Treated by 2 per cent. argyrol injections morning and evening; later by silver nitrate injections ($\frac{1}{2}$ gr. to 1 oz.), castor oil and bismuth by mouth. Recovered.

Case II.—Male, aged 23. Had served in the Army in India two years previously, where he had suffered from dysentery and malaria. Onset with diarrhoea (2 to 6 stools daily); abdominal pain and tenderness; no vomiting; fever, 99° F. to 102° F., irregular; complicated by liver abscess, which was opened. Duration of illness, five months. Post mortem: active ulceration of whole colon; sigmoid and rectum healthy; liver shows many small abscesses in addition to the large abscess drained during life.

Case III.—Male, aged 37. Dysentery in South Africa in October, 1900. In October, 1905, alternating diarrhoea and constipation, with blood and mucus in stools, and much tenesmus. Stricture found 7 in. inside anus; stricture divided; symptoms relieved, but bloody stools persisted up till October, 1906; readmitted for these and rectal pain; no fever; treated by *appendicostomy*, silver nitrate and perchloride of iron solutions being used to irrigate the colon. Patient much relieved.

(D) *Ulcerative Colitis in Nephritis, &c.*

Of the 7 cases coming under this heading, 1 occurred as a terminal event in *cirrhosis of the liver*. The symptoms lasted fourteen days; there was no fever; 7 to 10 stools daily, containing much blood and mucus. Post mortem: The whole of the cæcum and the greater part of the descending colon were ulcerated, and the denuded mucosa was covered by a false membrane. The rectum was natural.

Another case occurred in a woman, aged 56, who had been known to suffer from *myxœdema* for nearly twenty years. The onset was with diarrhoea (3 to 6 stools daily) six weeks before admission; slight abdominal pain and tenderness. Rectal examination revealed ulceration and thickening as far as the finger could reach. The patient died of exhaustion a few days after admission. Post mortem: The cæcum, colon, and rectum showed numerous circular ulcers, coalescing to form irregular patches. The thyroid gland was very small and white.

The remaining 5 cases in this group occurred in patients suffering from *chronic nephritis*. All the cases were fatal. The type of kidney disease, as verified post mortem, was chronic parenchymatous nephritis in 2, subacute parenchymatous nephritis in 1, "small white kidney" (consecutive interstitial or mixed nephritis) in 1, and granular kidney in 1. Three were males, 2

females. The ages were 5, 30, 33, 35, and 46 years. In 2 of the cases no symptoms of the colon disease were manifest during life; in the others there was diarrhœa, with blood and mucus—in 2 cases for three weeks, and in 1 for four weeks—before death. Post mortem: The form of the ulceration did not differ essentially from that seen in the primary cases. In one case an infective origin of the ulceration was suggested by the recovery of streptococci in pure culture from the heart's blood and from the spleen.

(E) *The "Innominate" form of Ulcerative Colitis.*

Of this, the primary form of the disease, there were 24 cases. The following synopsis of these cases includes the points of chief importance in connexion with them:—

Sex.—Males 12, females 12.

Age.—Youngest, 24; oldest, 56; average age of males, 39'2; average age of females, 36'9. The disease is thus essentially one of middle life: 17 out of the 24 cases occurred in the ten years 35 to 45.

Occupation, Residence, and Family History.—No bearing upon the disease.

Previous Diseases.—Little or no influence, judging by the infrequency of recorded illnesses. Evidence of syphilis was present in 1 case, rheumatic fever had occurred in 2, and typhoid fever in 1.

Mode of Onset.—Gradually increasing diarrhœa was the first symptom in half the cases (13); in 4 cases the onset was somewhat sudden, with abdominal pain, loss of flesh, and acute diarrhœa; initial rigors were present in 2 cases; in 2 cases there was at first obstinate constipation, an attempt to relieve which, by purges, brought on diarrhœa with blood and mucus in stools.

Chief Symptoms Noted.—Diarrhœa (3 to 16 stools daily) was present in 21 cases, constipation in 3, and both occurred in 2; blood occurred in the stools in all cases, mucus is noted in 10, sloughs in 3, and pus in 2; abdominal tenderness occurred in 20 cases—general in 14, localized to left iliac fossa in 3, to splenic region in 1, to right side in 1, and to the whole course of the colon in 1; abdominal pain was noted in 18 cases, tenesmus in 7; vomiting occurred in 10 cases, being marked in 2; fever was present in 20 cases, most often ranging between 97° F. and 101° F., and being irregularly remittent or intermittent; loss of flesh and anæmia were present in most of the cases, the anæmia becoming very marked in 6 cases.

Ulceration Seen or Felt.—Examination of the rectum and sigmoid showed ulceration in no fewer than 7 cases. It is, therefore, often possible to establish the diagnosis in this way, especially by thorough examination under an anæsthetic (6 cases).

Complications and Terminal Events.—Liver abscess occurred in 3 cases; perforation occurred in 5 cases, in 1 of which the perforations were multiple in the cæcum; in 2 cases the patients had been treated for a gastric and duodenal ulcer respectively, verified in both instances post mortem; malignant endocarditis occurred in 1 case, pyopneumothorax in 1, parotitis in 1, purpura in 1,

jaundice in 1, and erythema nodosum in 1. The majority of the cases (16) died of exhaustion.

Recurrences.—The disease was recurrent in 5 cases—that is to say, one or more previous and similar attacks had occurred, with quiescent intervals of some months. In one case, a nurse aged 38, the first attack took place ten years previously; eight years previously she contracted typhoid fever, and from that time until she came under observation she had had attacks yearly, each lasting from two to three months; on admission to hospital she was very weak and emaciated; the diarrhoea was slight (2 to 3 stools daily), the stools containing blood, pus and mucus, and there was severe rectal pain; fever was present, 99° F. to 102° F.; the rectal ulceration was felt by the finger; the duration of this attack was six months; treatment was by silver nitrate enemata and by opium, and bismuth by mouth; the patient recovered from the attack.

Duration of the Attack.—Between one and three weeks, 4 cases; between four and six weeks, 7 cases; between eight and twelve weeks, 7 cases; between six and nine months, 4 cases; over twelve months, 2 cases. Excluding 2 acute cases lasting less than two weeks and 1 very chronic case, which lasted nearly three years, the average duration of the cases was 11·4 weeks.

Mortality.—As already stated, if a somewhat rigid exclusion be made of cases in which the evidence of multiple ulceration of the colon is not forthcoming, and if the cases of acute colitis with ulceration in infants be omitted, the mortality of the disease is very high. Of the 24 cases here dealt with only 3 recovered, giving a mortality of 87·5 per cent.

Bacteriological Examinations.—Observations have been but few. In 4 cases agglutination tests with *Bacillus typhosus* were reported as negative, and in 1 case similar tests with *Bacillus paratyphoid A* and *Bacillus paratyphoid B* were also negative; in 2 cases pure cultures of *Bacillus coli* were obtained from the heart's blood and from the spleen post mortem; no amœbæ were found, although searched for in 5 cases; no positive blood-cultures were obtained in 2 cases investigated.

Treatment Adopted in the Cases which Recovered.—This consisted of injections into the bowel of solutions of silver nitrate and of argyrol, and the administration of opium and bismuth by mouth; in 1 case *Bacillus coli* anti-serum was used without good results. The case of dysenteric ulceration successfully treated by appendicostomy has already been referred to.

Post-mortem Observations were made in 19 of the fatal "innominate" cases. There is nothing to add to the accounts usually given of the state of the large bowel in these cases; death had occurred with perforation in 5 cases; the "membranous" form of ulceration was noted to be present in 4; extreme polypoid changes were present in 4; considerable variation existed both in the distribution and in the depth of the ulceration; in 2 cases great thinning of the whole colon was noted, in 1 the greater part of the posterior wall of the descending colon being absent; in 2 cases there was considerable general thickening of the wall of the gut; pigmentation was present in 4 cases and very marked in 2.

Synopsis of Cases under observation in the Wards of St. Bartholomew's Hospital during the twenty-three years 1885 to 1907, inclusive.

(1) Total number of cases, 32.

(2) The following cases of disease of the colon have been excluded from the collection: (A) *No evidence of Ulceration*.—(a) Simple colitis, (b) mucous colitis, (c) mucomembranous colitis; (B) *Evidence of Ulceration*.—(a) Single ulcers: (i) Malignant disease, (ii) occurring in pyæmia, (iii) due to gall-stones and hydatid cysts, (iv) associated with appendicitis; (b) multiple ulcers: (i) found post mortem in typhoid fever, (ii) found post mortem in tuberculosis, (iii) due to actinomycosis.

(3) *Classification* of the 32 cases included.—(i) "Innominate" form, 12; (ii) occurring in tropical dysentery, 3; (iii) occurring in typhoid fever, 4; (iv) occurring in active tuberculosis, 2; (v) associated with nephritis, 5; (vi) associated with infective endocarditis, 2; (vii) associated with gastric and duodenal ulcers, 2; (viii) associated with cirrhosis of liver, 1; (ix) associated with myxœdema.

(4) *Age*.—Average, 36 years; oldest, 56 years; youngest, 5 years.

(5) *Sex*.—Males, 17; females, 15. Of the "innominate" cases: females, 7; males, 5.

(6) *Chief Symptoms Noted*.—(i) Diarrhœa in 24 cases, constipation in 3 cases, both in 2 cases; (ii) abdominal pain in 21 cases, tenesmus in 7 cases; (iii) abdominal tenderness in 22 cases, 3 in left iliac fossa, 1 in splenic region, 1 in right side, 1 in the whole course of colon; (iv) stools noted to contain blood in 17 cases, mucus in 10 cases, sloughs in 3 cases, pus in 2 cases; (v) vomiting noted in 11 cases; (vi) pyrexia present in 20 cases; (vii) rectal ulceration felt or seen in 7 cases.

(7) *Complications*.—Nephritis, 5 cases; perforation, 5 cases; liver abscess, 4 cases; malignant endocarditis, 2 cases; gastric and duodenal ulcers, 2 cases; tuberculosis, 1 case; parotitis, 1 case; erythema nodosum, 1 case; jaundice, 1 case; purpura, 1 case; pyopneumothorax, 1 case; myxœdema, 1 case.

(8) *Recurrences*.—History of previous attacks in 6 cases.

(9) *Duration of Present Attack*.—Average duration, eleven weeks. (This is excluding 2 chronic cases lasting respectively six years and three years, and 2 acute cases lasting one week.)

(10) *Mortality*, 87.5 per cent; recovered, 4 cases; died, 28 cases.

(11) *Mode of Death*.—Perforation, 5 cases; exhaustion, 23 cases.

(12) *Treatment of the Cases Recovering*.—In 3 cases opium, bismuth; argyrol and silver nitrate injections; in 1 case, appendicostomy.

(13) *Post-mortem observations* were made in 27 cases. "Membranous" form of ulceration was noted in 4 cases; extreme polypoid changes in 4 cases. Considerable variation noted in distribution of the ulceration and in its depth. In 1 case great thinning of the whole colon noted, and in 2 cases considerable thickening.

St. George's Hospital.

By A. J. JEX-BLAKE, M.B., and F. W. HIGGS, M.B.

NUMBER OF PATIENTS.

DURING twenty-six years (January, 1883, to December, 1906) 19 cases of ulcerative colitis were admitted to St. George's Hospital.

ANNUAL NUMBER OF CASES.

It is curious that during the first eighteen years under consideration there were only three cases of the disease, while in the last eight years there were sixteen cases.

GENERAL MORTALITY.

Of the 19 patients 9 died and 10 were relieved.

AGE AND AGE-MORTALITY.

Only 1 case occurred in infancy; none occurred during the rest of childhood and adolescence; the other 18 occurred in adults. The following table shows the age, number of cases, and mortality:—

Age	No. of cases	No. of deaths	Mortality percentage
Under 1 year	1	1	100
1 to 20 years	—	—	—
21 " 25 "	2	1	50
26 " 30 "	7	3	42·8
31 " 35 "	—	—	—
36 " 40 "	4	2	50
41 " 45 "	2	—	—
46 " 50 "	2	1	50
51 " 55 "	1	1	100
Over 55 "	—	—	—

SEX AND SEX-MORTALITY.

Of the 19 patients 14 were males (including 1 infant) and 5 were females. Of the 9 who died 7 were males and 2 females.

RESIDENCE.

Only 5 of the cases had been abroad—3 in Africa and 2 in India. Neither of the two who had been in India had had dysentery. Of the three who had been in Africa one had had dysentery and enteric fever there ten years previous to his coming under observation, one had had dysentery there nine years previously, and one had had several attacks of "dysentery" during the four years between his leaving Egypt and his admission into St. George's Hospital.

OCCUPATION.

The patients seemed to be in fairly good positions—perhaps above the average of hospital cases. Excluding the patients who had been in the Army abroad the occupations were those of butler, printer, butcher, cook, porter (2), masseuse, ticket-collector, labourer (2), compositor, mechanic, cobbler, school-master, music teacher, carman.

PREVIOUS HISTORY.

Nothing in the past history throws any light on the causation of the disease; in fact, the patients seem to have been singularly free from illness.

FAMILY HISTORY.

Nothing of importance was discovered.

THE DISEASE IN CHILDREN.

The only child affected was an infant aged 7 months, who had been breast-fed for four months and subsequently had been given Frame Food and Mellin's Food. It had severe diarrhoea and vomiting for ten days before it was carried off by a terminal broncho-pneumonia. There was no blood in the stools.

Post Mortem.—The colon, particularly its descending part, presented a markedly worm-eaten appearance, the mucous membrane between the small ulcers being injected and swollen. The solitary follicles, too, were markedly inflamed. The ulcers tended to run together and were mostly superficial. The small intestine was not ulcerated.

THE DISEASE IN ADULTS.

The Onset.

The onset was acute in 10 cases and gradual in 8. Diarrhoea was a prominent symptom in all the cases and was associated with blood in the stools. While the patients were under observation pyrexia was present in 14 cases and absent, or very slight, in 5. Malaise and shivering were noted in 3 cases and hæmatemesis in 1.

Signs and Symptoms.

The prominent symptoms were abdominal pain and tenderness, diarrhoea, tenesmus, and irregular fever. The stools contained blood at some period in all the adult cases. In the unfavourable cases the blood in the stools became more copious as they progressed towards a fatal termination. Mucus was present in the stools in 13 of the 19 cases, but sloughs were not noted in any case. Vomiting occurred in 5 cases and hæmatemesis in 1 of these (a fatal case in a woman aged 29). In no case was the vomiting very severe.

Three cases were examined with the sigmoidoscope. In one, a man aged 27, the sigmoid and upper rectum were seen to be ulcerated. In another, a woman

aged 42, the rectum was ballooned and a vascular ulcer was seen high up in it. In the third, a man aged 42, the sigmoid was very tender on manipulation, acutely inflamed, and bled very readily.

Digital examination of the rectum was recorded in 2 cases. In one nothing abnormal was felt; in the other the rectal mucosa was felt to be thickened.

Condition of Blood.

A blood-count was made in 2 cases. In one, a woman aged 42, it was as follows: Red corpuscles, 2,500,000; white cells, 6,500; hæmoglobin, 20 per cent. The differential white count showed: Polymorphonuclear cells, 60 per cent.; small lymphocytes, 30 per cent.; large lymphocytes, 4 per cent.; eosinophils, 6 per cent. In the other case, a man aged 25, only a white count was taken, which showed (a) 21,000, and (b), later, 50,000.

Bacteriology.

In 1 fatal case, a man aged 25, the blood was sterile; no other blood-cultivations were recorded. In one case the blood-serum feebly agglutinated the Shiga-Flexner bacillus; this was in the case of a man who had been in India but had not had tropical dysentery. The stools were examined in 3 cases for amœba coli, but it was never found. (See also *Post-mortem Appearances* [6].)

Course and Duration.

In 8 fatal cases the duration varied from three and a half weeks to two and a half years, the actual figures being: Three and a half weeks (male, aged 46), six weeks (male, aged 40), six weeks (male, aged 25), thirteen weeks (female, aged 29), seventeen weeks (male, aged 30), twenty-two weeks (male, aged 51), twelve and a half months (female, aged 39), and thirty months (male, aged 30).

In 9 non-fatal cases the actual duration was: Fourteen days (male, aged 30), one month (male, aged 38), four months (male, aged 48), four months (male, aged 27), two years (female, aged 27), three years (male, aged 24), four years (female, aged 42), seven years (female, aged 29), nineteen years (male, aged 41).

One case, a man aged 40, who has been ill about four months, is still under treatment; he is not improving. In 5 cases there were alternating diarrhœa and constipation, and the disease ran an intermittent course.

Complications.

In only 3 cases were there any complications; all these cases were fatal. In 1 case the cæcum perforated and death ensued from general peritonitis, in the second vegetative endocarditis was present, and in the third double parotitis.

Mode of Death.

In the 8 fatal adult cases the causes of death were: Acute peritonitis following perforation (1 case), hæmatemesis (1 case, the duration of the illness

being three weeks), toxæmia (1 case, length of illness six weeks), and exhaustion from long-continued diarrhœa (5 cases).

Post-mortem Appearances.

(1) Male, aged 30 (M.R. 1360, 1895).—Colon much distended; cæcum perforated by three or four small round thin-edged holes; local peritonitis; large intestine ulcerated from cæcum to anus; ulcers shallow, discrete, clean, punched out, not larger than a sixpenny-piece; liver fatty; lungs very œdematous and congested.

(2) Male, aged 25 (M.R. 435, 1902).—Several large ulcers in descending colon and sigmoid, edges undermined, the largest ulcer being as big as a five-shilling piece; smaller ulcers affect solitary follicles; small gut deeply congested in lower part; the spleen and kidneys contained *Bacillus coli*—the spleen looked healthy, the kidneys containing multiple white infarcts; ante-mortem thrombus in left ventricle of heart: vegetations on aortic cusp; liver fatty.

(3) Female, aged 29 (M.R. 862, 1902).—Cæcum and beginning of ascending colon contained several ragged ulcers with their bases formed by the serosa only; "honeycombed" mucous membrane; remainder of large gut free from ulceration. Nothing abnormal in other organs.

(4) Female, aged 39 (M.R. 1449, 1902).—Very extensive ulceration of colon, the ulcers being as large as a five-shilling piece and very little healthy mucosa remaining: base of ulcers formed by the muscularis; lower colon thickened; liver intensely fatty.

(5) Male, aged 40 (M.R. 671, 1905).—Large gut extremely friable; no perforation; ulceration of whole of large gut; mucous membrane almost entirely disappeared; follicular inflammation of small gut; the heart showed slight fibrosis (there was a history of syphilis); ante-mortem clot in right ventricle.

(6) Male, aged 51 (M.R. 1726, 1905).—Whole of colon and rectum extensively ulcerated—most at lower part, least in cæcum; ulcers mostly confluent, edges undermined. The heart's blood, post mortem, gave no agglutination with *Bacillus typhosus*, *Bacillus Shiga-Flexner*, or *Bacillus Gaertner*, but positive reaction with *Bacillus coli* (1 in 50). Emphysema and bronchitis.

(7) Male, aged 46 (M.R. 79, 1906).—Follicular enteritis; large intestine much distended—in upper part lymphoid tissue much swollen but not ulcerated, in lower part much ulceration, the lymphoid tissue being in all stages from slough to gangrene; the base of the ulcers formed by muscularis, edges clean, not undermined; bronchitis.

(8) Male, aged 30 (S.R. 501, 1908).—Body extremely emaciated; cæcum adherent to abdominal wall; old cæcostomy operation scar sound; colon, from hepatic flexure downwards, empty and collapsed; ascending colon and cæcum somewhat distended: the whole colon was very friable and felt thickened. The lesions of ulcerative colitis began a hand's breadth from the ileocæcal valve; at first a few polypoid masses of purple mucous membrane projected

from a scarred mucosa, then from the hepatic flexure onwards little mucous membrane remained; the gut-wall was thickened, plum-coloured on its inner aspect, thrown into folds, and covered with purple remnants of mucous membrane; there appeared to be no very actively progressing ulceration, and yet no scarring of old date. The stomach and small intestine contained excess of mucus and were in a state of subacute catarrhal inflammation; the liver was cloudy and fatty; the heart was atrophied, but not fatty; the lungs showed aspiration: broncho-pneumonia; the viscera gave no sign of lardaceous disease.

TREATMENT.

(A) Medical.

The various classical drugs for the control of diarrhoea and for the treatment of dysentery were given by the mouth, but without obviously producing any permanently satisfactory result.

Rectal injections of silver nitrate and of ichthyol were used in recent cases, but the results were not particularly encouraging. In 1 case (male, aged 30, who subsequently died) *Bacillus coli* vaccin was injected several times into the colon through a cæcostomy opening. There appeared to be slight temporary benefit but no permanent improvement.

(B) Surgical.

Four cases were submitted to operation, and 3 of these showed very definite improvement, and the fourth died. The operation performed was either appendicostomy or cæcostomy. In each case the colon was irrigated daily through the artificial opening with either silver nitrate, ichthyol or boracic acid solution. The details are as follows:—

(1) Female, aged 42 (M.R. 1728, 1905).—Ill four years; had appendicostomy performed in December, 1905, and was discharged apparently well in June, 1906; she was readmitted in June, 1907, with diarrhoea and melæna; the appendicostomy wound then still required dressing daily; temporary improvement was obtained by irrigation of the colon through the wound. In September, 1907, cæcostomy was performed. In May, 1908, patient was sent to a convalescent home much improved. She returned in July, 1908, in order to have the cæcostomy opening closed. This operation was performed and the wound healed well by first intention. Patient was discharged a month later much improved and gaining in weight.

(2) Male, aged 48 (M.R., 392, 1906).—Four months' history. Appendicostomy performed on March 30, 1906; colon irrigated, but no improvement. On May 8, 1906, cæcostomy was performed and the colon was again irrigated daily. Patient rapidly improved and was sent to a convalescent home five weeks later. He returned to hospital to have the cæcostomy opening closed, and this was done. His condition was then satisfactory, and he has not been seen since that time.

(3) Male, aged 30 (S.R. 501, 1908).—Two and a half years' duration; cæcostomy performed April, 1908; some temporary improvement; cæcostomy wound closed October, 1908; wound healed. Patient did not do well at any time and gradually went downhill. He died February, 1909.

(4) Female, aged 27 (M.R. 1459, 1908).—Duration of illness, three months. Appendicostomy performed on December 21, 1908, after medical treatment for one month had produced no improvement. After the operation the colon was irrigated daily with silver nitrate solution. Patient improved rapidly, and after several weeks the stools were practically normal. She was discharged apparently almost completely recovered.

St. Mary's Hospital.

By SIDNEY PHILLIPS, M.D., and D. W. CARMALT JONES, M.B.

BETWEEN 1884 and 1907 the diagnosis of ulcerative colitis was made in 19 cases, 11 males and 8 females, aged from 4 to 51.

OCCUPATION.

The patients followed various callings, both indoors and out, and there is no indication of any occupation being a predisposing cause.

PERSONAL HISTORY.

One patient had suffered from typhoid fever and 1 from rheumatism. No other previous illnesses are recorded.

FAMILY HISTORY.

The father and sister of 1 patient are said to have died of colitis. There was no other evidence of family predisposition.

SYMPTOMS AND SIGNS.

Onset was acute in 12 cases and gradual in 7. Diarrhœa was present in 18 cases, alternating with constipation in 3. Pain was referred to the abdomen in 17 cases and to the rectum in 1 case. Tenderness was general in 10 cases, referred to the cæcum in 2, and to the left iliac fossa in 1 case; two complained of tenderness on rectal examination. The stools contained blood in 15, mucus in 16, and sloughs in 3 cases. Temperature was above normal in 10 cases. Leucocytosis was noted in 4 cases.

DISEASE ELSEWHERE.

Bronchitis occurred in 1 case and swelling of joints in 1.

DURATION.

Four cases were of long duration—nine years, two years, nine months, and five months respectively; the rest varied between two weeks and two months.

MODE OF DEATH.

Death occurred in 8 cases—2 from exhaustion, 2 from peritonitis, and 1 from hæmorrhage.

POST-MORTEM EXAMINATIONS.

Post-mortem examinations were made in 6 cases; in all there was ulceration of the large intestine, described as extensive in 4 cases; embolisms in 1 case.

BACTERIOLOGY.

In one instance an organism of the *Bacillus coli* group was isolated, which did not agglutinate with the patient's serum.

TREATMENT.

Irrigation in 6 cases, astringents in 5, aperients in 2, and intestinal antiseptics in 1.

The figures here given cannot in any sense be considered as authoritative, because it is clear that special attention has not been drawn to the points upon which information is asked.

CASES ANALYSED IN TWENTY-FIVE YEARS = 20.

Year	Cases	Year	Cases	Year	Cases
1884	0	1893	1	1902	1
1885	1 (?)	1894	2	1903	2
1886	0	1895	0	1904	2
1887	0	1896	0	1905	3
1888	0	1897	0	1906	1
1889	0	1898	0	1907	4
1890	0	1899	0	1908	2
1891	0	1900	1		
1892	0	1901	0		

Three cases died in hospital (15 per cent.) at ages of 19, 30, 51.

A diagnosis of "colitis" was definitely made in 11 cases; in 9 other cases the diagnosis seems a probable one from the notes.

In the majority of cases no special stress has been laid on the points upon which information is asked.

Statistics from St. Mary's Hospital, 1884-1907.

Name	ETIOLOGY				SYMPTOMS AND SIGNS										
	Age	Sex	Residence in tropics	Personal history	Mode of onset	Diarrhea	Constipation	Pain	Tenderness	Stools			Vomit	Temperature	Blood examination
										Blood	Mucus	Sloughs			
— B.	51	F.	O	O	Gradual	+	—	+	Left iliac fossa	+	+	—	—	97° 102.5°	O
— F.	33	F.	O	—	Gradual	Alternate	+	+	General	+	+	—	—	N.	O
(1) — F.	25	M.	India	Dysentery	Gradual	+	—	+	Umbilicus, left loin	+	+	+	+	99°	O
— J.	28	M.	India	—	Gradual	+	—	—	—	—	+	—	—	N.	O
(2) — A.	30	F.	O	O	Gradual	+	—	—	—	—	+	—	—	N.	O
(3) — D.	19	F.	O	O	Gradual	+	—	+	General	+	—	+	—	103°	O
(4) — H.	33	M.	O	—	Abrupt	+	—	+	Cæcum	+	—	—	—	N.	O
— S.	39	F.	O	—	Gradual	+	—	+	—	—	+	—	—	O	O
— W.	4	F.	—	—	Acute	+	—	+	—	—	+	—	—	O	O
— P.	18	M.	India	—	Acute	+	—	+	+	+	+	—	O	N.	O
(5) — H.	28	M.	India	Typhoid, dysentery, malaria	Gradual	+	—	+	+	Frothy	white	—	—	101°	O
(6) — J.	21	M.	S. Africa	Malaria	Gradual	Thirty a day	Alternate	+	O	—	+	—	+	N.	O
— M.	30	M.	S. Africa	Dysentery	Acute	Alternate	+	O	O	—	—	—	—	101.8°	O
— B.	33	M.	W. Africa	Dysentery	Acute	+	—	+	O	+	—	—	—	N.	O
(7) — M.	25	M.	O	—	Gradual	Alternate	+	O	O	—	+	—	—	N.	O
(8) — E.	48	M.	—	Typhoid	Gradual	+	—	+	O	+	+	Slight	+	N.	O
(9) — N.	22	M.	O	—	Acute	Alternate	+	+	O	+	—	—	—	N.	O
— A.	60	F.	Texas	—	Acute	+	—	+	+	+	+	—	—	O	O
(10) — N.	22	M.	O	O	Gradual	—	—	+	Right iliac fossa, colon palpable	+	+	—	—	N.	O
(11) — D.	27	M.	India, S. Africa	Dysentery, hepatic abscess	Gradual	+	—	+	Slight, colon palpable	+	+	—	—	N.	O
A. W.	19	M.	O	Rheumatism	Acute	+	O	+	General	+	+	O	+	O	?
C. J.	48	M.	O	O	Acute	+	O	+	General	+	+	O	+	+	
L. M.	25	F.	O	O	Acute	+	O	+	General	+	+	O	O	+	?
E. T.	18	F.	O	O	Acute	+	O	+	General	+	+	O	+	+	Leucocytosis ?
A. B.	21	M.	O	O	Acute	+	O	+	General	+	+	+	O	+	
F. H.	17	M.	O	O	Acute	+	O	+	General	O	+	—	+	+	
E. P.	26	M.	O	O	Acute	+	O	+	General	+	+	—	O	+	Leucocytosis
J. S.	39	M.	O	O	Acute	+	O	+	General	+	+	—	+		

The cases numbered are those in which the diagnosis of colitis is made in the notes.

+ = The symptom is described in the notes.

By SIDNEY PHILLIPS, M.D., and D. W. CARMALT JONES, M.B.

COMPLICATIONS				Duration	Death	MODE OF DEATH			POST-MORTEM EXAMINATION		BACTERIA			Treatment
Disease elsewhere	Perforation	Peritonitis	Hepatic abscess			Exhaustion	Hemorrhage	Peritonitis	Hepatic abscess	Intestines	Rect	Stools	Bowel-wall	
—	—	+	—	Five weeks	+	—	+	—	Stercoral ulcers, sigmoid	—	O	O	O	Astringents
—	—	—	—	Nine years O	—	—	—	—	—	—	—	—	—	Irrigation Did not stay
—	O	O	O	Three years	—	—	—	—	—	—	O	O	O	Irrigation
—	+	+	—	Nine months	—	—	—	—	—	—	—	—	—	Linseed irrigation
—	—	—	—	Three weeks	+	—	+	—	Ulcer in colon	O	O	O	O	Irrigation through stump of appendix
O	—	—	—	Four weeks	—	—	—	—	—	—	—	—	—	O
—	—	—	—	Two years O	—	—	—	—	—	—	—	—	—	O
Bronchitis	—	—	—	Six months	—	—	—	—	—	—	—	—	—	Irrigation
—	—	—	—	Fifteen months	—	—	—	—	—	—	—	—	—	O
O	—	—	—	Nine months	—	—	—	—	—	—	—	—	—	Astringents
—	—	—	+	One week	+	—	—	+	Three hepatic abscesses	O	O	O	O	O
—	—	—	—	Three weeks O	—	—	—	—	—	—	O	O	O	O
—	—	—	—	Two weeks	—	—	—	—	—	—	—	—	—	Irrigation, aperients
—	—	—	—	Five months	—	—	—	—	—	—	—	—	—	O
—	—	—	—	Six months	—	—	—	—	—	—	—	—	—	Astringents, aperients
—	—	—	—	Two months	+	—	—	—	—	—	Bacillus coli, not agglutinated by serum	O	O	Astringents
—	—	—	+	Eight months	—	—	—	—	—	—	O	O	O	Astringents
—	—	—	—	Twenty-three days	+	—	+	—	Extensive ulceration	—	—	—	—	O
—	—	—	—	Twenty-six days	+	+	—	—	Extensive ulceration	—	—	—	—	O
—	+	—	—	Two months	+	+	—	—	Extensive ulceration	—	—	—	—	O
—	—	—	—	—	+	—	—	—	Extensive ulceration	—	—	—	—	O
—	—	—	—	—	—	—	—	—	Extensive ulceration	—	—	—	—	O
Joint swelling	—	—	—	Two months	O	—	—	—	—	—	—	—	—	—
—	—	—	—	—	+	—	—	—	—	—	—	—	—	—

— = The symptom is stated to be absent.

O = There is no information.

N = Normal.

St. Thomas's Hospital.

By HERBERT P. HAWKINS, M.D., and G. G. BUTLER, M.B.

NUMBER OF PATIENTS.

DURING twenty-five and a half years (January, 1883, to July, 1908) 80 cases of the disease were admitted into St. Thomas's Hospital.

GROUND'S OF DIAGNOSIS.

Of these 80 patients 40 died. In all but 1 (cp. Case 8) of these fatal cases the colon was found post mortem to be extensively ulcerated, this ulceration not being due to typhoid fever, tuberculosis, or other recognized cause. As regards the 40 patients who left the hospital alive, the diagnosis of ulcerative colitis is based on the following evidence:—

(1) The persistence of severe intractable diarrhœa, though occasionally under treatment constipation was noted.

(2) The passage of blood in considerable quantity continuously, or from time to time.

(3) The fact that in their signs and symptoms these 40 patients differed in no way from the 40 patients who died, except in the lesser severity and greater length of the illness.

Care has been taken to exclude from the list:—

(1) Cases of temporary diarrhœa of unknown origin.

(2) Cases of mucous colitis, of which condition the distinguishing characters are taken to be some irregularity of the bowels with constipation predominant, the occurrence of paroxysmal pain, the passage of mucus in notable amount from time to time, the absence of blood in the stools except perhaps in streaks occasionally during the height of a paroxysm.

(3) Cases in which the disease could have been acquired out of the United Kingdom.

(4) Cases in which there was a possibility of carcinoma or tuberculosis of the colon.

The probability of a correct diagnosis in these 40 cases is very high. But mistakes are possible. In 3 cases of supposed ulcerative colitis, carcinoma of the pelvic colon was subsequently found in one, tuberculosis in another, and colitis polyposa in the third. It is more likely that slight cases and early stages of ulcerative colitis have been excluded than that any other forms of colon disease have been admitted into the list.

ANNUAL NUMBER OF CASES.

The number of cases admitted per annum can hardly be taken as indicating any decided increased prevalence of the disease. In the years 1883 to

1907 and in the first six months of 1908 the numbers admitted were: 6, 1, 1, 0, 5, 1, 2, 5, 3, 1, 3, 1, 1, 2, 1, 2, 2, 2, 2, 4, 6, 3, 2, 5, 9, 10, respectively.

GENERAL MORTALITY AND RESULTS.

Of these 80 patients 40 died, a percentage of 50·0. As regards the other 40 patients the results may be classified thus:—

(1) No improvement	14
(2) Improved, but symptoms persisting	19
(3) Cure or great improvement	7

AGE AND AGE-MORTALITY.

As regards the age-incidence the chief points that arise are: (1) the comparative frequency of the disease in infants and its total disappearance from infancy to the age of 15. (2) Nearly 70 per cent. of the cases fall between the ages of 20 and 45.

Age	Cases	Deaths	Mortality per cent.
Under 1 year	7	7	100·0
1 to 5 years	1	1	100·0
5 " 10 "	—	—	—
10 " 15 "	—	—	—
15 " 20 "	3	1	33·0
20 " 25 "	8	4	50·0
25 " 30 "	8	3	37·5
30 " 35 "	16	7	43·7
35 " 40 "	13	7	54·6
40 " 45 "	11	3	27·2
45 " 50 "	5	1	20·0
50 " 55 "	4	3	75·0
55 " 60 "	3	2	66·0
Over 60 "	1	1	100·0
	80	40	50·0

SEX AND SEX-MORTALITY.

Of these 80 patients 41 were males, of whom 21 died, and 39 were females, of whom 19 died.

RESIDENCE.

Of these 80 patients 75 had never been out of England. About three-fourths of these were Londoners and the remainder came from the Home Counties. Of the 5 patients who had been abroad, 1 had been in India ten years before, where he had had malaria but no dysentery; 1 had been in India nineteen years before, but he had had no illness; 1 had been in India, Egypt and Bermuda, but he had had no illness and had not been out of England for twenty-three years; 1 had been in Egypt some years before, but he had had no illness, and 1 had been in Russia for a short time six years before admission. It may be added that during the twenty-five and half years in which these 80 patients were in hospital 49 other patients were admitted suffering from tropical dysentery in various stages, and a steady ratio of 8 British to 5 tropical cases

seems to be preserved. No increase of ulcerative colitis seems to have followed the close of the Boer War. With the exception of the cases of a hospital sister and a nurse, there is no evidence of hospital infection among the adults, but (see later) one child certainly seems to have acquired the disease in hospital.

OCCUPATION.

The patients seem to be of good position and well fed, and in these respects are perhaps above the average of hospital patient. All kinds of occupation, indoor and outdoor, manual and mental, are represented. Only one man was a painter. The list includes clerks, policemen, gardeners, and bricklayers, with a butler, piano-tuner, builder, house-agent, sawyer, stone-mason, iron-moulder, carpenter, farmer, candle-packer, blacksmith, &c. Of the females there were three teachers, two dressmakers, a cook, a hospital sister, a nurse and a school-girl; the remainder were either married or in service.

PREVIOUS HISTORY.

Nothing in the past history throws any light on the causation of the disease. These patients seem to be singularly healthy. In 58 there had apparently been no previous illness. Two had had syphilis and 2 had had typhoid some years before, 1 had had malaria, 1 appendicitis, 1 lead-colic, 1 pleurisy, 1 tuberculous glands in neck, 1 rheumatic fever, and 1 man was alcoholic.

FAMILY HISTORY.

There was some evidence of tuberculosis in the family of 13 of these 80 patients. Evidence of similar colon disease was obtained in 4 instances: (1) A sister was stated to be liable to frequent attacks of diarrhoea; (2) a sister was stated to have had English dysentery; (3) a mother was subject to diarrhoea; (4) a father died from ulcerative colitis.

THE DISEASE IN CHILDREN.

The 8 instances occurring in children under the age of 5 seem to lie apart from the adult cases, and perhaps have a different cause. In the first place the curious gap in the incidence of the disease (cp. above) between the ages of 5 and 15 seems to separate them from the adult cases. Secondly, they are more acute and severe, the average duration of life being only fourteen days. Thirdly, though ulceration was present in all these 8 cases, it was insignificant in comparison with the acute inflammation of the general mucosa, except in Case 2, detailed below. These cases cannot be regarded as instances of common summer diarrhoea. Only one of them occurred in the summer. October contributed two cases, January, February, March, April, and September contributed one each. Moreover, vomiting is an unimportant feature. It was described at the onset of 1 case, it was noted occasionally in another, and in 6 cases it did not occur at all. One case, and possibly another, seem to have originated in hospital. The onset was sudden and severe in all cases, in two being ushered

in with fits (no cerebral disease post mortem). The stools varied generally from two to eight in the day, but in 1 case there were twenty-eight stools in twenty-four hours. The motions generally consisted of yellowish or yellowish-green offensive liquid, with free blood in 5 cases, profuse hæmorrhage in 1 case, and much mucus in 1 case. In 4 cases fever was high (100° F. to 102° F.), in 2 cases slight (99° F. to 101° F.), and in 2 cases the temperature was normal or subnormal. There was little, if any, indication of pain in any case.

The duration of the illness in these 8 cases varied from seven to forty days, the average being fourteen days. All of them had been previously healthy, except Case 1 (detailed below). All of them died. As regards the conditions attending the onset of the illness, 2 cases require separate mention:—

Case 1, aged 5 months, admitted into hospital suffering from intussusception; operation and complete recovery, the bowels acting normally. She was discharged a month later. In the evening of the day on which she reached home she was suddenly seized with diarrhœa. This soon became urgent. She was readmitted into hospital five days later, and died on the fortieth day from the onset. Post mortem there was found acute inflammation of the colon, numerous small abrasions, and a few ulcers that extended down to the muscular coat. There was also one ulcer in the lower end of the ileum.

Case 2, aged $4\frac{1}{2}$. He ate a tomato with its skin. There was one loose action of the bowels that night, and profuse diarrhœa the next day, during which he passed the tomato skin. On the third day there were twenty-eight stools in twenty-four hours, with streaks of red blood. Three days later, diarrhœa continuing with passage of much mucus and blood, a soft ill-defined mass was felt in the right iliac fossa. On a diagnosis of intussusception, laparotomy was performed, and the mass was found to be a large thick œdematous cæcum. Death ten days later. Post mortem there was found great œdema of cæcum and ascending colon, with ulceration and punctiform hæmorrhages; the transverse colon was hardly affected, but the descending colon and rectum were deeply ulcerated from end to end.

By reason of its association with meningitis and its probable causation by pneumococcus, 1 further case may be mentioned:—

Case 3, aged 11 months. There was a sudden onset of symptoms indicating meningitis, and the child was admitted three days later. After being in hospital for sixteen days clearly suffering from basal meningitis, profuse diarrhœa with passage of blood began, and continued till death fifteen days later. Post mortem there was found purulent basal meningitis, and the colon was closely set with shallow ulcers. Pure cultures of pneumococcus were obtained from the meningeal pus and from the splenic blood, and numerous similar cocci were found in the exudate on the surviving mucosa of the colon.

MODE OF ONSET.

(A) *In 32 Adult Fatal Cases.*

In nearly all cases the onset was sudden, and no immediate cause could be traced. In 1 case the onset was so sudden that the patient described it as a

"sudden discharge of blood and matter." Either diarrhœa or hæmorrhage was the first symptom, and these cases thus fall into two groups:—

(1) In nearly all cases (26 out of 32) diarrhœa was the first symptom. Blood, either bright red or dark-coloured, was generally noted in the stools within a day or two of the onset. Even on the first day of the diarrhœa hæmorrhage occasionally occurred. Sometimes blood was not observed during the first week or two. Hæmorrhage, generally in considerable quantity, occurred in all 32 cases sooner or later. In many cases the patient stated that he had noticed "slime" in his motions from the first. The diarrhœa was often profuse from the onset, and in 1 case twenty stools were stated to have been passed during the first day. In 18 of these 32 cases the onset was attended with some abdominal pain, but it does not seem to have been severe. In 4 of these at an early period there was definite rectal pain and tenesmus (all 4 were subsequently found to have ulceration of the rectum). In 14 cases pain was slight or absent. In 1 case pain in the left iliac fossa preceded the diarrhœa by a week, and then diarrhœa and hæmorrhage set in. Vomiting at the onset seems to have occurred in 3 cases only. In 1 case the initial diarrhœa was accompanied by sore throat (death on thirty-fourth day); in 1 case the onset was marked by headache and shivering (death on forty-fifth day), and in another case by general malaise and aching in back and legs (death on seventy-second day). With these exceptions there does not appear to have been great constitutional disturbance at the commencement. Nothing can be said as to the occurrence of fever at the onset; but in most cases when they came under observation there was some rise of temperature.

(2) In a few cases (6 out of 32) it is clear that simple hæmorrhage from the bowel, without diarrhœa, occurred as the first symptom. In 2 of these there was actual constipation with the hæmorrhage, so that aperients were used before admission. In 1 of these hæmorrhage continued off and on for eighteen months, and in another case for one year before diarrhœa set in. In all these 6 cases diarrhœa came on sooner or later, and the condition of the colon at death does not seem to have differed from that found in the patients who suffered from diarrhœa from the first.

(B) In Adult Non-fatal Cases.

The mode of onset is the same as in the fatal cases. In 36 out of the 40 diarrhœa was the first symptom, but in all of these hæmorrhage occurred sooner or later. In 2 cases it is clear that hæmorrhage preceded the diarrhœa by some weeks, in another case by three months, and in a fourth case by one year. In 1 of these there was constipation at first, but in all of them diarrhœa occurred later. In 12 cases pain attended the onset of the diarrhœa, and in 3 of these it seems to have preceded the diarrhœa by a few days. In 1 of these there was rectal pain. In 4 cases there was vomiting at the onset. As a rule there was no great feeling of illness at first, but in 1 case malaise and shivering accompanied the onset of diarrhœa, and in another case there was aching in the back and legs.

SIGNS AND SYMPTOMS.

(A) In 32 Adult Fatal Cases.

Tongue : in 6 cases red, dry, glazed or cracked, in 1 case described as clean, in the other cases furred and moist, in 2 cases ulcers, in 1 case thrush. Mouth : in 1 case ulcers. Parotid bubo in 2 cases, suppurating in 1 case. Hiccough in 2 cases towards the end. Vomiting never a prominent feature, occurring in 1 case throughout the illness, and occasionally in 4 other cases. Stools : blood in all cases at one time or another, generally bright red, often profuse up to several ounces, fluid or in clots ; mucus generally visible, often in plates stained with blood ; sloughs and shreds mentioned in 3 cases ; pus mentioned in 2 cases ; fluid with very little faecal matter, often greenish yellow without blood, or reddish brown to dark brown with blood, often offensive. Pain : more or less in 18 cases ; generally griping before, during and after defæcation ; sometimes localized, and then generally in left iliac fossa ; in 2 cases pain described as severe ; rectal pain in 4 cases, and painful straining in 4 cases. Tenderness never very marked, moderate in 10 cases, and in 3 of these cases only present over descending and iliac colon. Distension never very marked ; slight in 6 cases, in 4 cases abdomen described as flat, sunken or retracted. Rigidity never noted except with peritonitis. Fever : in 6 cases (all acute) high, 99° F. to 102° F. and sometimes 103° F. or 104° F. ; in 15 cases slight, 99° F. to 101° F. ; in 11 cases little or none. Spleen palpable in 2 cases (in both found enlarged at post mortem). Perspiration mentioned in 2 cases with high fever. Emaciation : the illness may run a long course and end in death with very little loss of weight ; wasting was marked in 7 cases, and slight in 8 cases. Albuminuria in 5 cases (in 2 of these cases slight chronic interstitial nephritis at post mortem). Purpuric eruption terminal in 1 case. Rectal examination : in 8 cases ulceration was felt and seen ; in 1 case rectum described as roughened and bleeding easily ; in 2 cases nothing was felt but bleeding was produced ; in 21 cases apparently rectal examination revealed nothing.

(B) In 40 Adult Non-fatal Cases.

No material difference was noted as compared with the signs and symptoms in the fatal cases. Tongue : in 2 cases red and glazed, in 5 cases described as clean, in the other cases furred and moist. Vomiting in 6 cases occasionally. Stools : as in fatal cases ; in 38 cases blood was seen, in 2 cases it was seen only before admission into hospital. Pain never severe ; mentioned in 26 cases generally in connexion with defæcation ; in 8 other cases there seems to have been rectal pain or tenesmus. Tenderness mentioned in 23 cases. Distension mentioned in 2 cases. Rigidity never described. Fever : in 2 cases high, in 12 cases slight but often prolonged (one rigor in 1 case), in 26 cases little or none. Spleen palpable in 2 cases. Perspiration in 1 case. Emaciation in 12 cases, none in 28 cases (1 of these cases described as stout). Albuminuria in 2 cases. Indicanuria in 2 cases. Rectal examination : in 1 case rectum described as roughened, sensitive, and bleeding easily.

CONDITION OF BLOOD.

As regards red cells and hæmoglobin there appears to be no constant change. Sometimes even in cases of long duration the blood has a nearly normal value—*e.g.*, in 4 cases red cells were 5,037,000, 4,456,250, 4,075,000, and 4,281,000 respectively, with indices of 0·8, 0·9, 1·0, and 0·9. Sometimes there is a secondary anæmia—*e.g.*, in 2 cases red cells were 2,315,625 and 3,365,000 respectively, with indices of 0·6 and 0·8. In 1 case anæmia was profound, red cells numbering 1,762,000, with index 0·7, although hæmorrhage was not a prominent feature.

As regards leucocytes the few observations made show a low or normal count in 4 cases—*viz.*, 2,200 to 7,860, and a high count in 4 cases—*viz.*, 10,820, 10,500, 10,600, 24,900. In this latter case there were no special features. In none of these cases did a differential count show any great change, except that in 2 cases eosinophil cells amounted to 4·8 and 2·3 per cent., and in 1 case polynuclear cells were 54·6, and lymphocytes 34·0 per cent. In 1 case complicated by hepatic abscess leucocytes numbered 38,620, polynuclear cells being 78·0, and lymphocytes 14·0 per cent. This blood contained also 5·8 per cent. of myelocytes, and 3 normoblasts and 18 megaloblasts were seen in counting 500 leucocytes.

BACTERIOLOGY.

Observations are scanty.

(1) *Bacillus coli*: pure culture from stools of 1 case; pure culture (post mortem) from intestine, pus of hepatic abscess, and splenic blood in 1 case, but microscopically the submucosa of the colon in this case was infected with streptococci.

(2) *Bacillus coli* and *Bacillus proteus* obtained from the stools of 2 cases and the splenic blood of a third.

(3) Other bacteria: in 1 case (*cp.* Case 3) probably pneumococcus was present in the colon; in the case mentioned above the submucosa was infected with a streptococcus; in 1 case *Bacillus pyocyaneus*, a streptococcus, and a spore-forming bacillus were obtained from the stools.

(4) Shiga's bacillus was searched for, in 4 cases and not found. In 3 cases there was no agglutination-reaction with this bacillus; in 1 case it was agglutinated at once (dilution 1-100), and in 1 case a dilution of 1·20 produced good diffuse clumping in one hour, 1·50 gave slight clumping in one hour, and 1·100 gave no reaction.

(5) Amœbæ were searched for in a few cases and not found.

(6) Other tests: Calmette's and Widal's tests were negative whenever employed. In 1 case no agglutination was produced on two strains of paratyphoid bacilli.

COURSE AND DURATION.

As regards the course of the illness and the total duration, the most surprising differences appear. These differences suggest either that the resistance to the organism which produces the ulceration varies greatly in different individuals, or that the organism concerned varies in kind or virulence.

(A) In 32 Adult Fatal Cases.

In 21 of these the illness was continuous from first to last, and there was seldom any evidence of improvement. Death occurred :—

Period	No. of Cases
Less than one month	1
Second month	5
Third month	3
Three to six months	2
Six to eight months	2
Eight to ten months	2
Ten to twelve months	2
Eighteenth month	1
Two to two and a quarter years	2

In 11 cases the course of the illness was intermittent, and periods occurred when the diarrhœa ceased, or was replaced by constipation. During these periods the general health was fairly good, and the patient often returned to work and a normal life, but in most cases it was a matter of abatement rather than complete subsidence of the diarrhœa. The total duration of the illness in these cases, including the periods of improvement or temporary cure, was :—

Period	No. of Cases
Six months	1
Eighteen to twenty-four months	2
Two and a quarter years	1
Two and a half years	2
About three years	2
Four and a quarter years	1
Five to five and a half years	2

As an example of this intermittent disease :—

Case 4, a governess aged 34, was admitted into hospital in April, 1896, suffering from diarrhœa, passage of blood and mucus, and abdominal pain. In two months she seemed to be cured, and she was discharged in June. She remained well till September, when the same symptoms recurred, and she was again admitted in October. After one month of treatment she seemed to be well again, and she remained well till April, 1897, when the symptoms returned, hæmorrhage being severe, and she was readmitted in May. Treatment now had little effect, and in September left colotomy was performed without benefit. In January, 1898, ileosigmoidostomy was performed, and in April an attempt was made to close the colotomy wound, and she died three days later from general peritonitis. Post mortem there were found a large ulcer 3 in. by $\frac{3}{4}$ in. in the long axis of the splenic flexure, cicatrizing, its floor formed by muscular coats, a similar ulcer in the iliac colon, and numerous patches of grey scar tissue elsewhere in the colon.

(B) In 40 Adult Non-fatal Cases.

As regards the non-fatal cases the distinction between continuous and intermittent cases, which held good in the fatal cases, cannot be made. Certainly 17 cases out of this series had had no intestinal trouble before the illness

which brought them into hospital, but though they left the hospital alive and perhaps improved they cannot be said to have been cured, and it is exceedingly probable that they enter into the statistics of other institutions. In these cases the duration of continuous illness up to the date of discharge from hospital was :—

Period	No. of Cases
Four to nine weeks	5
Two to six months	3
Seven to twelve months	4
One to one and a half years... ..	2
Nearly two years	1
About three years	1
Five and a half years	1

As regards the remaining 23 cases, in many of them it is difficult to say when the illness began. Many of them gave a history of previous attacks of diarrhœa, and if the illness is measured from the first of such attacks to the end of stay in hospital the duration is very great. They may be roughly tabulated thus :—

Period	No. of Cases
Under one year	2
Under two years	4
Two to three years	1
Three to four years	6
Four to five years	1
Five to six years	2
Longer periods (10, 12, 15, 16, 17, 18, 18 years)	7

COMPLICATIONS.

With the exception of the changes described under "Post-mortem Appearances," there were practically no complications in any of these 80 cases. These patients appear to have been singularly healthy in all other parts save the colon. Unless the fever be so regarded, there was little evidence of toxæmia, except in the acute cases, and towards death.

MODES OF DEATH.

(A) *In 8 Children.*

All died from exhaustion due to diarrhœa and in much less degree to hæmorrhage.

(B) *In 32 Adults.*

The mode of death may be stated as exhaustion from diarrhœa in 9 cases, exhaustion from hæmorrhage in 1 case, exhaustion from diarrhœa and hæmorrhage in 8 cases, hepatic abscess in 1 case, peritonitis from perforation in 6 cases, peritonitis without perforation in 2 cases, peritonitis resulting from operation (closure of artificial anus) in 5 cases.

POST-MORTEM APPEARANCES.

(A) In the Colon.

In all but one of the 32 adult cases the colon was extensively ulcerated. One fact seems to come out clearly—viz., that the ulceration was nearly always deeper and more extensive in the cæcum and in the lower part of descending colon than elsewhere. Conversely in some cases the transverse colon was hardly affected. In 10 cases the rectum was ulcerated, and in 1 case the ulceration reached the anus. In 4 cases the appendix was noted as being ulcerated. In 2 cases only did the cæcum escape, and in 1 case only was there no ulceration below the splenic flexure. In 1 case the small bowel was affected. The ulcers varied in size from a pin's head to two or three square inches. The larger ulcers seemed to lie in the long axis of the bowel. They seemed to be of different ages in the same colon, and shallow abrasions occurred side by side with ulcers which exposed the muscular coats. The larger ulcers generally had the muscular coats as a floor; although perforation occurred in 6 cases, extension through the muscular coats to the peritoneum seemed to be very rare. Over considerable tracts only polypoidal islets of the mucosa remained in some cases.

A striking feature in some cases was the undermining of the mucosa, by which bridges of that tissue were left spanning an ulcer. This would seem to indicate that in some cases (as in amœboid dysentery) the submucosa was the layer first affected. In 1 case (fatal by hepatic abscess) there were closely set shallow ulcers scattered through the whole length of the colon, but in addition to this there were points of pus situated in the submucosa, around which streptococci were grouped. The wall of the colon was often thin and rotten. In 7 cases this was very marked, and in 1 of these it was almost impossible for this reason to deal surgically with the cæcum. In 2 cases, after long use of an artificial anus, the wall of the colon was thickened and the calibre diminished by contraction of its muscular coat, and this was sufficiently marked to render ileosigmoidostomy very difficult. The long history in some of the fatal cases and in the majority of the non-fatal cases (this long history consisting of previous attacks of diarrhœa with or without the passage of blood, separated by periods of comparatively good health) indicates that there does exist some tendency to cure, spontaneously or under treatment. Evidence on this point can be obtained from the post-mortem appearances:—

Case 5, man, aged 66. In addition to a large number of recent ulcers, large and small, there were several bands and patches of whitish cicatricial tissue, and about these parts the colon was thickened and narrowed. He died on the thirty-fifth day of his illness, but there was a history of previous attacks of diarrhœa. Colotomy was performed, but only just before death, and at too late a period to have induced the healing.

Case 6, man, aged 34. Diarrhœa and bleeding for four months before admission. He died after an illness of eight months. Some of the numerous ulcers in the colon showed a decided tendency to heal. Colotomy was performed, but only a few days before death.

Case 4, *cp.* above. Cicatrization of a large ulcer was proceeding.

Case 7, man, aged 39. Total duration of illness was two years. Colotomy was performed two years and ileosigmoidostomy twenty months before death. Smooth white patches were found here and there in the colon, with active ulceration in pelvic colon and rectum—*i.e.*, below the point of anastomosis.

Case 8, man, aged 34. Total duration of illness was two and a half years (frequent attacks of diarrhoea and bleeding). Colotomy was performed twenty months before death, and his death was due to peritonitis arising in connexion with performance of ileosigmoidostomy five days before death. The whole of the colon was much thickened and irregularly contracted, but no actual scarring could be recognized, and no ulceration was present.

Case 9, woman, aged 24. Long history for five and a half years of intermittent diarrhoea and hæmorrhage. Extensive ulcerations were found, but some ulcers were certainly healing. Colotomy was performed, but only just before death.

(B) In Other Parts.

Chronic interstitial nephritis was found in 2 cases; in both cases of slight degree. Considering the long history of diarrhoea and hæmorrhage in both cases it is unlikely that there was any connexion between the renal disease and the ulceration. Spleen: enlarged in 2 cases (palpable during life); in 12 cases it was small and under weight. Liver: stated to be fatty in 8 cases. Tuberculosis: obsolete apical in 1 case. Endocarditis: recent in 1 case. Hypostatic pneumonia: in 1 case. Goitre: in 1 case. Hepatic abscess: in 1 case. Erosions in the stomach: in a woman aged 28.

TREATMENT.

(A) Medical.

Varieties of drugs to the number of nearly forty were used in the treatment, and except in reducing the number of stools per diem they seem to have had little effect. Irrigation with silver nitrate, creolin, carbolic acid, boracic acid, and hydrogen peroxide was employed. Treatment with ipecacuanha and with magnesium sulphate seems to have been useless. Antidysenteric serum was used in 2 cases, but it was combined with other forms of treatment. Two cases were treated with coli-vaccine, of which one improved considerably and the other left hospital unrelieved.

(B) Surgical.

(a) *Fatal Cases.*—(1) In 8 cases colotomy or ileostomy was performed at a very late stage of the illness. Two of these died from resulting peritonitis; the other six died unrelieved within a week or two. (2) In 7 cases colotomy or ileostomy was performed at an earlier stage. Of these 1 died forty-three days later without improving; 1 died thirty days later, having experienced some relief. In the other 5 cases considerable improvement, and in 1 case probably complete cure, resulted; in 2 of these ileosigmoidostomy was performed four

months after the first operation and was successful in both, but both died later from peritonitis in an attempt to close the artificial anus; in the other 3 cases ileosigmoidostomy was performed five, eight, and nine months after the first operation, one dying at once from collapse, the other 2 from peritonitis. In 1 of these cases an ulcer visible in the rectum had been excised at an earlier period.

(b) *Non-fatal Cases.*—In 1 case transverse colotomy was performed; she was discharged cured, or greatly improved, two months later; two years later she remained in fair health, though occasionally liable to diarrhoea. The colotomy wound never acted as an artificial anus, and it probably had no share in the result except as facilitating irrigation. In 1 case colotomy was performed, and the artificial anus acted efficiently for seven months, when it was successfully closed. The patient left hospital apparently cured. In 2 cases appendicostomy was performed, and irrigation employed. One of these was discharged three months later unrelieved, the other one year later either cured or much improved.

University College Hospital.

By SYDNEY A. OWEN, M.B.

*Statistics from 1883 to 1908.*¹

TABLE I.—SUMMARY OF CASES.

Year	RECOVERIES		DEATHS		Total
	Males	Females	Males	Females	
1883	—	—	1 (1)	—	1
1884	—	—	—	1 (2)	1
1885	1 (3)	—	—	—	1
1886	1 (5)	—	1 (4)	—	2
1887	—	1 (6)	—	1 (7)	2
1888	—	—	—	—	—
1889	1 (10)	—	2 (8, 9)	—	3
1890	—	—	—	—	—
1891	—	—	—	—	—
1892	—	—	1 (11)	—	1
1893	1 (12)	—	—	—	1
1894	2 (13, 14)	3 (15, 16, 17)	—	—	5
1895	2 (19, 20)	1 (18)	—	—	3
1896	—	1 (20)	—	—	1
1897	2 (22, 25)	2 (23, 24)	—	—	4
1898	1 (27)	1 (26)	—	—	2
1899	2 (28, 29)	—	—	—	2
1900	—	1 (30)	—	—	1
1901	1 (32)	2 (31, 33)	—	1 (34)	4
1902	—	2 (36, 37)	1 (35)	—	3
1903	—	—	—	1 (38)	1
1904	2 (39, 40)	—	—	—	2
1905	1 (41)	—	—	—	1
1906	1 (42)	2 (43, 44)	—	—	3
1907	3 (47, 48, 49)	1 (45)	1 (46)	—	5
1908	—	—	—	2 (50, 51)	2
Total (25 years)	21	17	7	6	51

¹ In all tables figures in parentheses refer to number of case for reference.

TABLE II.—AGE-INCIDENCE.

(A)			
	Males		Females
Oldest ...	45 years (4)	...	76 years (23)
Youngest ...	17 ,, (39)	...	16 ,, (7)

(B) Number of Cases occurring between the following Ages.

Age in years	Males	Females
15 to 20 ...	1 (39)	2 (7, 50)
21 ,, 30 ...	14 (1, 3, 5, 8, 12, 13, 14, 20, 29, 40, 41, 42, 46, 47)	7 (2, 15, 16, 17, 26, 31, 37)
31 ,, 40 ...	11 (9, 10, 11, 19, 25, 27, 28, 32, 35, 48, 49)	8 (6, 18, 21, 24, 30, 34, 43, 44)
41 ,, 50 ...	2 (4, 22)	2 (33, 51)
51 ,, 60 ...	—	2 (36, 38)
61 ,, 70 ...	—	1 (45)
71 ,, 80 ...	—	1 (23)
Total cases ...	28	23 = 51

(C) Age-incidence in Fatal Cases.

MALES			FEMALES		
Case	Age in years		Case	Age in years	
1	27	...	2	21	...
4	45	...	7	16	...
8	26	...	34	31	...
9	39	...	38	52	...
11	35	...	50	19	...
35	35	...	51	42	...
46	25	...	—	—	...
Total cases ...	7		6	=	13

TABLE III.—RESIDENCE (? TROPICAL OR ELSEWHERE).

	Males	Females
1 No definite evidence ...	16 (1F, 4F, 5, 10, 11F, 12, 19, 20, 27, 28, 29, 32, 35F, 39, 40, 46F)	22 (2F, 6, 7F, 15, 16, 17, 18, 21, 23, 24, 26, 30, 31, 34F, 36, 37, 38F, 43, 44, 45, 50F, 51F)
2 Egypt ...	1 (3)	—
3 India ...	4 (8F, 22, 41, 42)	—
4 Rangoon, Burma, India	1 (9) F	—
5 Malta, Ceylon, India ...	1 (13)	—
6 Poland ...	1 (14)	—
7 India, Afghanistan, Malta, Gibraltar ...	1 (25)	—
8 Burma, India ...	—	1 (33)
9 India, South Africa ...	1 (47)	—
10 South Africa ...	1 (48)	—
11 West Africa ...	1 (49)	—

Total—28 males; 23 females.

“F” in this table refers to fatal cases.

TABLE IV.—ANALYSIS OF OCCUPATIONS.

MALES.		FEMALES.	
Occupation	No.	Occupation	No.
Potman ...	1 (1)	Housewife ...	8 (15, 21, 24, 34, 36, 38, 43, 44)
Army ...	3 (3, 22, 47)	Parlourmaid, Domes- tic, &c. ...	8 (16, 17, 18, 26, 30, 31, 45, 51)
Platelayer ...	1 (29)	Nurse ...	1 (33)
Labourer ...	2 (4, 42)	Silver case maker ...	1 (2)
Carriage fitter ...	1 (5)	Teacher ...	2 (6, 37)
Machinist ...	1 (46)	Dressmaker... ..	1 (7)
Engineer ...	1 (41)	Nil ...	2 (50, 23)
Engine fitter ...	1 (49)		
Railway porter ...	1 (8)		
Horsekeeper ...	1 (40)		
Groom ...	1 (9)		
Carman ...	1 (32)		
Cook ...	1 (10)		
Painter ...	1 (35)		
Decorator ...	1 (11)		
Waiter ...	1 (12)		
Butler ...	1 (13)		
Tailor ...	1 (14)		
Timber-yard manager...	1 (19)		
Shop assistant ...	1 (20)		
Butcher ...	1 (25)		
Turner ...	1 (27)		
Hospital Attendant ...	1 (28)		
P.O. telegraphist ...	1 (39)		
Miner ...	1 (48)		
Total	28	Total	23

TABLE V.—DURATION OF ILLNESS AS GAINED FROM THE HISTORY OF THE PATIENT.

MALES		FEMALES	
Case	Duration prior to admission	Case	Duration prior to admission
1	Three weeks (fatal)	2	Fifteen and a half weeks (fatal)
3	Three months	6	Four months
4	Six weeks (fatal)	7	Four weeks (fatal)
5	Four months (similar attack four years ago)	15	Eight months
8	Three weeks (fatal; dysentery ten years ago)	16	Twenty-one weeks (similar attack two and a half years ago)
9	Three weeks (fatal; third attack; first attack eleven and a half years ago)	17	Fourteen years
10	Four days	18	Eight and a half years
11	Three weeks (fatal)	21	Eight years
12	Five years	23	One day
13	Two days	24	Sixteen months
14	Nine months	26	Five days
19	Thirty-five weeks	30	Five years (two previous attacks)
20	Ten months	31	Nineteen days
22	Seven months	33	Five years
25	Four days	34	Nine months (fatal)
27	Fifteen months	36	Two weeks
28	Twelve days (first attack three months ago)	37	Twenty-one months
29	Four months (dysentery seven years and two years ago)	38	"Many years" (fatal)
32	Eighteen months	43	Six years
35	Nineteen days (fatal; colic twenty years ago)	44	Nine months
39	Six weeks (four similar attacks previous year)	45	Ten years (similar attacks last ten years)
40	Four weeks	50	Nine months (fatal)
41	Five weeks (fifth attack; first attack three years ago)	51	Six weeks (fatal)
42	One week (dysentery six years ago)		
46	Four weeks (fatal)		
47	One year (two attacks of dysentery; two years and five years ago)		
48	Three and a half years		
49	Nine months (in-patient twelve years ago for passing blood)		

TABLE V—(continued).

(B) Total Duration of Case in Fatal Cases.

MALES		FEMALES	
Case	Duration	Case	Duration
1	About eleven weeks (first attack)	2	About nineteen weeks (first attack)
4	About ten weeks (first attack)	7	About five weeks (first attack)
8	About seventeen weeks (dysentery ten years ago)	34	About ten months (inflammation of bowels two years ago)
9	About four weeks (third attack)	50	About nine months (first attack)
11	About eight weeks (first attack)	51	About seven weeks (first attack)
35	About seven and a half weeks (colic twenty years ago)	38	About three and a quarter months (liable to diarrhoea many years)
46	About seven weeks (first attack)	—	—
7	—	6	—

(C) Mode of Death.

MALES		FEMALES	
No. of cases		No. of cases	
Exhaustion ...	7 (1, 4, 8, 9, 11, 35, 46)	5 (2, 7, 34, 38, 50)	
Hæmorrhage ...	(?) 1 (35)	0	
Perforation ...	3 (1, 11, 46)	1 (51)	
Peritonitis ...	3 (1, 11, 46)	2 (34, 51)	
Hepatic abscess ...	1 (8)	0	

TABLE VI.—PREVIOUS DISEASE.

(A) Intestinal.

MALES		FEMALES	
Nature of Disease	No. of cases	Nature of Disease	No. of cases
(a) No previous intestinal disease ...	14 (1, 3, 10, 11, 12, 13, 14, 19, 20, 22, 27, 32, 40, 46)	12 (37, 38, 43, 44, 50, 51, 6, 7, 18, 21, 26, 36)	
(b) One similar attack ...	5 (5, 28, 39, 48, 49)	2 (16, 45)	
More than one similar attack ...	2 (9, 41)	—	
(c) Dysentery ...	4 (8, 29, 42, 47)	2 (15, 33)	
(d) Chronic colitis ...	—	1 (30)	
(e) "Inflammation of bowels" (one attack) ...	1 (25)	1 (34)	
"Inflammation of bowels" (more than one attack) ...	—	1 (2)	
(f) "Colic" ...	1 (35)	—	
(g) Enteric fever ...	3 (4, 8, 48)	3 (17, 23, 31)	
(h) Tapeworm ...	—	1 (24)	
(i) Hæmorrhoids ...	1 (49)	1 (17)	

(B) Disease elsewhere.

(a) No previous disease elsewhere ...	16 (1, 8, 10, 11, 14, 19, 20, 29, 32, 39, 40, 41, 42, 46, 47, 48)	13 (2, 6, 7, 15, 24, 26, 30, 31, 34, 37, 38, 43, 45)
(b) Gonorrhoea or syphilis ...	5 (3, 5, 12, 13)	No details
(c) Pleurisy ...	2 (1, 28)	1 (23)
(d) Phthisis ...	1 (12)	—
(e) Rheumatism ...	3 (12, 13, 35)	4 (16, 21, 23, 50)
(f) Smallpox ...	1 (4)	—
(g) Influenza ...	—	1 (36)
(h) Measles ...	—	1 (44) ¹
(i) Gastric ulcer ...	—	1 (18)
(k) Convulsions ...	1 (25)	—
(l) Jaundice ...	1 (27)	—
(m) Ague ...	1 (22)	—
(n) Intermittent and jungle fever ...	1 (9)	—
(o) Cyst of broad ligament ...	—	1 (33)

¹ This patient dates her illness from severe attack.

TABLE VII.—EARLY SYMPTOMS.

(Gained from history for the most part.)

		Males	Females
1	Pain (gripping, colic, &c.) ...	20 (1, 4, 5, 11, 12, 13, 14, 20, 27, 28, 29, 32, 35, 39, 40, 42, 46, 47, 48, 49)	19 (2, 6, 7, 15, 16, 17, 18, 21, 23, 24, 26, 30, 31, 33, 34, 37, 38, 44, 51)
2	Nausea or vomiting...	6 (1, 11, 20, 22, 27, 48)	13 (6, 7, 21, 23, 24, 30, 31, 34, 36, 38, 43, 44, 51)
3	Diarrhoea ...	25 (3, 4, 5, 9, 11, 12, 13, 14, 19, 20, 22, 25, 27, 28, 29, 32, 35, 39, 40, 41, 42, 46, 47, 48, 49)	16 (2, 7, 15, 18, 21, 23, 24, 26, 33, 36, 37, 38, 43, 44, 45, 51)
4	Constipation ...	—	3 (16, 34, 50)
5	Alternation of Nos. 3 and 4 ...	3 (3, 30, 27)	4 (6, 24, 36, 44)
6	Blood (in stools) ...	20 (3, 4, 5, 9, 10, 12, 13, 14, 19, 20, 27, 35, 39, 40, 41, 42, 46, 47, 48, 49)	15 (2, 6, 7, 16, 21, 24, 26, 30, 31, 33, 37, 36, 44, 50, 51)
7	Mucus (in stools) ...	13 (3, 4, 5, 9, 12, 13, 20, 25, 27, 28, 41, 47, 48)	10 (6, 18, 24, 26, 30, 31, 33, 45, 50, 51)
8	Purulent discharge...	—	1 (16)
9	Tenesmus ...	6 (3, 12, 22, 32, 48, 49)	3 (2, 18, 37)
10	Anorexia ...	1 (1)	—
11	Thirst ...	1 (1)	2 (2, 23)
12	Sweating ...	—	1 (2)
13	Malaise (headache, chill, &c.)	7 (1, 12, 14, 25, 29, 46, 47)	8 (2, 6, 18, 23, 24, 36, 50, 51)
14	Wasting ...	2 (20, 27)	5 (6, 23, 35, 37, 43)
15	Cough ...	3 (1, 8, 11)	—
16	Hæmoptysis ...	1 (8)	—
17	Swelling of feet ...	—	1 (15)

Note.—In no case is there any record of a similar affection in the same family or household.

TABLE VIII.—ANALYSIS OF THE SITUATION OF THE PAIN.

(From clinical observations for most part.)

(A) Abdominal Pain.

		MALES	FEMALES
	No. of cases	No. of cases	No. of cases
1	No definite locality ...	9 (8, 13, 28, 29, 35, 40, 42, 46, 48)	10 (2, 18, 23, 30, 31, 33, 34, 36, 44, 51)
2	Epigastric ...	1 (14)	1 (38)
3	Cæcum or right iliac fossa ...	4 (1, 3, 5, 41)	4 (16, 30, 37, 50)
4	Left iliac fossa ...	4 (20, 32, 41, 47)	5 (7, 15, 26, 38, 50)
5	Umbilical ...	4 (4, 5, 9, 10)	—
6	Described as "gripping" or "colicky" ...	7 (4, 11, 12, 14, 21, 22, 48)	3 (15, 24, 37)
7	Confined to lower half of abdomen ...	1 (39)	3 (21, 37, 44)
8	Right inguinal ...	—	1 (16)
9	Pain entirely absent...	1 (19)	1 (45)

(B) Pain in other Situations.

1	Lumbar ...	2 (12, 32)	2 (30, 44)
2	Left chest ...	—	1 (18)

TABLE VIII—continued.

(C) *Relation of Pain to Motions.*

1	Pain associated with passage of motions	4 (27, 47, 48, 49)	...	4 (6, 9, 17, 30, 37)
2	Abdominal pain relieved by motions	2 (28, 46)	...	1 (51)
3	Abdominal pain aggravated by motions	—	...	1 (16)

(D) *Abdominal Tenderness.*

1	General	2 (25, 24)	...	2 (21, 23)
2	Local	7 (5, 27, 41, 42, 46, 47, 49)	...	4 (15, 16, 31, 51)

TABLE IX.—FREQUENCY, &C., OF THE STOOLS.

(From clinical observations for most part.)

(A) *Average Number in Twenty-four Hours.*

				MALES		FEMALES
				No. of cases		No. of cases
From 1 to 5	18 (1, 5, 8, 10, 11, 12, 13, 14, 19, 22, 27, 28, 29, 32, 39, 42, 47, 49)	...	7 (7, 17, 26, 36, 43, 50, 51)
From 5 to 10	6 (9, 25, 35, 40, 46, 48)	...	5 (21, 23, 30, 37, 38)
Above 20	2 (4, 8)	...	—
"Marked"	—	...	2 (2, 31)

(B)

Bowels regular after admission	—	...	4 (6, 15, 18, 33)
Bowels confined after admission	2 (20, 41)	...	1 (16)
Alternation of diarrhoea and constipation	—	...	4 (24, 34, 44, 45)
Visible peristalsis	—	...	3 (21, 34, 43)
Sloughs	—	...	(No definite statement in any case.)

(C) *Frequency of Stools in Fatal Cases.*

Case				Case			
1	1 to 5 stools in twenty-four hours			2	Marked in twenty-four hours		
4	Above 20 stools in	"	"	7	1 to 5 stools in	"	"
8	1 to 5 stools in	"	"	34	Alternation of diarrhoea and constipation		
9	5 to 10	"	"	38	5 to 10 stools in twenty-four hours		
11	1 to 5	"	"	50	1 to 5	"	"
35	5 to 10	"	"	51	1 to 5	"	"
46	5 to 10	"	"				

TABLE X.—PYREXIA.

(A) *Temperature range.*

				Males		Females
1	Subnormal to 99°	16 (3, 5, 9, 12, 13, 19, 20, 22, 27, 28, 32, 40, 41, 42, 47, 48)	...	7 (15, 16, 18, 33, 43, 44, 45)
2	Up to 100°	6 (10, 14, 25, 29, 39, 49)	...	3 (17, 24, 31)
3	Up to 101°	2 (4, 35)	...	6 (6, 26, 30, 34, 37, 50)
4	Up to 102°	—	...	2 (23, 51)
5	Up to 103° and over	4 (1, 8, 11, 46)	...	5 (2, 7, 21, 36, 38)
	Rigors	—	...	—

TABLE X—(continued).

(B) Range of Temperature in Fatal Cases.

Case		Case		Case	
1	99°—103°	35	99°—101°	34	98°—101°
4	99°—101°	46	97°—103°	38	Normal to 104.4°
8	100°—103°	2	96°—103.8°	50	98.4°—101°
9	95°—98°	7	Normal to 103°	51	99°—101.8°
11	97.2°—104°				

TABLE XI.

(A) Blood Examination.

	No. of cases	MALES	No. of cases	FEMALES
1 No mention ...	25	(1, 3, 4, 5, 8, 9, 10, 11, 12, 13, 14, 19, 20, 22, 25, 27, 28, 29, 32, 39, 40, 41, 42, 46, 47)	20	(2, 6, 7, 15, 16, 17, 18, 21, 24, 26, 30, 33, 34, 36, 37, 38, 43, 44, 45, 50)
2 Widal negative ...	1	(35)	3	(23, 31, 51)
3 Reaction against Shiga's bacillus negative ...	2	(48, 49)	—	—

(B) Rectal and Vaginal Examination.

1 No mention ...	21	(1, 3, 5, 8, 9, 10, 11, 12, 13, 14, 19, 22, 25, 27, 32, 39, 41, 42, 47, 48, 49)	14	(2, 6, 7, 15, 17, 18, 21, 23, 26, 31, 34, 36, 45, 51)
2 Nil detected, or normal ...	6	(4, 20, 29, 35, 40, 46)	5	(16, 33, 38, 43, 44)
3 Definite signs ...	1	(28) ¹	3	(30, ² 37, ³ 50) ⁴
4 Pelvic peritonitis and fibroids ...	—	—	1	(24)

¹ Male. "Nil definite, except some swelling of mucous membrane."

² Female. Per speculum. "Surface, except for 1 in. above anus, raw and covered with granulations; constant bleeding; occasionally small patches of toughish white membrane are seen; these appear flaky on separation."

³ Female. Under chloroform. "The rectal mucous membrane looked raw; no definite ulceration."

⁴ Female. "So far as finger can reach, firm polypoid masses; much induration; no bleeding; ulceration just inside the anal margin; ulcer irregular, sharp edged, surrounded by hypertrophied mucous membrane; complete loss of control of rectal sphincter; skin around posterior edge of anus is ulcerated." This patient had an operation for relief of fissure in another hospital.

TABLE XII.—COMPLICATIONS AND ASSOCIATED CONDITIONS.

	Males	Females
1 Marked emaciation ...	4 (1, 11, 35, 40)	3 (34, 50, 51)
2 Thrombosis (left leg) ...	—	1 (37)
3 Pneumonia ...	1 (1F)	1 (2F)
4 Haemoptysis ...	1 (8F)	—
5 Empyema ...	1 (8F)	—
6 Pneumothorax ...	1 (8F)	—
7 Stomatitis ...	1 (4F)	2 (6, 43)
8 Alveolar abscess ...	—	1 (21)
9 Peritonitis, general ...	3 (1F, 11F, 46F)	3 (7F, 34F, 51F)
10 Local peritonitis (?) with local perforation ...	—	1 (37)
11 Perforation ...	3 (1F, 11F, 46F)	1 (51F)
12 Chronic renal ...	1 (25)	—
13 Pelvic peritonitis and fibroids ...	—	1 (24)
14 Hepatic abscess ...	(1 8F)	—

"F" refers to fatal cases.

TABLE XIII.—POST-MORTEM (MALES).

	Case No. 1	Case No. 4	Case No. 8	Case No. 9	Case No. 11	Case No. 35	Case No. 46
(a) <i>Small intestine</i>							
Ulceration ...	Superficial ulceration down to serous coat; Peyer's patches free	—	Numerous small ulcers on last 2 in. of ileum; coating of drab-like lymph	A few ulcers in lower part; sharp, punched out, extending to variable depths; one with deeply pigmented base	Four small ulcers in last 1½ in.	No ulcers	—
Other changes...	Hæmorrhagic extravasations; much injected; dark slate colour in parts	Ileum only vascular	Beyond slight congestion nothing abnormal down to last 2 in. of ileum	—	—	Mucous membrane hyperæmic	—
(b) <i>Large intestine</i>							
Cæcum ...	No notable change	No notable change	Small, thickened, ulcerated; ulcers have clean-cut edges; covering of drab-like lymph	—	Extensive ulceration; almost all mucous membrane destroyed; large black pulpy sloughs	Hyperæmic throughout; mucous membrane left in polypoid masses; much pigmentation; muscular coat laid bare	Small perforations
Ascending colon	Large sloughing area in lower part, with perforation; dense adhesions	Thickened; mucous membrane corrugated; ulceration	Ulcerated	Principal ulceration; rounded, punched out, mostly distinct	—	—	Ulceration of whole colon; very little mucous membrane left
Transverse colon	No notable change	Thickened; mucous membrane corrugated; ulceration.	Ulcerated	No thickening of coat	—	Hyperæmic throughout; mucous membrane left in polypoid masses; much pigmentation; muscular coat laid bare, very hyperæmic	—
Descending colon	No notable change	Ulcerated; fewer, but larger transverse colon especially vascular	Ulcerated; ulcers	—	Ulcers serpigineous in lower part and upper rectum, but have definite transverse arrangement elsewhere; ulcers external down to anus	—	—

Sigmoid	...	No notable change	Thickened; mucous membrane corrugated; ulceration, transverse colon especially vascular?	Ulceration extends into rectum, and more uniform over the last 2 in. or 3 in. inside the anus; the ulcers are of a dirty green colour; floor rather granular	—	Ulcers serpiginous in lower part and upper rectum, but have definite transverse arrangement elsewhere; ulcers extend down to anus	Hyperemic membrane left throughout; mucous membrane left in polypoid masses; much pigmentation; muscular coat laid bare, very little mucous membrane left	Ulceration of whole colon; very little mucous membrane left
Rectum	...	No notable change			Numerous ulcers	Extensive; much matting; much recent lymph and pus, large collection of pus and faeces in pelvis, pieces of mucous membrane loose in cavity	Large tracks of more healthy mucous membrane, but ulcers extend to anus	Lower part of abdomen
(c) Peritonitis	...	Plastic; much matting of intestine	No	No	No	Five in large intestine	No	No
Perforation (gut)	In ascending colon	Marked	No	No	No	Marked	Marked	Yes
Emaciation	...	Left lung, patchy consolidation; left kidney, cystic	Stomach and duodenum show vascular patches; abscess in left tonsil; lungs oedematous	Liver: large abscess cavity (does not communicate with empyema), other necrotic areas; right pleura; many adhesions, large empyema (this communicates with large shallow area on upper surface of liver), a second smaller area on the upper surface of left lobe; right lung: large abscess	Liver, slight peritonitis over right lobe	Left lung, septic broncho-pneumonia; mesentery thickened, mesenteric glands +	Omentum adherent to colon; vessels of transverse mesocolon injected	No
(d) Other organs	...							Left lung, partial consolidation

TABLE XIV.—POST-MORTEM (FEMALES).

	Case No. 2	Case No. 7	Case No. 34	Case No. 38	Case No. 50	Case No. 51
(a) <i>Small intestine</i>	Natural ileum, which is bound down to pelvis	Normal internally	Recent adhesions to abdominal wall, otherwise natural	Stomach, jejunum, and upper two-thirds of ileum natural, lower third polypoid, pigmented masses; few cysts		Normal
Ulceration ...	No	No	No	Extensive ulceration of lower third of ileum extending to muscular coat	Normal; ulceration commences abruptly at ileo-caecal valve	Normal
Other changes ...	No	No	No	Extensive enteritis; intestines distended and hyperemic		Normal
(b) <i>Large intestine</i>	Natural down to lower end of descending colon	Everywhere inflamed, soft, easily torn; mucous membrane congested, swollen, ulcerated	Much thickened, especially muscular coat; mucous membrane thickened	—	Gut wall thickened, no narrowing; no peritoneal lesions	Very hyperemic from caecum to rectum
Appendix ...	Bound down to pelvis	—	—	—	—	No ulceration
Caecum ...	Bound down to pelvis	Mucous membrane almost entirely gone, only a few purple patches left	Especially involved, much dilated	Much distended, very thin, a few small sharply defined ulcers	Ulcerated, though not extensively	Not dilated; upper anterior part gangrenous; numerous large ulcers, closely set, extending into muscular coat, edges slightly undermined; mucous membrane between hyperemic, swollen; many ulcers have membranous deposit

Ascending colon	Natural	Ulcers less marked	Scattered throughout large intestine are ulcers, large, irregular, mostly transverse, penetrate to various depths; bases clean, but some have yellowish slough; edges sharply cut, irregular, undermined	Shows chronic ulceration down to muscular coat	—
Transverse colon	Natural	Numerous ulcers and hemorrhages	—	Whole of mucous membrane gone, except for small islands forming polypi	—
Descending colon	Natural down to lower end	Less involved	—	Throughout rest of large intestine lymphoid follicles enlarged, in many places ulcerated; ulcers sharply cut	Recent lymph; several ulcers on point of rupture
Sigmoid	...	Upper part covered with large deeply seated ulcers, edges undermined; below this gut healthy but narrowed by fibrous tissue overgrowth	—	Shows chronic ulceration down to muscular coat	—
Rectum	...	Only congested	—	—	—
(c) Peritonitis	...	Recent lymph; free gas	General	No	Yes (early); soft recent adhesions
Perforation (gut)	No	None detected	No	No	Yes
Emaciation	No	No	Yes	Yes	Poorly nourished
(d) Other organs...	Lungs: hypostatic congestion; omentum adherent to pelvis; abscess in psoas muscle (? origin)	Omentum adherent, no other conspicuous change	Mesenteric glands larger and redder than normal	No definite change	Stomach, superficial erosion near pyloric end; liver, fatty; uterus, fibroids; mesenteric and tracheal glands, calcareous

TABLE XV.

(A) *Exploratory laparotomy performed in—*

No. 34 Female: fatal.

No. 51 Female: fatal.

(B) *Test meal given in No. 43 F.**Result—*

No free HCl.
 Free lactic acid.
 Total acidity: 0.146 gr. HCl per cent.
 Free HCl: 0.0 per cent.
 Proteid HCl: 0.0657 gr. HCl per cent.
 Organic acid: 0.09.

Microscopical— Lactic acid bacilli: *Torulae*.

TABLE XVI.

(A) *Bacteriology or other Examination of Stools.*

	MALES		FEMALES	
1 No examination made	...	26	...	19
2 Examination made...	...	2 (35, ¹ 42 ²)	...	4 (17, ³ 24, ⁴ 30, ⁵ 37 ⁶)

¹ *Feces*.—First examination: liquid, acid reaction; large amount of blood and bile (no urobilin); microscopically: much mucus, many blood-cells, bacteria and cells, and much casein. Second examination: liquid alkaline, urobilin +; microscopically: blood, pus ++, columnar cells, no growth, no amoeba coli.

² "No amoeba coli."

³ "No amoeba coli."

⁴ "Epithelial cells, debris, many motile bacteria."

⁵ Microscopically: "pus with filamentous material."

⁶ Macroscopically: acid reaction, much bile pigment, no blood, much mucus; microscopically: pus, fatty extracts, no sloughs, no tubercle bacilli, numerous streptococcal chains.

(B) *Bacteriological Examination from Peritoneal Exudate taken during Operation.*Only record in one case, F 51 (*Bacillus pyocyaneus*).

TABLE XVII.

DIAGNOSIS.

	Males		Females	
1 Colitis and enteritis, ulceration of colon, colitis, ulcerative colitis, ulcerative colitis and enteritis, chronic colitis, ulceration of the sigmoid, chronic ileocolitis	...	15 (1, 13, 19, 20, 22, 27, 32, 35, 39, 40, 46, 28, 29, 47, 42)	...	18 (2, 37, 18, 23, 24, 31, 16, 17, 26, 36, 50, 51, 30, 33, 44, 45, 38, 43)
2 Dysenteric ulceration, dysenteric diarrhoea, chronic dysentery, dysentery, subacute dysentery	...	13 (11, 10, 5, 12, 14, 41, 49, 4, 3, 9, 25, 48, 8)	...	5 (6, 15, 7, 21, 34)

Westminster Hospital.

By A. M. GOSSAGE, M.D., and F. W. PRICE, M.D.

DURING the past twenty-five years—1884 to 1908—there have been 42 cases of ulcerative colitis in the Westminster Hospital. Of these 17 have been males and 25 females, and of the 42 cases 19 died—8 males, and 11 females.

Age	Number of cases	Deaths
Under 5	2	2
5—10	1	1
10—20	4	2
20—30	9	4
30—40	14	4
40—50	6	4
50—60	4	2
Over 60	2	0

DURATION.

Fatal Cases.

Period	Cases
Less than one month	6
Less than two months	3
Less than three months	2
Three to six months	1
Twelve months	1
Two to three years	2

Non-fatal Cases.

One to two months	5
Three to four months	2
Six to seven months	5
Ten to twelve months	2
Twelve to eighteen months	4
Two years	3
Ten years	2

Incidence in successive years from 1884 to 1908. The numbers admitted were : 0, 1, 0, 0, 1, 0, 1, 4, 0, 1, 3, 1, 2, 4, 2, 2, 2, 1, 3, 6, 4, 2, 2, 0 respectively.

The occupation of the patients had been of the most varied description, and seemed to have had no influence in the causation of the disease. Two men had previously resided in the tropics as soldiers, but had not either of them suffered from dysentery, and the onset of their complaint dated from a period some time after their return to England. One man had spent some years in Australia with the enjoyment of perfect health, his disease starting shortly after landing in England. In 2 cases—a man and a woman—the symptoms followed almost immediately after an over-indulgence in fruit.

There was a previous history of intestinal trouble in 13 cases; of these 1 had had diarrhoea twelve months previously; 1 had had recurrent diarrhoea with blood and mucus for ten years; 1 had had diarrhoea off and on for several years, and had been operated on for piles five years before; 1 had had pain in the left side of the abdomen and attacks of diarrhoea and vomiting for six months before blood and slime were noticed; 1 had had diarrhoea and vomiting four years before onset, and again twelve months before; 1 had had diarrhoea off and on for nine years; 1 passed a tapeworm two years previously, and at that time had diarrhoea with blood and slime; 1 had had typhoid two years before, and was admitted for the treatment of an ischiorectal abscess and a rectovaginal fistula; 1 had had pain after food and vomiting for three months before the onset of the diarrhoea; in 1 man the symptoms immediately followed an accidental squeezing of the abdomen; 3 cases were preceded by chronic constipation.

Individual cases were first affected after measles, influenza, childbirth, and the vomiting of pregnancy respectively. Three males had previously been heavy drinkers. No similar affection had been noticed in any other member of the family of any case. The cases tended to occur in groups—*i.e.*, 2 or 3 patients with the affection would be in the hospital about the same time, and then there would be a long interval without any cases. No connexion could be traced, however, between any 2 patients.

SYMPTOMS.

The main symptoms on which the diagnosis has been based have been diarrhoea, with the passage of blood and mucus with the stools. In 1 case, however, a woman who had died from pulmonary tuberculosis and hæmoptysis, extensive ulceration of the colon was found which was not due to tubercle, and yet during life there were no symptoms pointing to anything wrong with the intestines. Blood was absent from the motions of 2 other patients whose large intestines were found after death to be the seat of severe ulceration, and in another similar case mucus is definitely stated to have been absent. In the cases that died the diagnosis was confirmed by post-mortem examination, but in the other cases there must be some doubt as to its correctness. In 1 of these a rectal examination showed ulceration of the rectum, but in the others the diagnosis depended on the long continuance of the diarrhoea and the character of the stools. In some patients a thickened colon could be felt through the abdominal walls, either with or without an anæsthetic. In 1 such case abdominal section was performed, as it was thought that the swelling felt in the left iliac fossa might be a malignant growth. In some other cases a thickening of the colon was thought to be present, but the signs were indefinite; 1 of these turned out to be a case of ankylostomiasis. It is important to notice that thickening of the colon was not found in all cases that died, the walls in some being quite thin. The presence of blood and slime cannot be regarded as necessarily implying ulceration, since they are frequently

present in the acute enterocolitis of children. Where, however, the condition lasts many months the existence of ulceration is more than probable. Three cases, female aged 30, female aged 57, and male aged 34, had diarrhœa with blood and slime in the motions, and all apparently recovered in from six to eight weeks. These 3 cases may be regarded as specially doubtful. Besides these 3 cases there was complete recovery in 11 others; that is to say, that the motions were normal on discharge. The possibility of recovery is proved by the discovery of healed ulcers in post-mortem examination. Cicatrization was found in some of the ulcers in the ascending colon from a man aged 26, who died after only two weeks' illness; there was also here extensive ulceration of the transverse and descending colon with no sign of repair. Healed ulceration was also found in a woman, aged 55, in the transverse and descending colon; there was no clinical history pointing to previous intestinal disease in this patient, and she is not included in the collected cases.

As a rule the first symptom noticed was diarrhœa; this was so in 25 patients. In 3 instances pain in the abdomen was the first thing noticed. In 2 cases a discharge of blood *per rectum* occurred for some days before the onset of the diarrhœa. In one of these the hæmorrhage followed shortly after an accidental squeezing of the abdomen; in the other the bleeding occurred after a hard day's work. In 2 cases fever was the first deviation from health, and in 1 patient there was a rigor at the onset.

In nearly every patient diarrhœa set in early and persisted, but in 1 instance there was constipation throughout, the motions being covered with slime. In 2 cases there was alternation of diarrhœa and constipation, in 1 of which on admission there was a discharge *per anum* of blood and slime, while large hard masses of fæces were found in the ulcerated rectum. Abdominal pain is recorded in 21 instances, in 5 of which it was chiefly on the left side. In 4 case records it is specially stated that there was no pain. Pain on defæcation was found five times and tenesmus three times. There was general abdominal tenderness in 6 cases; tenderness over the descending colon in 6 cases, and in the course of the ascending, transverse and descending colon in 2 cases. Tenderness was said to be absent in 2 patients. There was abdominal distension in 4 cases, in 2 extreme, but one of these was associated with an incarcerated hernia.

The stools were as a rule loose, fetid and often profuse. In 3 records they were described as pea-soupy, these cases all running an acute course to a fatal termination. Their colour was sometimes dark, sometimes pale, and on two occasions green. Blood was present in the stools thirty-two times, absent six times. Mucus was present twenty-nine times, absent once. Pus was found in the stools of 6 patients, and sloughs also in the stools of 6. Vomiting, which was present in 13 cases, was never an urgent symptom and was generally described as occasional. Pyrexia occurred in 17 patients, of whom all but 5 died. The fever was sometimes severe, sometimes moderate; sometimes hectic in type and sometimes continued and like that of typhoid. In 4 cases nothing abnormal was found on rectal examination; in 2, examined under anæsthetics,

ulceration was found; of these 1 died and the other was discharged unrelieved.

No clinical history could be found of 3 of the patients who died. One of these, a child, aged 2½, was brought in dead. The notes of the other 2 had apparently been lost.

The condition was only rarely complicated by disease in other parts. In 4 instances tubercle of the lungs was found, of which 2 died, 1 having also tubercular peritonitis. In 2 of these cases a search was made for tubercle bacilli in the fæces without success. In 3 cases pneumonia occurred as a terminal phenomenon. There were 3 examples of purpura, all of whom died. Albuminuria was recorded in 4 cases with 2 deaths. In 1 fatal case the sigmoid was incarcerated in an inguinal hernia. One woman had phlebitis during convalescence. Parotitis occurred twice with one death.

Perforation was recorded four times, but as in one of these there was no peritonitis the perforation must have taken place after death. In one of the others it was considered that the perforation was probably post mortem. Besides these 3 cases, 3 patients had peritonitis without perforation, all of whom died. In 1 of these the peritonitis was tubercular; in the other 2 it was slight and local and probably had nothing to do with the fatal issue. There was no instance of hepatic abscess.

Duration.—The cases may be divided into two classes—those lasting under two months and those that lasted longer. In the former class there were 15, of whom 10 died. Of the 5 who did not die 1 was found to have ulceration of the rectum under an anæsthetic and was discharged unrelieved; another was found to have thickening of the descending colon and sigmoid under an anæsthetic and there was slime, pus and blood in the stools. This patient improved, but there was still some diarrhoea on discharge. The other 3 cases recovered and have been already discussed. In 24 cases the duration was over three months, of whom 5 died. In 11 patients the motions became normal before discharge, 2 of whom had had previous similar attacks from which they had recovered. Most of the patients were lost sight of after leaving the hospital, and some of them may have drifted into other hospitals. Two cases had been treated elsewhere for similar attacks from which they had recovered. One, who had never been abroad, had recovered from an attack of "dysentery" in another hospital five years before; he was admitted with a history of diarrhoea of a few weeks' duration and died in five days; his colon was inflamed and ulcerated from end to end, very little of the mucous membrane being left. In 5 cases there was still diarrhoea on discharge; 1 of these was under observation at intervals during the next eighteen months, as she had pulmonary tuberculosis; the stools never were quite normal, there being three stools daily, but there was no return of the slime and blood.

Death occurred sixteen times from exhaustion, twice from perforation and peritonitis. One patient died from pulmonary tuberculosis and hæmoptysis.

POST-MORTEM FINDINGS.

At the post-mortem examination the ulceration of the gut was found to be more severe as a rule at the lower part. In 14 instances the ulceration extended throughout the colon, twice it was only below the splenic flexure, while in 1 case it was confined to the sigmoid, rectum and cæcum. In 2 bodies the ascending and transverse part were ulcerated, but the descending colon and rectum were free. In 2 acute cases there was also ulceration in the lower part of the ileum. The ulceration varied from shallow abrasions to deep ulcers exposing the muscle-coat and in 1 case the peritoneum. Ante-mortem perforation was found in 2 cases and possibly in a third. The ulcers were sometimes discrete, sometimes confluent in large patches with just tags of mucous membrane left. These tags were thickened and polypoidal. The edges of the ulcers were sometimes sharp, but were usually undermined when the ulceration was at all deep. In 1 case there was a good deal of pus in the submucosa. It is interesting to note that in 1 case, a male aged twenty-six, who had only been ill two weeks, cicatrization was taking place in some of the ulcers in the ascending colon. The walls of the gut were sometimes thickened, but in other cases they were quite thin. Thickening of the walls was found in some cases where the onset was quite recent—*e.g.*, in a case of only two weeks' duration, while the walls were thin in a case where the symptoms dated back for twelve months. The gut was often rotten and broke easily, and in at least 1 case perforation had taken place after death. The mesenteric glands and spleen were occasionally hyperæmic and slightly enlarged, but in other instances they are specially noted as being normal. Peritonitis apart from perforation was found three times, but beyond this no change was found in other parts which could be regarded as having any but an accidental association with the condition of the colon. In a man aged 29, whose illness lasted six weeks, there were some rose spots and ulceration of the larynx.

BACTERIOLOGY.

The fæces from 2 patients were examined for tubercle bacilli with a negative result. Plate cultures were made from 2 cases, and in 1 *Bacillus coli* and numerous other bacilli were obtained, whilst the other gave *Bacillus coli*, another large bacillus, staphylococcus and streptococcus, and a yeast. Amœbæ were noted as being absent from the stools in 1 case. The blood of 1 patient was examined to see whether it agglutinated *Shiga's bacillus*; the result was negative. The opsonic index for tubercle was taken from a patient with tubercular peritonitis and was found to be 0.91. In 1 case numerous bacilli staining with methylene blue were found in the bowel-wall. A blood-count was made in 1 case. Red corpuscles, 5,020,000; white, 17,000; polymorphonuclear, coarse 2, fine 43; mononuclear, small 45, large 10.

TREATMENT.

A number of drugs were administered by the mouth, such as bismuth, opium, castor oil, lead acetate, &c. Judging by results the most successful treatment was by the use of enemata; these enemata contained boracic acid, ferric perchloride, argyrol 1 per cent., or starch and opium. Colotomy was performed on 2 patients, both of whom died a few days afterwards. A laparotomy was performed on 1 case, but as tubercular peritonitis was found nothing further was done.

Medical Section.

February 23, 1909.

Dr. T. HENRY GREEN, Vice-President of the Section, in the Chair.

Chloroma.

By F. DE HAVILLAND HALL, M.D.

WITH PATHOLOGICAL REPORT.

By R. G. HEBB, M.D., and J. M. BERNSTEIN, M.B.

I AM venturing to read the following notes, as I believe it is the first time that a case of chloroma has been brought before any of the Medical Societies in London, and it is therefore high time for a discussion on the subject. Moreover, in this case a diagnosis of the nature of the disease was made during life, whereas in most of the cases hitherto reported the diagnosis was made only after death from the green colour of the tumours. Dr. Melville Dunlop's case was the first one in which a correct diagnosis was made during life. The patient, a boy aged 5, was shown at a meeting of the Edinburgh Medico-Chirurgical Society in November, 1901. Considering that the first case was recorded by Alan Burns in 1823, it seems somewhat extraordinary that a disease with so characteristic a syndrome should not have been recognized during the life of the patient earlier. I will now read the notes of my case.

M. B., aged 4 years and 3 months; admitted November 8, 1907; died December 24, 1907. Diagnosis, chloroma.

Family history: Father and mother alive and well. Patient eldest of three children. No miscarriages. Previous illnesses: measles when a baby; no other serious illness.

History of present illness: A month ago the mother noticed that the child was very irritable and that it complained of pain in its forehead, followed by pain on top of head. This pain seemed to increase

and kept the child awake at night time. A fortnight previous to admission the mother noticed that on each side of the neck a hard lump was forming, and at the same time the child's eyes were becoming prominent, and the veins over the temporal region on each side were distended, but more marked on right side. The mother has noticed that the child has had some difficulty in articulation and has "slurred" her words. Previous to this articulation had been normal. Child has shown no difficulty in mastication or swallowing. There has been no dizziness or difficulty in walking. Child has been off her food, and bowels have been constipated.

Present condition: On admission there is prominence of eyes, more marked on right side; there is yellowish discoloration of upper eyelids; when asleep eyes are partially open. On the left side there is a hard immovable swelling reaching from the mastoid downwards over the sternomastoid muscle and forwards towards the ramus of jaw; the swelling does not quite reach as low as angle of jaw. On the right side there is a similar swelling, smaller in size, in position just below the zygoma and reaching back as far as the ear. These swellings are not tender when touched and do not seem to give rise to any pain. Neither swelling interferes with the movements of the lower jaw. Over both temporal regions there is enlargement of the superficial veins, more marked on the right side. When crying distinct facial paralysis of the right side is noticed. No glands in other parts of body enlarged. A small hard lump can be felt just above the umbilicus; spleen not enlarged. Heart *nil*. Lungs *nil*. Knee-jerks and ankle-jerks present. Temperature 98 on admission. Has vomited once. Urine acid, 1018; no albumin or sugar.

November 12: Eyes examined by Mr. Hartridge, "double optic neuritis." The two swellings are distinctly larger; veins more marked. Child is drowsy. November 13: Child is a little brighter to-day and takes some interest in her surroundings; when asked if she had any pain in her head she replied in the negative. The engorgement of veins has now spread on right side as far as centre of forehead and nose and down over the right cheek. November 15: Patient remains about the same. November 16: Head circumference, tape resting $\frac{1}{2}$ in. above occiput, 20 in. Oedematous condition of tissue from tumour on right side of face to external canthus of eye. Veins on right cheek are enlarged and similar to those described over the temporal region. November 18: There is oedematous condition of tissues from tumour to the outer canthus of eye on the left side now; both upper eyelids

very œdematous and discoloured; child irritable and drowsy. November 20: Left ear gives a disagreeable smell, but no discharge is noticeable from it; the tumours are slightly larger and forehead is very œdematous; right side of face is now completely paralysed; loss in weight of 8 oz. during the week; pulse 140. Child was lumbar punctured this afternoon. At first fluid came out slowly, but very soon came out under very great pressure. Report from clinical laboratory of examination of cerebrospinal fluid shows marked leucocytosis 120 to $\frac{1}{8}$ field, mostly monomorphes with characters similar to those of the blood. November 22: Patient vomited once this morning (3 a.m.); quantity very small; left ear syringed, but no discharge noticeable. November 24: Swelling on left side of face much less now; right side is larger, and some small hard glands can be felt in posterior triangle of neck; œdema of face on right side great, on left side practically absent. November 27: Patient has gained 6 oz. in weight this week and is eating better; tumour on left side much smaller; that on right side is gradually getting larger. Proptosis of right eyeball is more marked.

December 2: Child is a little brighter to-day; both swellings are smaller; œdema of face is much less and veins are not so enlarged. December 4: Gain in weight of 6 oz. this last week; patient seems better, but is getting much paler; both swellings are gradually becoming smaller, and œdema over forehead is very slight now. December 11: Patient has gained 8 oz. in weight this week; patient is becoming paler every day; practically no sign of tumour on left side now; right side seems about the same size and has some slight œdema over it; veins over this area still enlarged; patient complains of no pain; pulse 152; glands in both inguinal regions are now palpable; spleen is not palpable. December 13: Pulse 160. December 17: Increase in weight of 14 oz. during the last week; child seems much better in herself; tumour on right side is slightly less and veins are not so enlarged; a hard lump can now be felt at angle of right jaw, which seems to be rather tender; a prolonged attack of epistaxis. December 19: Temperature, which has been normal, suddenly rose to 102.6° F. December 20: Temperature has now dropped to normal again; child has been very sleepless and rather irritable; pulse 168. December 23: Temperature has been getting gradually higher the last two days; child more drowsy and off her food; will only take milk now. December 24: Breathing very rapid, 66; pulse 180; child semi-conscious; death.

The first points to be noticed are the age and sex of the patient.

Of 47 cases of chloroma mentioned by Mr. Treadgold,¹ 33 were males and 14 were females. The earlier figures of Dr. Dock² showed a much greater preponderance of males. The average age of the 47 cases was 17.6 years. The disease is, therefore, much more common among males than females. It usually appears before puberty, and is comparatively rare after early adult life, though a case has been recorded in a man aged 52.



FIG. 1.
Case of Chloroma.

On turning to the clinical history the sudden onset of the illness in a previously healthy child is to be noted, the first symptom of which complaint was made being pain in the head. A fortnight later the mother noticed the lump in the neck and the protuberant eyes. In almost all the cases of chloroma some part of the head is affected, and

¹ *Quart. Journ. Med.*, Oxford, 1908, i, p. 260.

² *Trans. Assoc. Amer. Phys.*, Philad., 1904, xix, p. 75.

in my case the tumours apparently started from the temporal region. The accompanying photograph will assist the description. The prominent eyes and the puffiness of the eyelids at once attract attention. The face was broadened owing to the masses in the temporal region, and the complexion was very pale and had a waxy, yellow tint. The swellings over both sides of the neck gave a superficial resemblance to mumps—indeed, the suggestion that the condition was



FIG. 2.
Case of Chloroma.

due to a chronic inflammation of the parotid was put forward by one of my colleagues, but the distribution of the swelling did not correspond with the area usually occupied by an enlarged parotid. Even during life the tumours had a suspiciously green colour. The enlargement of the veins was so pronounced as to suggest pressure on a main trunk, and the facial paralysis on the right side was evidently due to pressure on the facial nerve.

From the time of admission on November 8 to December 24, when death occurred, the course was almost steadily downwards. This is well shown by the blood-counts made on November 9, 16, 30, and December 7 and 23. From the table it will be seen that there was a steady fall in the number of red corpuscles and in hæmoglobin, and to a less extent in the leucocytes.

The patient was at first bright and seemed interested in her surroundings, but towards the end she became drowsy and was irritable when awake or roused. While in hospital the child apparently did not suffer much pain, and there was no difficulty in mastication or swallowing. A curious feature in the case was the slight but steady gain in weight during the four weeks preceding the week in which death occurred; the total gain was 2 lb. 2 oz., and this in spite of the fact that the patient was visibly becoming more anæmic and going downhill. There was no dropsy, which sometimes gives rise to a deceptive appearance of increase in weight.

As is usual in these cases there is nothing characteristic about the temperature chart. During the first ten days there was an evening rise to about 99.2° F. or 99.4° F.; then the temperature became more irregular and higher for four days, 100° F. to 101° F.; it gradually came down in a few days; after that it was irregular but not rising above 99° F. to 100° F. For a few days there was no rise of temperature. On December 17 the evening temperature rose to 100° F., on the 19th to 102.4° F.; from this date until death there was an evening rise of 100° F. to 102° F. Death was due to asthenia, preceded by a great increase in the frequency of the respiration and circulation. If the mother's account of the onset of the illness be accepted death occurred within three months from the commencement of symptoms; thus it may be regarded as rather an acute case. In Dock's series¹ of 22 cases the average duration was 5.5 months; in 10 it was four months or less, two each being stated as one and two months; in one case it was one year, another thirteen months, and in a third one and a half years.

The medicinal treatment of the patient was confined to the administration of arsenic, at first alone in 3 m doses of the liquor arsenicalis, given three times a day, but afterwards the dose of arsenical solution was increased to 5 m, and given in combination with the ammonio-citrate of iron. As was to be expected the treatment had no effect

¹ *Trans. Assoc. Amer. Phys.*, Philad., 1904, xix, p. 75.

in arresting the progress of the disease. Indeed, it must necessarily be only symptomatic, as we are ignorant of its cause. In the case under consideration, as soon as the question of chloroma was raised, and this was done before the child was admitted into the ward, the diagnosis was comparatively easy. In the first place the aspect of the child was very suggestive; the masses in the temporal region, which, even during life, had a greenish tint, the protrusion of the eyeballs and the anæmia presented a complex of symptoms unlike that met with in any other disease. The examination of the blood also helped to confirm the diagnosis, as there was a leucocytosis affecting the large lymphocytes. But I can quite understand that if the case had been seen at an earlier period there would have been great difficulties in the diagnosis. Supposing the exophthalmos had been the first symptom, the possibility of a cerebral tumour causing the protopsis would have had to be considered; the pain in the head would have also assisted in fixing attention on some intracranial mischief, and the optic neuritis would probably have seemed to clinch the diagnosis. The tender age of the patient would have rendered the diagnosis of Graves's disease very unlikely, though cases have been recorded as early as 3 years and under. In patients who come under observation without swellings in the orbits and temporal regions, it is easy to understand that especial difficulties in diagnosis may arise. Indeed, I do not see how in some of the reported cases it would have been possible to make a definite diagnosis during life.

As I have already mentioned, a chronic inflammatory condition of the parotid was suggested as the cause of the trouble, but the distribution of the swellings, the exophthalmos, and the blood-count put this out of court. In the absence of any tumours the diagnosis of chloroma from leukæmia would be practically impossible, as deafness, optic neuritis, exophthalmos are met with in both complaints, and the characteristic symptoms of leukæmia—*i.e.*, progressive anæmia, epistaxis, subcuticular, subconjunctival and retinal hæmorrhages—are seen in chloroma. The blood in chloroma presents the features met with in lymphatic leukemia. As Dr. Byrom Bramwell¹ points out, there are cases which seem "to show that no hard-and-fast line could be drawn between acute lymphatic leukæmia and chloroma."

In almost all the cases I have come across in the literature of the subject the first local manifestations of the disease have been seen in

¹ *Lancet*, 1902, ii, p. 512.

the head, usually in the temporal region, but in a case recorded by Dr. T. Harris and Dr. F. C. Moore,¹ the patient, a boy aged 14, complained of a pain in the left hip, and he was thought to be suffering from sciatica. It was not until nine weeks later that he showed signs of orbital and intercranial growths, and he died a month later. At the necropsy new growths of a grass-green colour were found in connexion with the inner aspect of the dura mater, with the periosteum of the orbits, of the vertebræ, the sacrum, the iliac bone, and in the kidneys.

Dr. Bramwell² groups the characteristic features of chloroma as follows: (1) The presence of lymphoid deposits in the orbits, temporal fossæ and periosteum of the bones of the skull, and the symptoms and signs resulting therefrom; (2) the green colour of the lesions, this being the remarkable feature of the disease; (3) a profound blood-change, characterized by anæmia, cachexia, hæmorrhages into the skin, bleedings from the mucous membranes, especially epistaxis, and petechial extravasations into the retinæ and internal surfaces; (4) infiltration of the bone-marrow, the spleen, the lymphatic glands and the organs throughout the body with lymphoid deposits, as in lymphatic leukæmia.

Dock³ sums up very concisely the whole question of chloroma as follows: "Out of the protean symptoms of chloroma cases we can separate three sets of conditions: the mechanical results of the green growths, causing many symptoms, both objective and subjective (exophthalmos, visible tumours, pain, deafness, &c.); the toxic symptoms, as weakness, fever, emaciation; and the blood symptoms, such as pallor, hæmorrhages, and alterations of the blood itself."

A clue to the recognition of the disease is to be found in an examination of the blood. In my case the changes produced by the progress of the disease are well shown on the accompanying chart. From this it will be seen that the red corpuscles fell from upwards of 4,000,000 per cubic centimetre to a little over 1,000,000. But the most notable change is to be noted in the leucocytes. On admission there was a very considerable leucocytosis, upwards of 40,000 leucocytes per cubic centimetre. At the examination made on December 23 they had, however, fallen to 23,900. More remarkable than the mere increase of leucocytes is the change in their relative proportion. The bulk of the leucocytes is made up of large lymphocytes, the small lymphocytes being about their normal number,

¹ *Lancet*, 1902, i, p. 525.

² *Lancet*, 1902, i, p. 522.

³ *Trans. Assoc. Amer. Phys.*, Philad., 1904, xix, p. 82.

whereas the polymorphonuclears were at first less than a quarter of the normal proportion, and at the fourth examination they had diminished to about one-tenth. Curiously enough, at the last examination, made on December 23, the polymorphonuclears had gone up to 44 per cent.

While in many cases of chloroma the blood resembles that of acute leukaemia, in my case there are some notable differences. Though there is a considerable increase in the leucocytes, they do not approach anything like the number met with in a severe case of leukaemia. The result of the examination of the blood in my case corresponded pretty closely with that of Dr. Dunlop's case at first, but whereas in my case there was a steady fall in the leucocytes from 40,600 to 23,900, in Dr. Dunlop's case the leucocytes increased from 24,500, to 54,000, then to 68,000, then to 107,000, and finally to 123,000.

I am afraid that I cannot give any help in explaining the cause of the green colour of the tumours and tissues met with in these cases. It has been stated that a pigment allied to bile is the cause, or that it is due to putrefactive changes, but both these views are now given up. Then, again, it has been practically proved that the colour is not of bacterial origin. A curious point about the colour is the varied experience of different observers as to the effect of exposure to air. The majority, however, are of opinion that the colour fades on exposure to air, or should one say to light? In the case I am bringing before you the colour of the tumours was a bright green, visible even through the skin during life, but it somewhat faded away after the removal of the growths at the necropsy.

Mr. Treadgold¹ points out that abnormal myelocytes and myeloblasts are the pathogenic cells in chloroma, and he advances the theory that, "possibly, a degeneration of the granular or pregranular protoplasm of these cells, or an abortive attempt to form granules, is the real source of the colour, aided by the broken-down products of hæmoglobin."

Dr. Byrom Bramwell² suggests the possibility that the green colour might be due to different causes in different cases, and even goes so far as to make the paradoxical statement that it seems to him "probable that cases of chloroma occurred in which there were no green tumours and no green lesions."

¹ *Quart. Journ. Med.*, Oxford, 1908, i, p. 276.

² *Lancet*, 1902, i, p. 552.

BLOOD-COUNT.

	November 9	November 16	November 30	December 7	December 23
Red corpuscles ...	4,100,000	3,710,000	1,690,000	1,612,500	1,140,000
Leucocytes ...	40,600	38,400	37,400	27,100	23,900
Polymorphonuclears	15 per cent.	17 per cent.	7 per cent.	21 per cent.	44 per cent. (finely granular 42, coarsely granular 2)
Monomorphonuclears					
Large ...	65	66	66	35	19
Small ...	85	83 per cent.	93 per cent.	71	44
Transitional ...	17	17	22	30	19
	3 per cent.	cent.	5 per cent.	6 per cent.	6 per cent.
	1 eosinophile, several myelo- cytes, several basophiles	—	—	Eosinophilous myelocytes, 8 per cent.	Eosinophilous myelocytes, 12 per cent.; few nucle- ated red
Hæmoglobin ...	60 per cent.	50 per cent.	30 per cent.	35 per cent.	18 per cent.
Serum to corpuscles	3 to 1	—	4 to 1	4 to 1	—
	300 leucocytes counted	200 leuco- cytes counted	200 leuco- cytes counted	Eosinophiles in- creased; prob- ably many of the large mono- morphonuclears are myelocytes, but they stain indifferently	250 leucocytes counted

Note.—On several occasions nucleated red corpuscles were found in the films.

PATHOLOGICAL REPORT ON CASE OF CHLOROMA.¹

By R. G. HEBB, M.D., and J. M. BERNSTEIN, M.B.

Necropsy made December 25, 1907, eighteen hours after death.

Body somewhat emaciated. The head, circumference of which is 20 in., is irregularly bossed by masses of growth in the temporal and parotid regions. There is marked proptosis of the eyes. The cutaneous veins over the bosses are much in evidence. On removing the skull-cap and brain, growths are seen in the ethmoidal region, in the middle fossa and near the end of both lateral sinuses; other prominent growths are found in the orbits and in the dura of the vertex. On reflecting the skin of the face and sides of the head chloroma-growth

¹ "P.M. Book," xviii, f. 200.

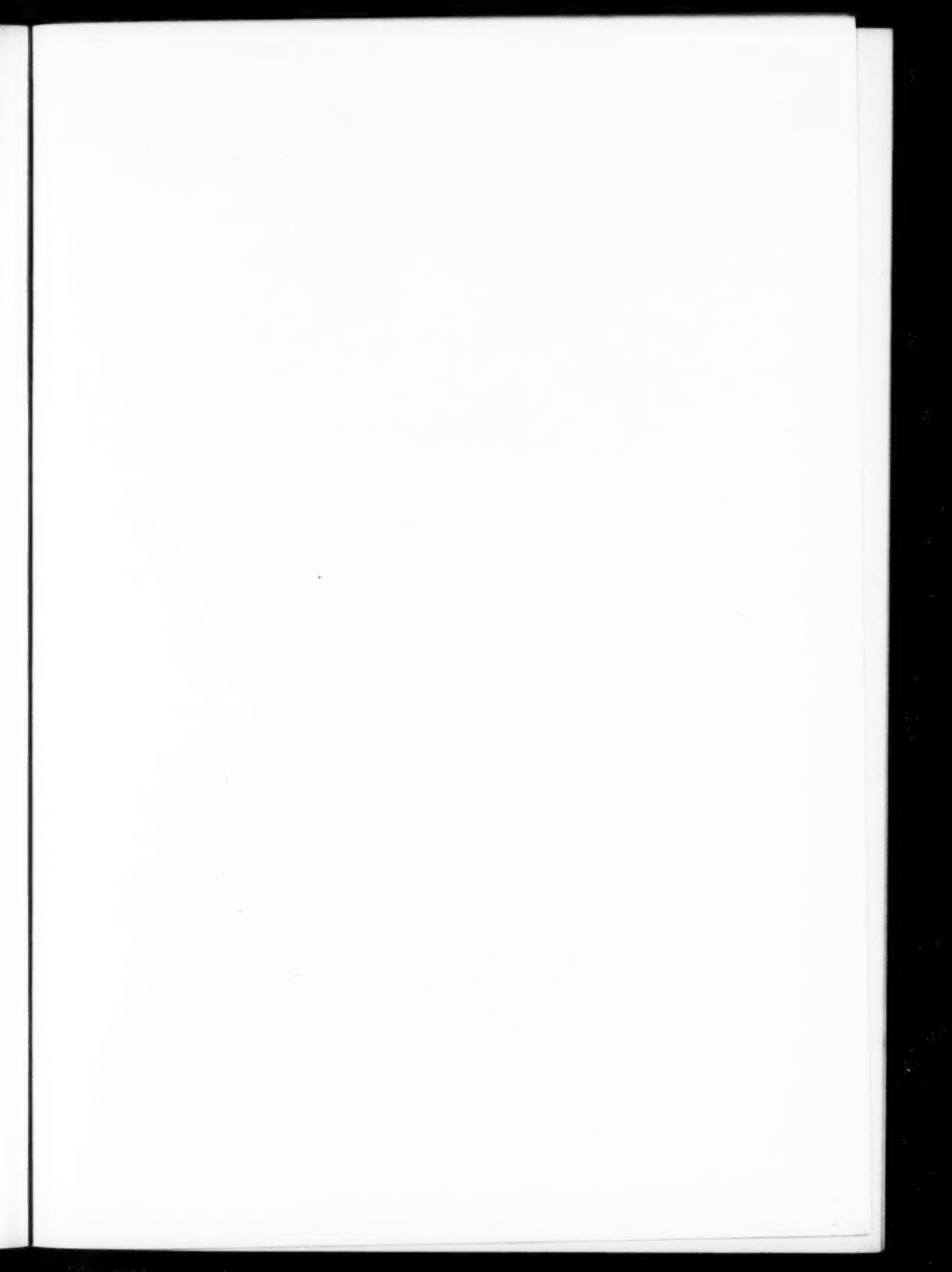




FIG. 1.
Right middle fossa.



FIG. 2.
Left iliac bone.

DE HAVILLAND HALL, HEBB & BERNSTEIN: *Chloroma.*

is seen implicating the periosteal tissue of the malar, temporal and sphenoidal regions and invading the fossæ.

The largest intracranial growth is in the right middle fossa (*see* Plate). This, which is about the size of a small walnut, has eroded the superjacent temporosphenoidal convolutions, the cortical tissue around the depression being slightly but distinctly green. In the trunk chloromatous tissue is found on the internal aspect of all the ribs, the innominate bones and sacrum, and on the external surface of both scapulæ in the sub-spinous regions. The long bones (only one examined) appear exempt. All the growths which are periosteal in location are of an olive-green hue. The colour faded somewhat after the lapse of an hour or two. The cancellous tissue of the bones was at the time of the necropsy red. The liver, spleen and heart gave a slight iron reaction. There was nothing else specially noteworthy, and except for their pallid and anæmic condition the thoracic and abdominal viscera were normal to the naked eye.

Microscopical examination of the following tissues and organs was made: Spinal cord, heart, lymphatic glands, bone-marrow, spleen, kidneys, adrenal, liver, blood-clot.

Microsections of the obviously affected tissues show that the growth is a small to medium-sized round-celled sarcoma, and as the parts affected are almost entirely periosteal there is a considerable amount of connective and fibrous tissue ramifying throughout, so that perhaps the most embracing histological definition might be expressed as a highly cellular fibrosarcoma. The cells are, however, not only the dominant but the essential feature. The cytoplasm of many contains a pigment which, when inspected in the fresh state, is, under the microscope, of a greenish-yellow hue, homogeneous and oily looking.

In material treated in any way—*e.g.*, in sections after fixation and staining—it becomes granular and yellow, and in most preparations disappears or is recognizable only with difficulty. It may here be noticed that, notwithstanding the bone-marrow at the time of the necropsy looked red, the presence of pigmented cells is demonstrable in unstained preparations. The same remark holds good for the spleen. These pigmented cells are mostly scattered and infrequent. In teased-out preparations of formalin-preserved material which has retained its olive-green hue the pigmented cells are unrecognizable after a lapse of six months, hence the pigment is probably more or less slowly soluble in aqueous media. Formalin solution after nine months is quite green. Too much has been made of the fading colour of the pigment. After

fourteen months solution is still green, but intensity has greatly diminished. Hydrogen peroxide, far from restoring the colour, acts in the opposite way.

The effect of the following reagents on the pigment was tested: ether, chloroform, turpentine, alcohol, nitric and hydrochloric acids, xylol, ferrocyanide of potassium and hydrochloric acid and caustic potash, lugol solution. Of these the only one which had a definite solvent or decolorizing action was 0.2 per cent. KOH, which was allowed to act for eighteen hours at 37° C.



FIG. 3.

Section of one of the intracranial growths; $\times 500$. (From photograph by Dr. Wellesley Harris.)

Special examination was made for the presence of bacteria (Gram, Löffler, Giemsa, Wright, fuchsin). None were detected, nor did the general aspect (macroscopical and microscopical) lend any hope for their discovery.

It will have been noticed that there was an increasing diminution of red corpuscles and excess of leucocytes, the monomorphic variety being predominant, and that the cerebrospinal fluid also showed a marked leucocytosis, the proportion of polymorphonuclears and monomorphonuclears being roughly the same as in the blood. These facts open up the question

as to whether the blood or the blood-making viscera are primarily at fault, or whether these aberrations are to be regarded as phenomena, consecutive and secondary to the periosteal changes. The fact that there is some direct and indirect evidence of translation of the blood-stream cuts either way, and as it is obvious that the greatest amount of change exists in the periosteal tissue, it is only reasonable to suppose that the implication of the blood and cerebrospinal fluid is secondary.

If all the recorded cases had been of exactly similar type, the foregoing conclusion might fairly well be considered legitimate, but as the post-

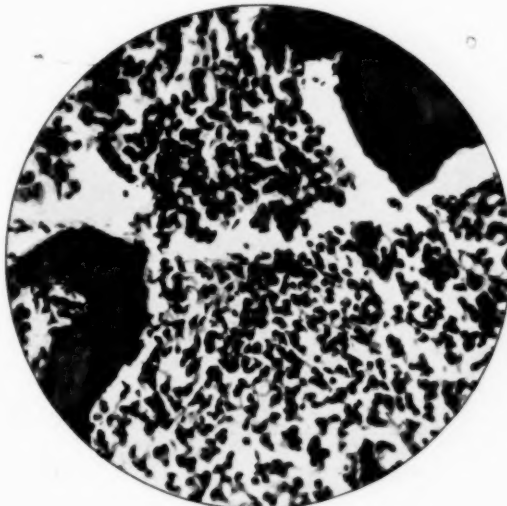


FIG. 4.

Bone marrow; $\times 500$. Two spicules of bone with intervening sarcomatous tissue.
(From photograph by Dr. Wellesley Harris.)

mortem findings in other recorded instances have shown marked differences, these deviations must be noticed. Thus, in Pope's case,¹ the spleen was very large, 70 oz.; in others there have been numerous naked-eye chloromatous deposits in the thoracic and abdominal viscera and elsewhere. In another disease, splenomedullary leukæmia,² it was noted that the lymphoid deposits had a green colour, and this instance opens up the

¹ *Lancet*, 1907, i, p. 1351.

² Türk's case, quoted by Abrahams, *Allechin's Manual of Medicine*, 1903, v, p. 668.

question whether Pope's case may not have been similar. Dunlop's case¹ and Butler's case² are very much like Dr. Hall's, both in regard to general appearances and the condition of the blood.

Resemblances have been discovered between chloroma, splenomedullary, lymphatic leukæmia, and acute lymphæmia, but the macroscopic evidence hardly supports these views, the chief arguments in favour of the similarity being the condition of the blood. Now blood-states in relation to any particular disease are not sufficiently well known to afford a certain criterion for coming to a correct decision in these

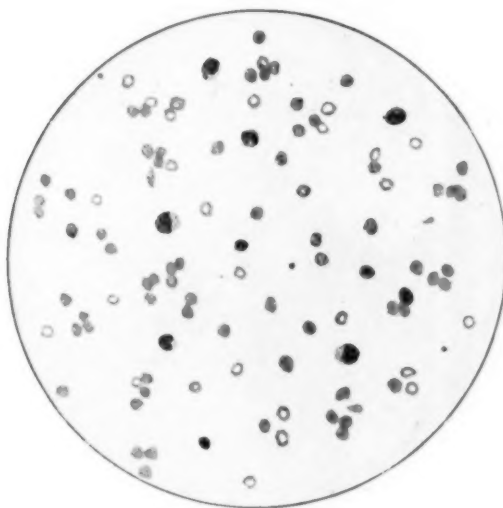


FIG. 5.

Blood-film; showing characteristic lymphocytes.

matters, and therefore to say that because a particular disease is accompanied by a diminution of red corpuscles, by the presence of nucleated red corpuscles, and by an increase of whites without taking, not only the total number and the relative number of the different kinds of whites, but other and more striking findings into consideration, should not warrant drawing a comparison between these and inferring that they may be one and the same disorder under different aspects.

¹ *Brit. Med. Journ.*, 1902, i, p. 1072.

² *Brit. Med. Journ.*, 1907, i, p. 929.

During the present year one of us had under his care a case of lymphæmia in which examinations of the blood and of the cerebrospinal fluid showed the presence of enormous numbers of monomorphonuclears (over 300,000), but there was no trace of coarse macroscopic abnormalities, and the bone-marrow was yellowish, pale, gelatinous. Yet this disorder has been dragged into a comparison with chloroma, with which it has practically nothing in common.

After all is said the question amounts to this, that there are certain clinical syndromes associated with various and varying expressions of

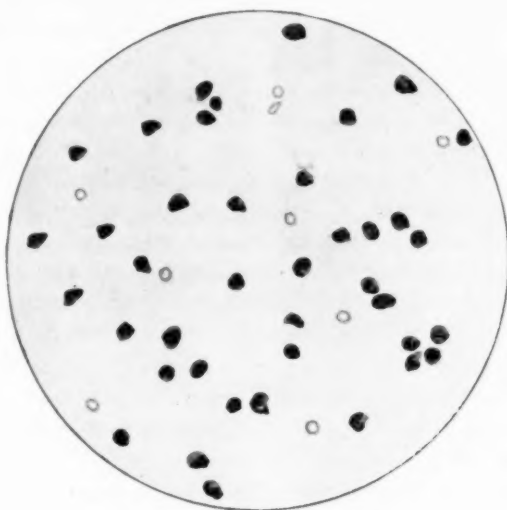


FIG. 6.

Cerebrospinal fluid; showing marked lymphocytosis.

anæmia, some of which are complicated by the presence of a green pigment, but the clinical symptoms and the morbid anatomy appearances are so different and the analogies so trifling that it becomes unwise to draw the conclusion that these disorders are too closely associated. At any rate, with regard to chloroma we may express the opinion that in the present state of knowledge this should be regarded as a disease *sui generis*, and that any likeness to other disorders is accidental or superficial.

Remains the one prominent factor common to all cases described as chloroma—viz., the pigment. With regard to this it may be asked if it be the same in all the recorded cases, and if so, what is its nature? It has been stated to be a lipochrome, but from the results of our examination we think on insufficient grounds. As will be seen from the specimens of the present case the growths have retained fairly well their pristine hue, though no doubt much has been lost owing to the solubility of the pigment in aqueous media, a fact which also accounts for some diffuse staining of tissues and of bone-marrow.

As at present no exact and satisfactory analysis of the pigment has been made, some doubt must still remain as to whether the nature and composition of the pigment in all described cases of chloroma has been exactly the same. At first sight it might seem that the green pigment might be a part of the anæmia, but if this were so thin green-coloured tissues, &c., would be very common instead of being extremely rare.

One more point to be noticed—viz., that in the typical cases of chloroma, of which we think this is one, the growth is very marked about the ethmoidofacial region, and we are inclined to think from the analogy of other growths that this is the starting-point of this peculiar disease. What there is in this region which gives rise to the characteristic cells and pigment it is difficult to surmise, though it is conceivable that embryology might supply an answer to the question.

Dr. R. G. HEBB showed some lantern slides of the appearances in cases of chloroma. One of the cases was noted by Dr. Melville Dunlop. The slides comprised drawings of chloroma tissue, showing the connective tissue element and also the cells. Coloured photographs presented a view of sections of bone, with the characteristic cells in the marrow, while one photograph (in Dr. Dunlop's case) showed the chloroma growths in the liver.

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DISCUSSION.

Dr. T. HENRY GREEN said that the Section was greatly indebted to Dr. Hall and his colleagues for bringing forward the interesting case upon which their paper was based. He complimented them upon the very great care and thoroughness with which the case had been studied, and said that when the paper was printed it would be an important contribution to the records of the Section. Cases of chloroma were rare, and very few examples had been brought before the societies. He only knew of one Fellow who said that he had seen a case. If any member present had had the opportunity of examining this condition it was really his duty to mention it in the course of discussion.

Dr. A. E. GARROD pointed out that there was another morbid condition met with in children which, in its clinical aspects, resembled chloroma very closely; so much so that the pictures shown of Dr. Hall's patient would serve very well to illustrate its features. He referred to the sarcomatous growths upon the skull and in the orbit, which are associated with sarcoma of the suprarenal capsule. This disease was certainly less rare than chloroma, and he had had three cases of the kind under his care at the Hospital for Sick Children during the past few years. One of these was included in the series of cases brought together by his colleague Dr. R. Hutchison in his paper in the first number of the *Quarterly Journal of Medicine* (October, 1907, p. 33). In each instance chloroma had been suspected, but had been excluded by the examination of the blood. In the most recent case the child was admitted with rheumatism and a cardiac murmur, and hæmorrhage into the upper eyelid, soon followed by the development of exophthalmos, was the earliest sign of the graver trouble. As regards the pigment of chloroma, it would appear from the coloured photographs shown that in the present case the green colour was much more intense than in a case which he had seen a year or two ago; the pigment of which he had attempted to investigate. The colour quickly disappeared from specimens preserved in alcohol, even although they had been kept in a dark cupboard. The pigment was certainly not biliverdin, nor was it probable that it was any other derivative of hæmoglobin. In some respects it resembled lipochromes, and among these the serum pigment, which could easily be converted into a green colouring matter by appropriate treatment. However, as Dr. Hebb's observations showed, it had not the solubilities of fats or of lipochromes. Obviously the nature of the chloroma pigment was still in doubt.

Dr. DE HAVILLAND HALL said that he was greatly indebted to Dr. Garrod for his interesting statement. He was aware that the protrusion of the eyes and distension of the veins over the temporal region in the first instance might be indicative of sarcomatous growths instead of chloroma, but he pointed out in his paper that in the diagnosis of chloroma they had to take into

account not only the protrusions of the eyeballs and the growth of lumps in the neck, but also the condition of the blood. As soon as the blood-count came to be examined it would show whether the effects were due to a sarcomatous condition or to this chloroma, which was definitely associated with leucocytosis. There was no difficulty in making the diagnosis if once the idea of chloroma presented itself to the medical man in charge of the case. With regard to the pigment, the greenish colour in the masses in the temporal region was certainly marked during life. He would leave Dr. Hebb to make some remarks upon the cause of the peculiar coloration.

Dr. HEBB said that he could not undertake to tackle Dr. Garrod on this subject, and he had stated all that he knew in the paper. There was only one contribution he could make to the question of the pigment. The assertion that the colour was rejuvenated after a time proved to be quite contrary to the fact. There was no effect of the kind even when the material was kept in formalin for months. This suggested to him that the kind of pigment might be different in different cases. It was unfortunate that the opportunity of examining the effect of the fresh pigment on polarized light was lost.

Medical Section.

March 23, 1909.

Dr. T. HENRY GREEN, Vice-President of the Section, in the Chair.

Auto-inoculation versus Hetero-inoculation in the Treatment of Established Infective Disease, in Pyrexial and in Apyrexial Conditions, as controlled by (a) Clinical Observation, (b) Estimation of the Antitryptic Index.

By E. C. HORT.

As I shall frequently use the term hetero-inoculation throughout this paper I may say that it is a word coined, from obvious analogies, to denote inoculation with immune serums, and with tuberculin and other vaccins from without.

All hetero-inoculative treatment, as at present employed, is based on the assumption that successfully to overcome infection the tissues have only to defend themselves against extracellular factors such as bacteria and their products. No allowance whatever is made for the possibility that even in diseases admittedly bacterial the tissues may have intracellular (non-bacterial) foes to contend with as well as extracellular.

This neglect of cytological pathology in its relation to infection is, I maintain, of fundamental importance. For it retards, I submit, all progress in the treatment of infective disease and explains the disappointing results that follow the administration of many antitoxic serums, antimicrobial serums and bacterial vaccins. And, further, it demonstrates the futility of attempting satisfactorily to gauge immunity response by measuring such purely bacterio-tropic, and therefore relatively unimportant, bodies as agglutinins, præcipitins, opsonins and the like.

That cells may be attacked from within as well as from without is an obvious truism. But no serious attempt seems yet to have been made to define the nature of those intracellular processes, however set in motion, which tend to break down that state of immunity to extrinsic and intrinsic foes on which healthy cell-life depends. It appears from recent research that amongst the potential enemies of a cell from within must be ranked its own intracellular enzymes, and the products of their morbid activity.

Unless these are constantly restrained, equilibrium cannot be maintained. It is not the stomach only that can, under suitable conditions, autolyse or digest itself. Every tissue in the body has similar powers. That the body as a whole does not digest itself is apparently due to the inhibitory action of its anti-enzymes. These seem to be incited into being and action by the intracellular enzymes, on the strict analogy of toxin and antitoxin. Between perfect integrity of a cell and the completed act of autolysis there are many grades of cell-aberration. Fatty degeneration, cloudy change and many other processes, including possibly even cancer itself, are familiar examples. That under certain circumstances the products of morbid enzymic activity may be themselves auto-intoxicative is well known. Between demonstrable auto-intoxication from the products of autolysis and other anomalies of enzymic activity and its earliest stages there must also be an infinite number of grades.

Under the stress of life there could be no escape from one or other of these aberrations unless there were some restraining influence constantly at work to hold a tendency to perverted action in check. This restraining influence may be defined as the protective response called out by cellular enzymes and their products whenever cell-integrity is threatened by anomalies of cell-function. In the evolution of cell-maintenance, it is only this power of restraint that enables a cell not only to remain a cell of its own kind, but to remain a cell at all. In other words, natural selection transforms what was in the first instance accidental into protective purpose. To paraphrase Herbert Spencer, cell-life is the equation of cell-reaction. But the conception of reaction must not be confined to reaction against outside forces. It must also embrace a cell's reaction against its own enzymes, and against the products of enzymic activity. The very weapons with which a cell can destroy itself incite the restraint necessary to avert such disaster. When, therefore, a cell is damaged by bacteria or their toxins, its powers of restraint of what are then abnormal enzymic metabolic processes are

impaired. It can of its own accord only return to a normal state by inciting responsive reaction in the fertilizing lymph that supplies it. The sign of restoration is the exhibition of appropriate restraining bodies. Cell-life is, in fact, inconceivable in the absence of such restraint, which is the only intelligible explanation of the horror auto-toxicus postulated by Ehrlich.

Hence bacterially infected tissues must, I maintain, defend themselves, not against one factor only, bacteria and their products, but against many. Amongst the remaining factors are their own enzymes, and the products of their own enzyme activity. If attack is plural, defence must be plural. Antibody production *qua* bacteria will do much, only, however, by protecting from bacterial harm cells still undamaged. It can, however, in no sense immunize cells against anomalies of metabolism when induced from within, which may as often precede bacterial infection as follow it. Neither can it directly enable cells already damaged to restore the condition of immunity on which effective cell-life depends. Many cells, in any given infection, no doubt, are injured beyond repair, but there must be also hosts of cells not beyond hope of recovery, whether spontaneously or artificially induced. For such as these no antitoxin, bactericidal body, nor opsonin can be of the slightest avail, except by preventing further injury. If this be so, artificial specific treatment, which aims at production of bacterio-tropic bodies only, to the exclusion of cellulo-tropic, can never meet with full success, so long as tissue damage is extensive. Further, the economy itself can never be relied on to spontaneously effect a cure unless it can consider the cellular element as well as the bacterial. The very fact that more people get well of infection than die of infection is strong proof that auto-cellulo-tropic restraint does actually and naturally occur, and is also the strongest possible argument for artificially inducing such restraint when natural efforts fail.

If the exponents of the hetero-inoculative method had not confined themselves to the use of vaccins of bacterial origin, and of artificial immune serums, dependent for their value on the production of bodies inhibitory to bacteria and their toxins only, there would have been the less excuse for this communication. Inasmuch, however, as the value of artificial vaccins made from emulsions of cells and cell-enzymes, and of immune serums dependent for their value on the production of bodies inhibitory to anomalies of cell-metabolism, and to the products of such, has been neglected, the possibilities of such enlargement of the scope of hetero-inoculation are worth exploring. No doubt the inherent

difficulties of preparing cell-emulsions and enzymic vaccins are very great. There can, however, be little question that if they could be satisfactorily prepared, the addition of such to the armoury of the hetero-inoculator would enormously enhance the effect of his bacterial vaccins and serums. Even then, however, the combined value of artificial inoculation with cellular and bacterial vaccins would be considerably less than the same weapons when autogenously wielded, as what I have to say on the subject of auto-inoculation is intended to show.

It has always been laid down that tissue resistance must be reckoned with in the treatment of established infective disease, and that the object of general treatment is to provide for this factor. But hitherto we have only imagined that tissue resistance must be encouraged against extrinsic attack. We have, I maintain, not realized that it must also be encouraged against intrinsic. Obviously by adequate food supply and so forth we can encourage cell-nutrition and so indirectly aid in passive immunization against extracellular and intracellular force. But to promote cell-nutrition is not to ensure cell-restraint. No amount of general treatment can ensure active immunization. All it can do is to provide material from which the cells and bacteria can incite bacterio-tropic and cellulo-tropic restraint, or in other words to make possible specific stimulus from without and from within. We believe the former to be the essence of specific treatment *qua* bacteria, whether such treatment be naturally or artificially induced, and there is much evidence to show that exactly the same principles are applicable to specific restraint of intracellular enzymes and their toxin-like products.

If these observations are well founded it is clear that future progress in immunity research is impossible unless it be based on the study of the physiology and pathology of intrinsic cellular change. Such study must embrace the phenomena of autolysis and other anomalies of cell-metabolism and their relation to intracellular enzymes. Further, it must concern itself with intoxication from the by-products of morbid enzymic activity, and, above all, with the methods by which restraint of all these phenomena is naturally maintained. If it be true that immune response to infection should be expressed in terms of cellulo-tropic and bacterio-tropic restraint it is clear that no method of measurement which confines itself to the latter only can ever be of any great practical value except in diseases in which cellular disturbance is comparatively slight. Apart from this consideration is the question of reliability of present methods of estimating degrees of immunity by blood examinations. As far as tuberculosis is concerned I have recently shown that

in thirty-eight out of fifty-one observations on tuberculous serums no two observers could agree as to what any tuberculo-opsonic index actually was. Hence in this disease the opsonic method of estimation at least is at present quite unreliable. I now go further and submit that these elaborate blood examinations are unnecessary in tuberculosis and other infections so long as there is a marked fever. I am, in fact, about to show that temperature charts appear often to be far better guides than any hæmatological method yet devised, because they reflect the economy's natural attempt to deal with *all* the factors in infection—namely, the cellular as well as the bacterial. Such claim is based on this dual understanding:—

(1) That so long as there is free communication between an active focus of infection and the lymph-stream natural or spontaneous auto-inoculation is potentially always in operation.

(2) That whenever infection undergoes spontaneous cure, without, that is to say, artificial specific aid, such event can only take place by a process of spontaneous auto-inoculation, the infected tissues and the bacteria being then the inoculating agents.

With auto-inoculation, spontaneous and artificial, and its relation to fever as a thermometric index, I am therefore now concerned.

History.—The history of the evolution of auto-inoculation as a therapeutic measure to be deliberately applied is intimately associated with that of hyperæmia, whether such hyperæmia be a natural phenomenon, as in inflammation and spontaneous inoculations, or artificially induced, as in any of Bier's methods. Inflammation, as we know, so long as it is neither inadequate nor excessive, is largely a protective and curative mechanism. The protection afforded thereby is partly due to transference of preformed tropic substances to a damaged area, and partly to the restraining response called out by stimuli from tissue-cells and bacteria. In other words, to spontaneous auto-inoculations employed by Nature from the beginning of time, though unrecognized by us.

Artificial auto-inoculation has been unconsciously applied from time immemorial. Whenever fomentations, poultices, stupes, setons, blisters, and the like have been applied in the treatment of established infections of joints, skin, serous sacs, sense organs, abdominal or thoracic viscera, &c., our forefathers have unwittingly employed processes of active and passive immunization by inducing a form of artificial hyperæmia. Such hyperæmia may be directly induced, or indirectly by the agency of neurons. This is also true of other methods of inducing artificial hyperæmia, whether arterial or venous. When, for instance, Ambroise

Paré treated fractures with obstructive bands, or when early Egyptian physicians employed light and heat baths, artificial auto-inoculations were in use. In 1894 the possibilities of active and passive hyperæmia were insisted on by Bier, whose methods have since invaded every domain of medicine and surgery. The conception did not, as is often supposed, originate with Bier. As pointed out by Adami, Bier's methods were based on the observations of Farre, Travers, Louis and Frerichs. Even to-day, however, Bier does not seem to have fully realized on what a sound basis his work stands. He makes no mention of auto-inoculation, and yet it is to this factor, as much as to any other, that his happiest results are due. As an instance in point may be given the use of the mask in inducing hyperæmia in the treatment of apyrexial cases of pulmonary tuberculosis, a method which clearly foreshadowed the modern adoption of graduated labour in the treatment of that affection. The conscious and deliberate employment of the principles of auto-inoculation in the treatment of apyrexial cases of pulmonary tuberculosis will always be associated with the names of Freeman, Meakin, Wheeler, Paterson, and Inman. The remarkable results obtained by Paterson, and elucidated by Inman, in the use of graduated labour, are well known, though these authors have unnecessarily restricted their field to the treatment of apyrexial cases, as I shall presently show. The history of spontaneous auto-inoculation is of much greater antiquity, dating back as it does to the first invasion of man by infective disease. Whenever in man such disease has obtained a footing, and has run a chronic course extending into months and years, and whenever the subject of such disease has been cured or has progressed towards cure without the aid of outside specific treatment, spontaneous auto-inoculation has been in operation. Further, even when disaster has finally overtaken him as a result of any given infection, auto-inoculative efforts have at some stage or other in his disease been at work.

If it be true that reaction against infection can be expressed in terms of the increased work done by tissue-cells, then we should expect to find a direct relation, within common-sense limits, between the amount of pyrexia and the degree of immunity production.

The possibility that temperature charts might prove to be of great service in estimating immunity response in bacterial infections was demonstrated by Dr. Arthur Latham in 1908 in a most valuable contribution to the study of immunity delivered before this Society. His observations were to some extent associated with the inverse ratio

that he found to exist in many cases between the temperature and the opsonic index. If it be held that estimations of the latter, especially in tuberculosis, are, even if consistent, not a trustworthy gauge of immunity response as a whole, some other controlling standard must be substituted. This does not, however, in any degree detract from the value of his contention that in many cases it is possible more or less accurately to read an immunity curve in terms of a temperature chart. This contention, in spite of the fact that it is still unproven from blood examinations *quâ* opsonins or other immunizing bodies, is, I submit, true, though, as I shall show, much more information can be gained from a study of temperature charts than has hitherto been elicited. Before proceeding with the evidence it is worth noticing the somewhat fallacious answer recently given to the suggestion that reliable information could be gleaned from clinical charts as to immunity production. In dealing with the subject, Sir Almroth Wright maintains that the temperature curve is a measure of intoxication but not of immunization, and that there is no direct and constant relation between such curve and the production of anti-bacterial bodies. This statement is surely a confusion of terms. It has been well said by Pembrey that fever is not an entity, but a convenient clinical term for a group of phenomena with a more or less definite sequence and origin.

If this be so, a fever chart, if it means anything at all, ought to reflect such origin and sequence.

The factors in immunization are stimulus, plus, where such be possible, response. In other words, the combination of stimulus and reply to stimulus represents what we call the act of immunization, since active immunization is obviously impossible without the impetus of intoxication. Hence if the temperature curve is also an immunity curve it must be a measure of intoxication and response. If there is no response, the temperature curve in a pyrexial case shows its absence as well as its presence, and is therefore a measure of absence of immunization. Hence, there may be, I maintain, a most direct and constant relation between a temperature curve and the production of protective bodies. That such relation actually does exist in many cases is suggested, though by no means proved, by a study of the ensuing charts.

Charts 1 to 4 are of cases of pulmonary tuberculosis from various sources who did well under general treatment. No one of them received artificial specific treatment of any kind. The charts are not, as they appear to be, ordinary charts showing diurnal variations of

temperature. In each case they represent the evening temperatures joined by a continuous line. The reason for this procedure is the great difficulty of getting a correct idea of the general course of intoxication and of response from a series of sheets recording diurnal variations. It is, in fact, impossible for the eye to receive an exact impression of a chart as a whole, if in a disease such as tuberculosis, with wide excursions between morning and evening, the ordinary charts are studied. The abbreviated method has the additional advantage of reducing to small compass the readings of many weeks. Each chart given below is capable of registering the observations of four months. In order to make clear the object of recording in a graphic manner what I conceive to be effective response to spontaneous auto-inoculation, the broken curve of declining indices has marked on it at intervals the letter X. Whenever, therefore, this letter appears, it is intended to indicate the crest of an effective auto-inoculative wave. It will be noticed that each crest is, as a rule, lower than preceding crests, higher than succeeding crests, and is separated from its fellows by intervals of from one to fourteen days. The smaller waves noticeable in the charts are not lettered.

Chart 1 is from a case of pulmonary tuberculosis with well-marked signs in both lungs. The sputum contained tubercle bacilli. The total gain in weight during treatment was 19 lb. Improvement was progressive. The case is of interest because not only does it show to perfection the broken curve of declining indices, but it also shows the absence of this curve in the first month of treatment before there was any sign of clinical improvement.

Chart 2 is from a case of pulmonary tuberculosis under treatment for five weeks only. The broken curve of declining indices coincided with well-marked clinical improvement.

Chart 3 is from a case of pulmonary tuberculosis published by Dr. F. W. Burton-Fanning, and treated by him in 1896, under the open-air *régime*. Tubercle bacilli were present. Fever declined, he said, after four months' treatment, and in a further seven months all physical signs and symptoms, as well as tubercle bacilli, had disappeared. The total gain in weight was almost 14 lb. He was well and at work twelve months after discharge. The case is interesting on account of the favourable course with many weeks' high fever. The chart shows very well, not only the broken curve of declining indices, but also the sharp elevations at X1 and X2, which seem to have been followed by great improvement.

PULMONARY TUBERCULOSIS.

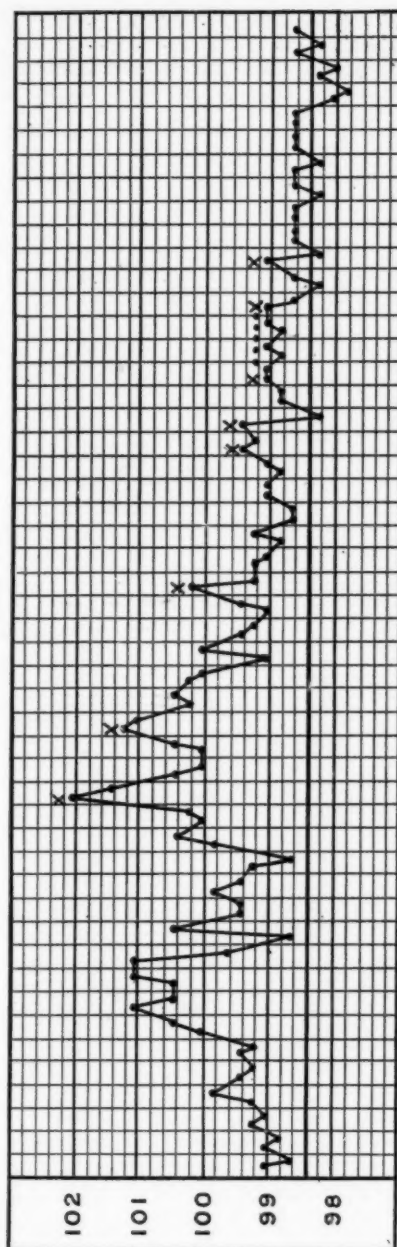


CHART 1.
Continuous evening chart.

PULMONARY TUBERCULOSIS.

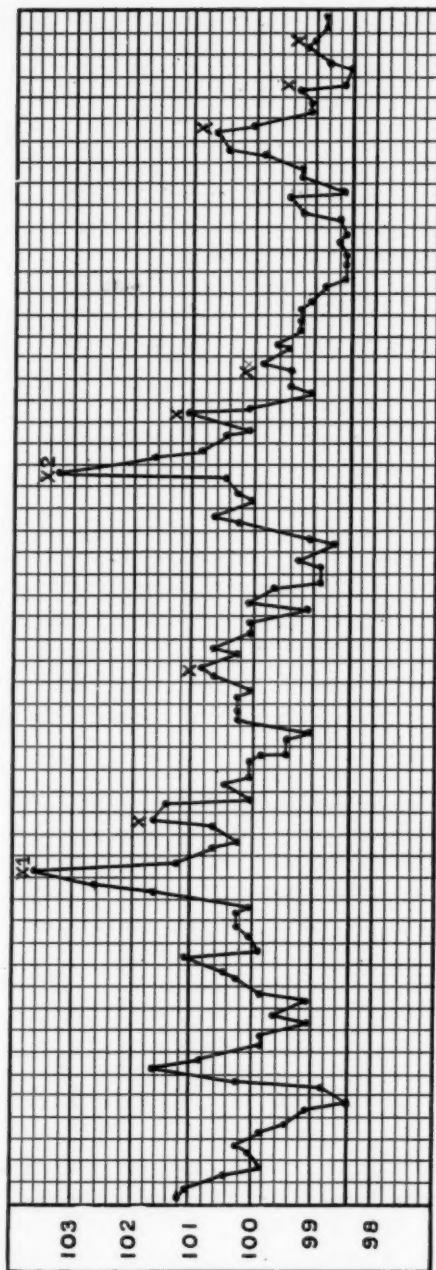


CHART 3.
Continuous evening chart.

inoculation which was again followed by a further declining curve and final descent to nearly normal.

Chart 5 is from a case in whom no attempt at inoculative treatment was made. He went steadily from bad to worse, and the chart shows almost total absence of attempts at effective response to spontaneous auto-inoculation. This chart is reproduced in order to demonstrate the reverse picture to that shown in Charts 1 to 4.

Charts 6 to 10 represent broken curves of declining indices in cases of enteric, scarlatina, influenza, and non-tuberculous broncho-pneumonia. They are reproduced to show that the picture of spontaneous and effective auto-inoculation is not confined to tuberculosis. The two enteric charts are continuous evening temperatures, the morning temperatures being again omitted as obscuring the point at issue. The influenza chart is a four-hourly one. In acute infections running a short course, such as scarlatina, influenza and measles, the insertion of morning temperatures is essential to correct interpretation. Since the remittency is slight there is no difficulty in reading.

Chart 11 represents an exactly opposite type to that shown in Charts 6 to 10. Effective response to auto-inoculative stimulus is, as a rule, conspicuously absent in cases in which death finally occurs.

This is a convenient place to notice that in some cases ultimately ending in death a broken curve of descending indices is occasionally shown almost up to the time of death. In such cases, however, it appears that, although response to auto-inoculation may be fair, the economy is unequal to the strain, and the ship goes down, literally with all flags flying. This is well seen in Chart 5, from A to B.

In order to represent more graphically still the contention that a temperature chart may often indicate with great accuracy presence or absence of effective response to auto-inoculation, the following auto-inoculation curves are reproduced.

Chart 12 is moulded from temperature Chart 2, and will be found to represent with sufficient accuracy for practical purposes the rise and fall of intoxication and response, leaving out of account minor variations.

Chart 13 is moulded on Chart 1. While Chart 12 represents undisturbed response to effective auto-inoculations, Chart 13 represents an early stage of nearly four weeks' duration in which response to such was in abeyance, and in which the patient was actually losing ground.

Chart 14 represents in the first half a partial absence of response to acute inoculation which was completely overpowered and eventually, in the second half, ended in death. The picture of Chart 14 is the inverse therefore of 12 and 13.

PULMONARY TUBERCULOSIS.

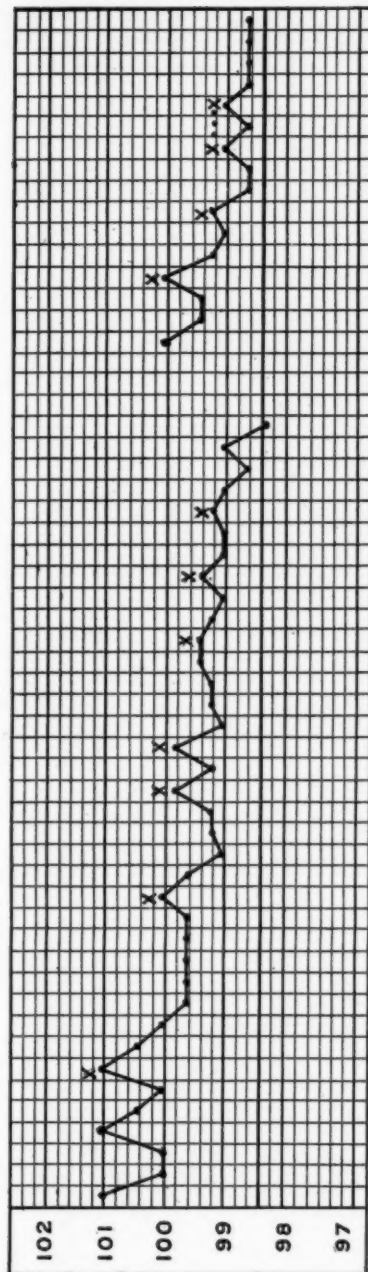


CHART 4.
Continuous evening chart.

PULMONARY TUBERCULOSIS.

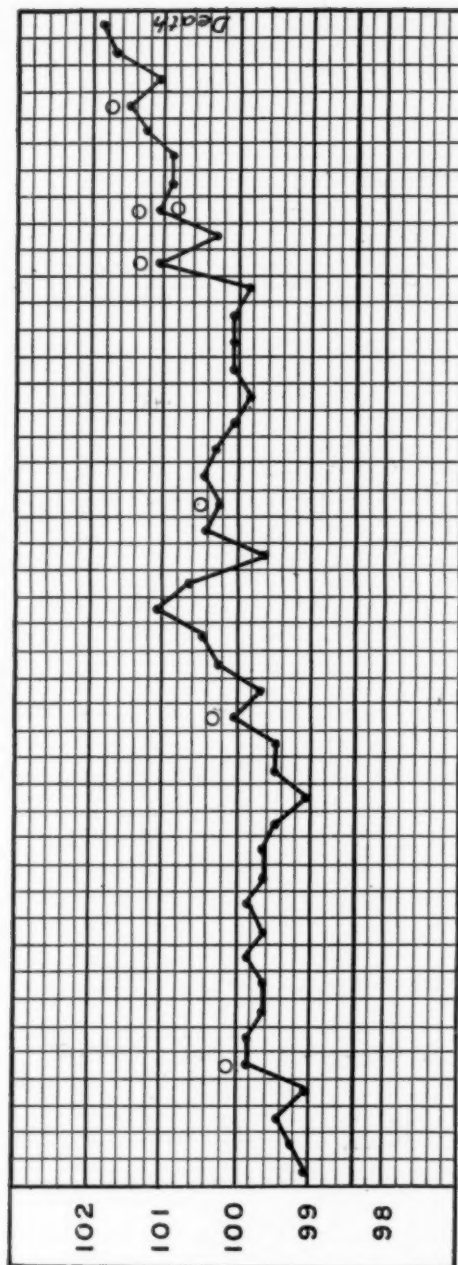


CHART 5.
Continuous evening chart.

ENTERIC FEVER—RECOVERY.

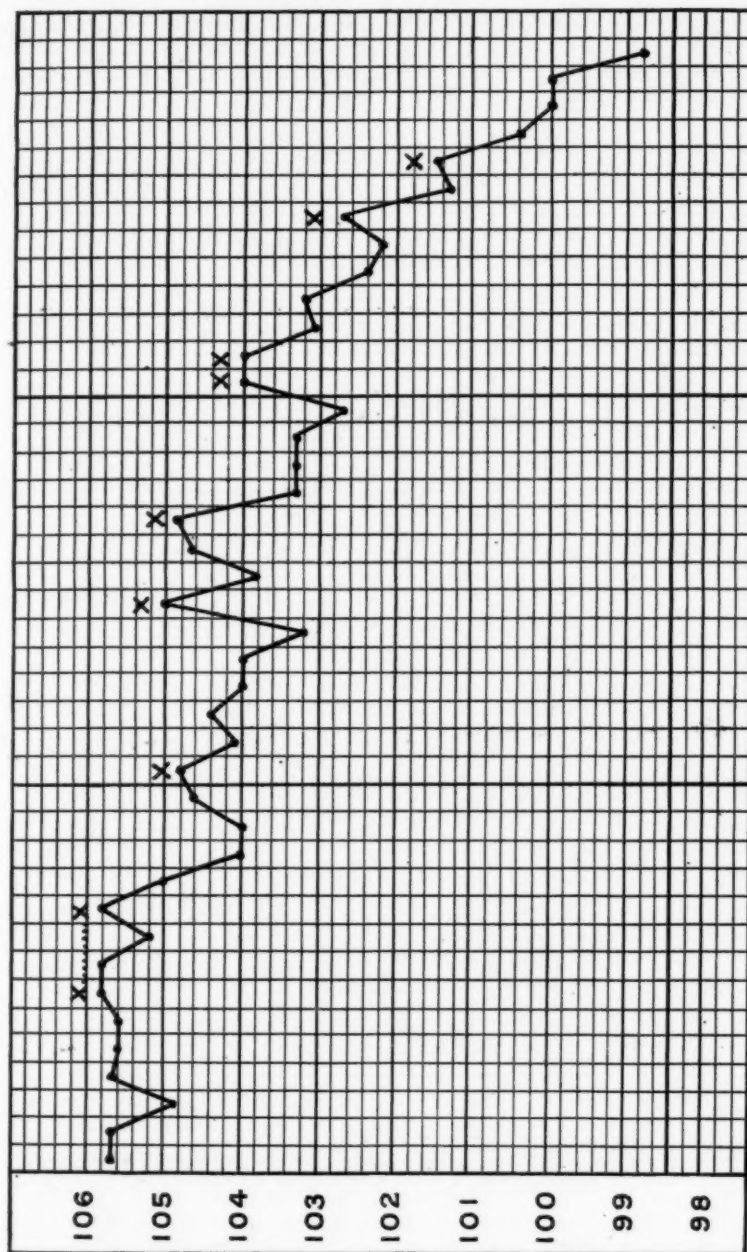


CHART 6.
Continuous evening chart.

SCARLET FEVER-RECOVERY.

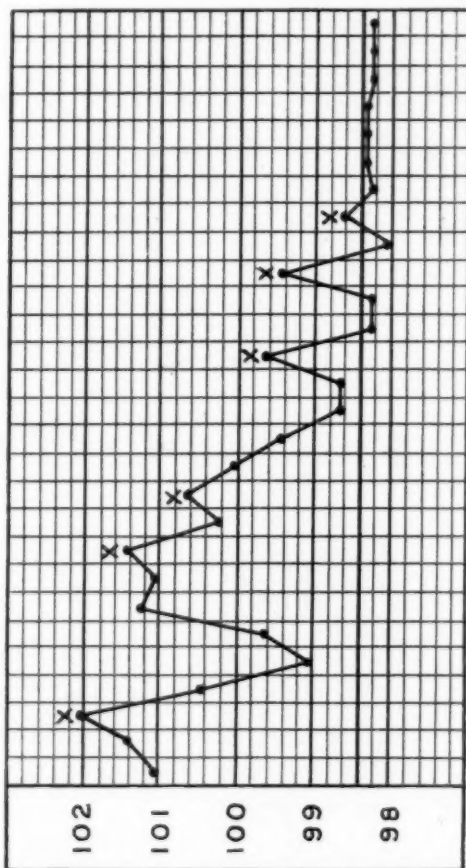


CHART 7.
Continuous evening chart.

Chart 16 represents the spontaneous inoculation curve moulded on Chart 15. It will be found to represent a striking contrast to Chart 13, in which the patient did very well, and to occupy an intermediate position between 13 and 14, Chart 14 being that of a case ending in death.

INFLUENZA—RECOVERY.

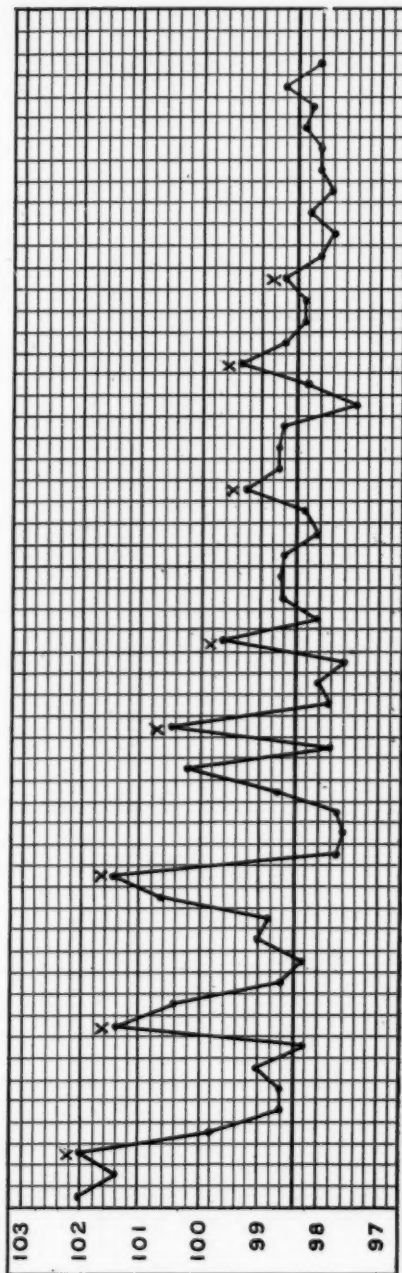
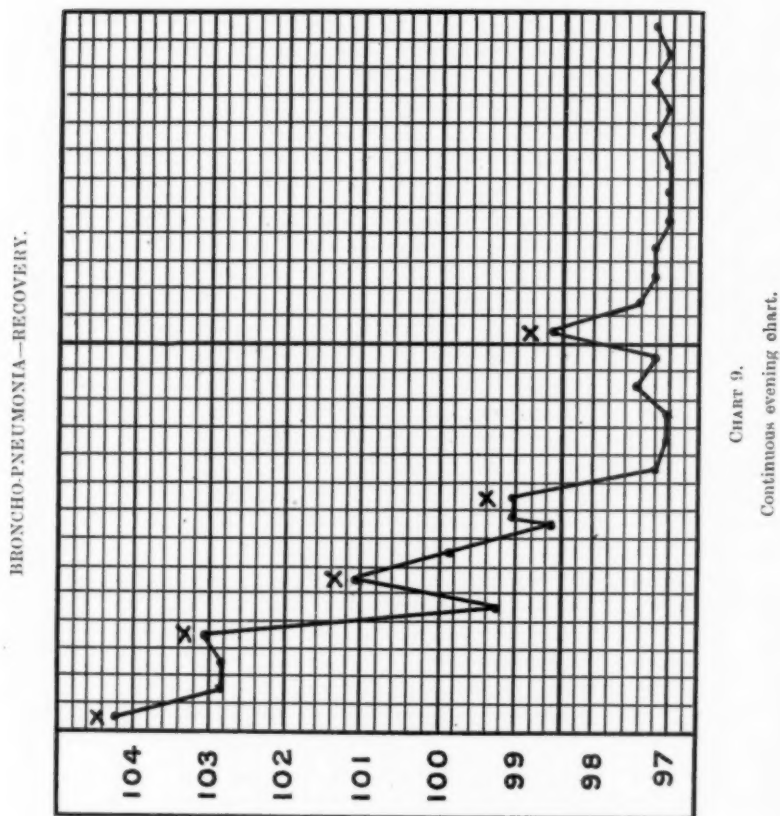


CHART 8.
Four-hourly chart.

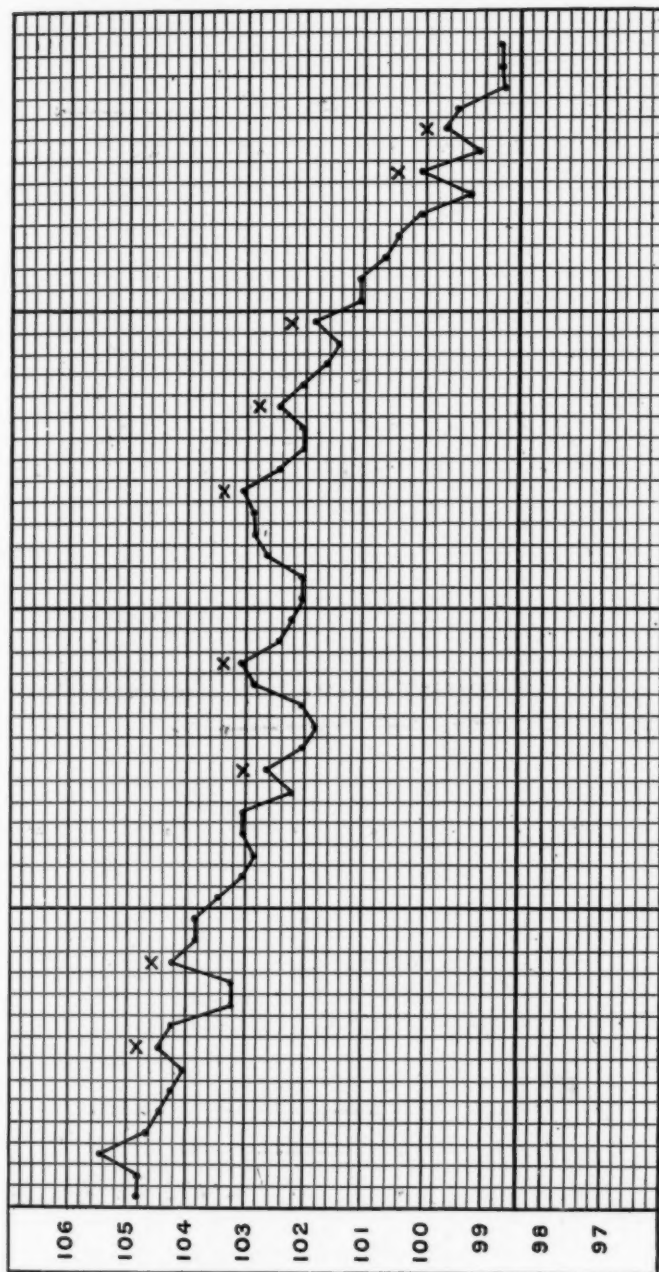
Having thus sketched the course taken in certain cases of pulmonary tuberculosis by the temperature charts and by what appear to be their intoxication-response curves where they were :—

- (a) Progressing towards cure ;
- (b) Rapidly going downhill ;
- (c) Doing neither well nor ill ;

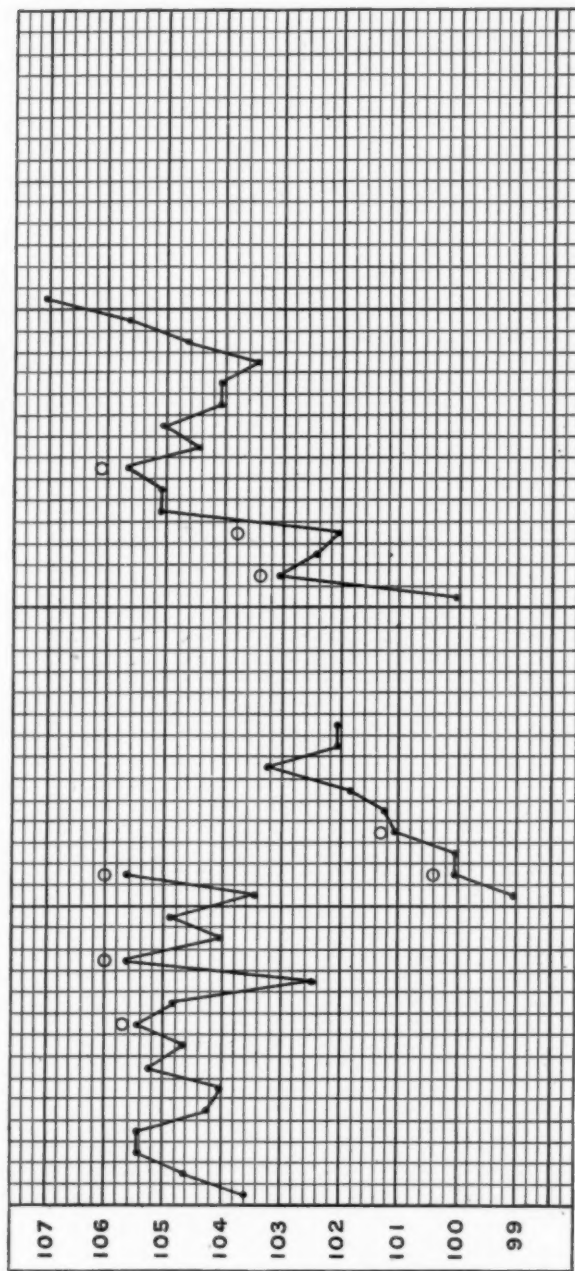


it remains to study the charts and auto-inoculation curves of cases in classes B and C in whom the course has been artificially altered by artificial means.

ENTERIC FEVER—RECOVERY.



SCARLET FEVER-DEATH. SCARLET FEVER-DEATH. MEASLES-DEATH



Continuous evening chart.

4-hour chart.

4-hour chart.

CHART 11.

PULMONARY TUBERCULOSIS.

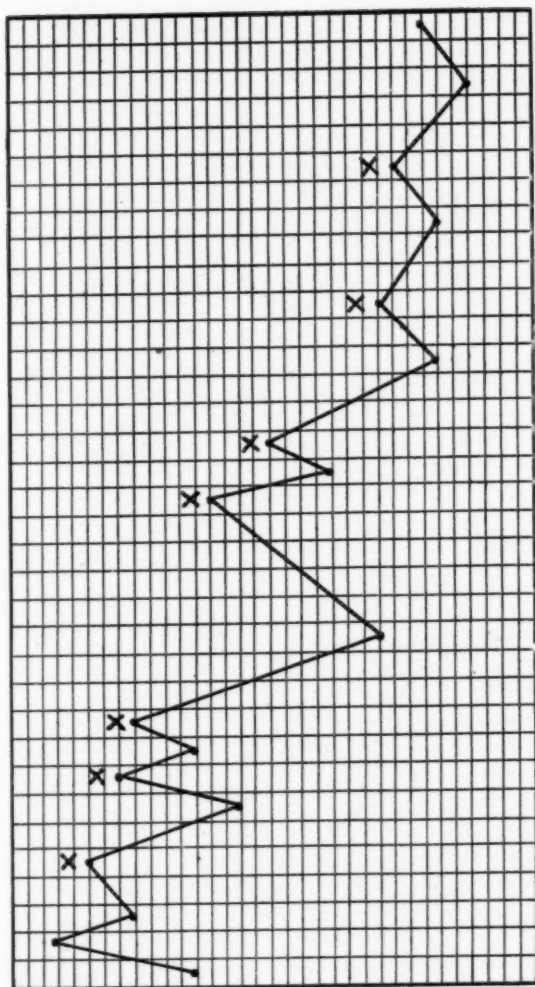


CHART 12.

Intoxication-response curve of case doing consistently well.

CHART 18.

PULMONARY TUBERCULOSIS.

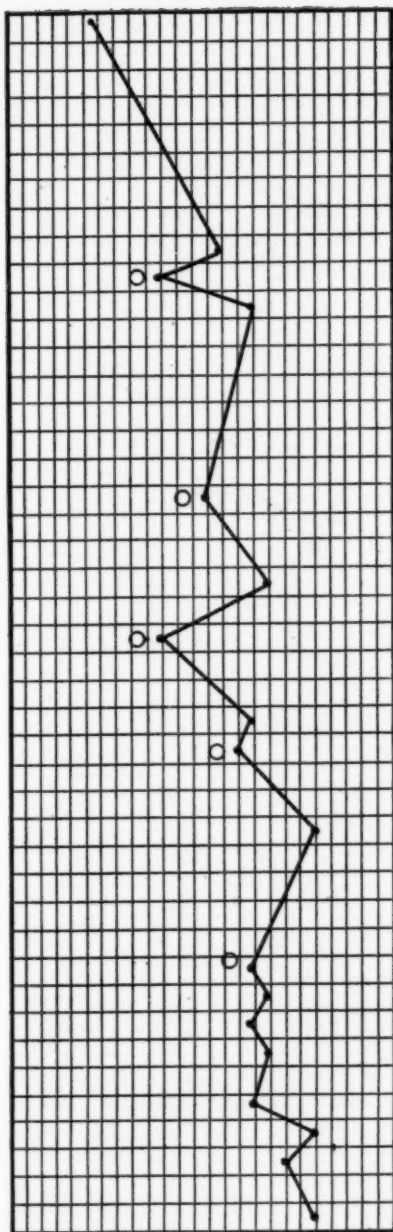
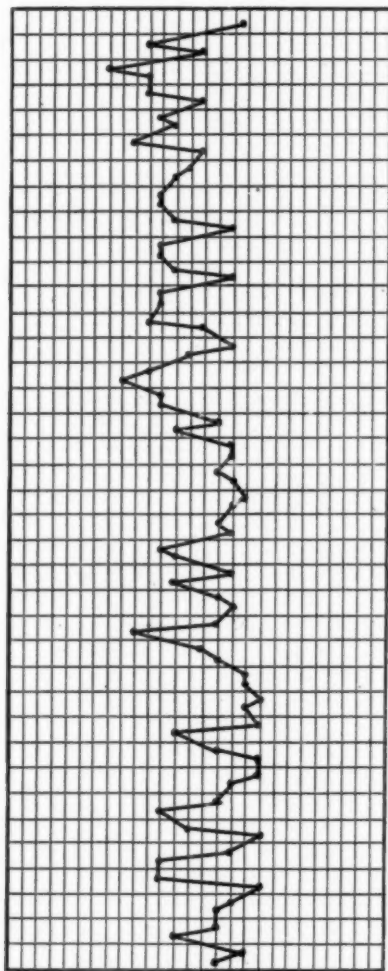


CHART 14.
Intoxication-response curve of case doing badly. Death.

PULMONARY TUBERCULOSIS.

CHART 15.
Continuous evening chart.

PULMONARY TUBERCULOSIS.

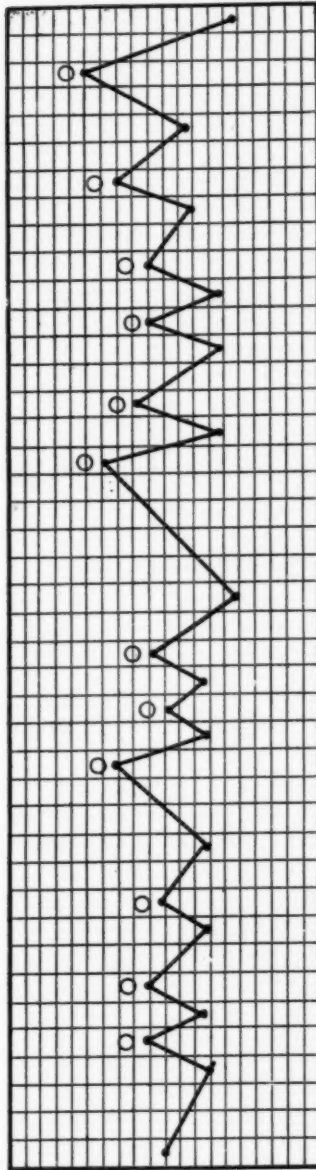


CHART 16.

Intoxication response curve of case doing neither well nor ill.

Charts 17 to 23.—Of these, 17, 20, 21, 22 and 23 are reproduced to demonstrate how far cases of pulmonary tuberculosis that are not doing well can be artificially induced to approximate to cases that are doing well. The procedure adopted in all cases was one of artificial auto-inoculation, modelled on the information gleaned from Charts 1 to 17 of natural auto-inoculation. The material is not large, but it affords sufficient justification for extended trial of the method. In other cases not here reported in which tuberculin has been employed, the results obtained bear no comparison with the results obtained with artificial auto-inoculation, although the guiding principles were identically the same.

Chart 17 represents four isolated phases where a correct auto-inoculative dose was given, as shown by the lower range of temperature following the apex of response preceding it. Such curves are apt to be misinterpreted as instances of hyper-inoculation. That such interpretation is erroneous, however, is shown by the coincidence of clinical improvement with the broken curve of declining indices. This consideration also applies to the injection or other administration of tuberculin.

Chart 18 represents three isolated phases of three different cases where the dose was actually excessive, as shown by declining physical improvement and absence of broken declining curve. It is therefore labelled a hyper-inoculation chart. This chart is a true intoxication record, with practically no evidence of response.

Chart 19 represents two isolated phases of two different cases where the dose was too small, as evidenced by practically no result following inoculation, either in the chart or in clinical signs. It is therefore labelled a hypo-inoculation chart.

Chart 20 is that of a case of pulmonary tuberculosis with moderate fever, but losing weight and with extending physical signs. Tubercle bacilli in the sputum. Until artificial auto-inoculation was begun no improvement took place. At the point marked on the chart a higher rise of temperature was produced by artificial auto-inoculation than had previously been recorded. At intervals further inoculations were given, and steady progress ensued. By the time this chart comes to an end the patient had lost all cough as well as all physical signs of active disease, and returned home with half a stone to her credit. For the time being the disease is arrested. Auto-inoculations are still being kept up. The absence of the broken curve of declining indices before interference is as marked as its presence afterwards. A continuous morning curve is introduced in this chart, to show the gradual lessening of the excursion between morning and evening temperatures as the patient improved.

PULMONARY TUBERCULOSIS

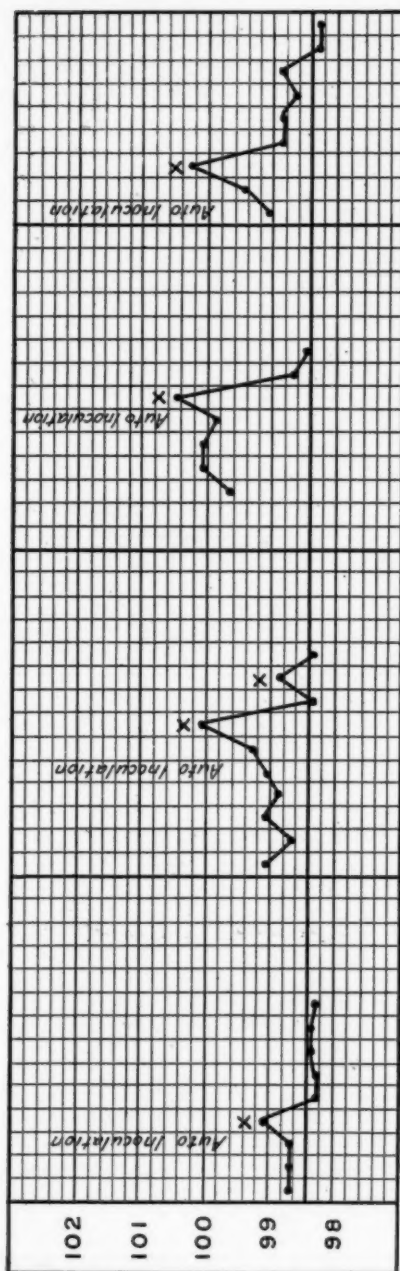


CHART 17.
Auto-inoculations. Correct dose.

PULMONARY TUBERCULOSIS.

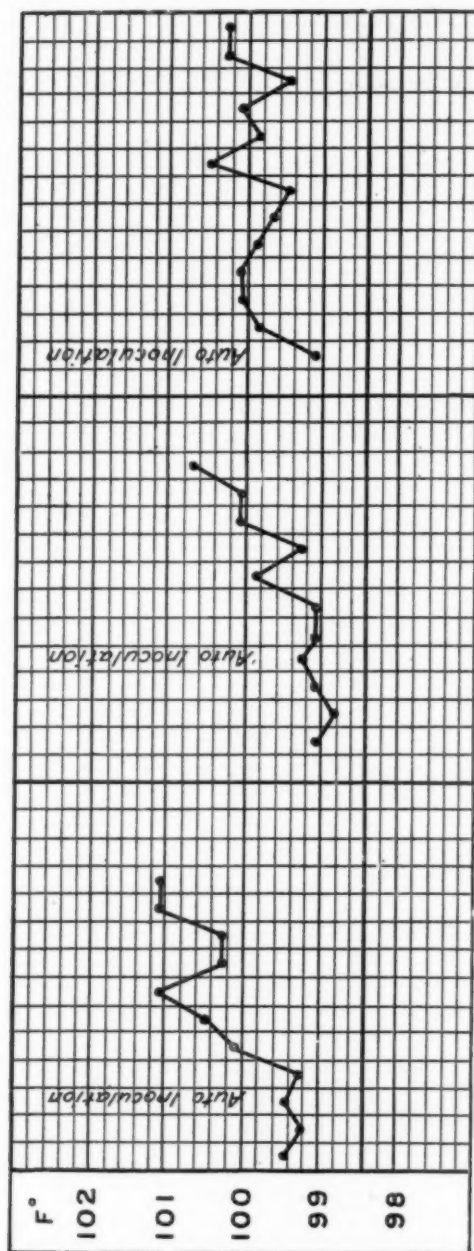
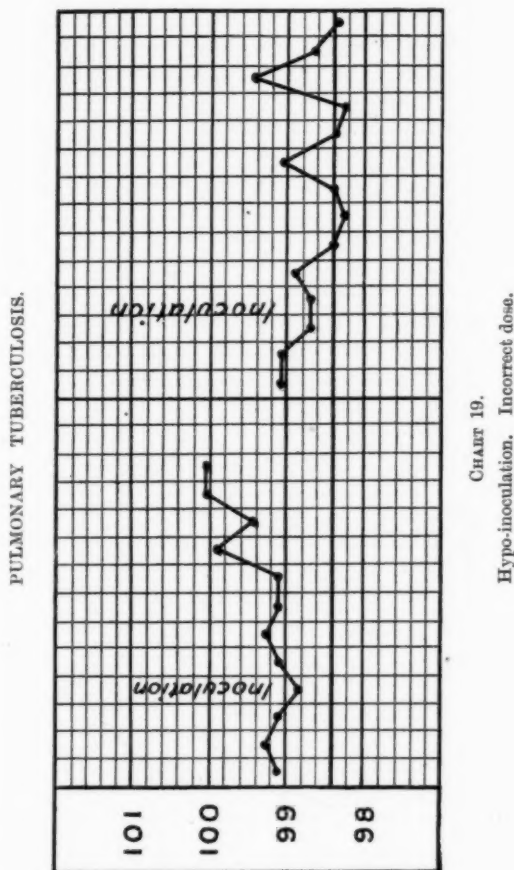
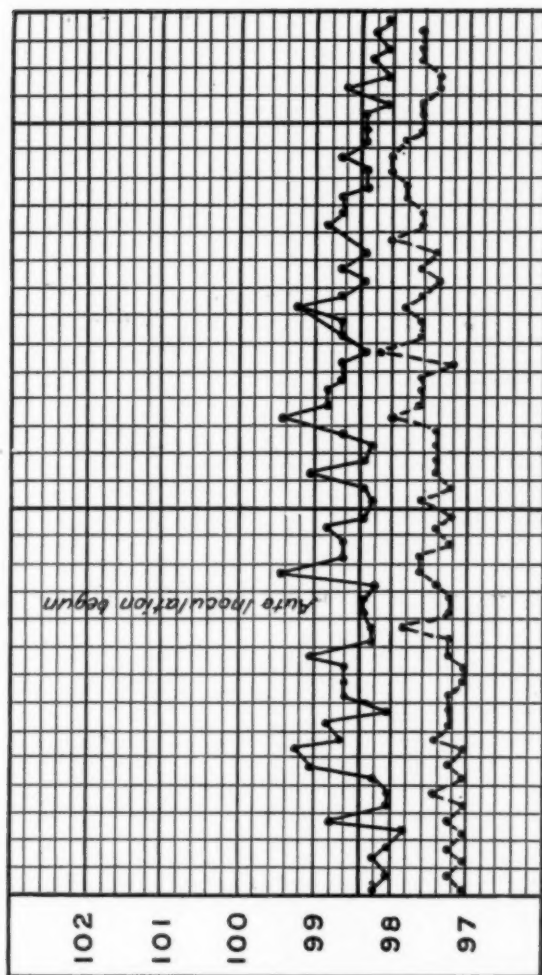


CHART 18.
Hyper-inoculation. Incorrect dose.

Chart 21 is that of a boy with physical signs in both lungs and bacilli in the sputum. In the first month of this chart he had been treated with tuberculin T.R., with apparently nothing but harm. For two months he rapidly lost ground, the disease extending to an alarming extent, especially in the left lung. After the correct dose of auto-



PULMONARY TUBERCULOSIS.



PULMONARY TUBERCULOSIS.

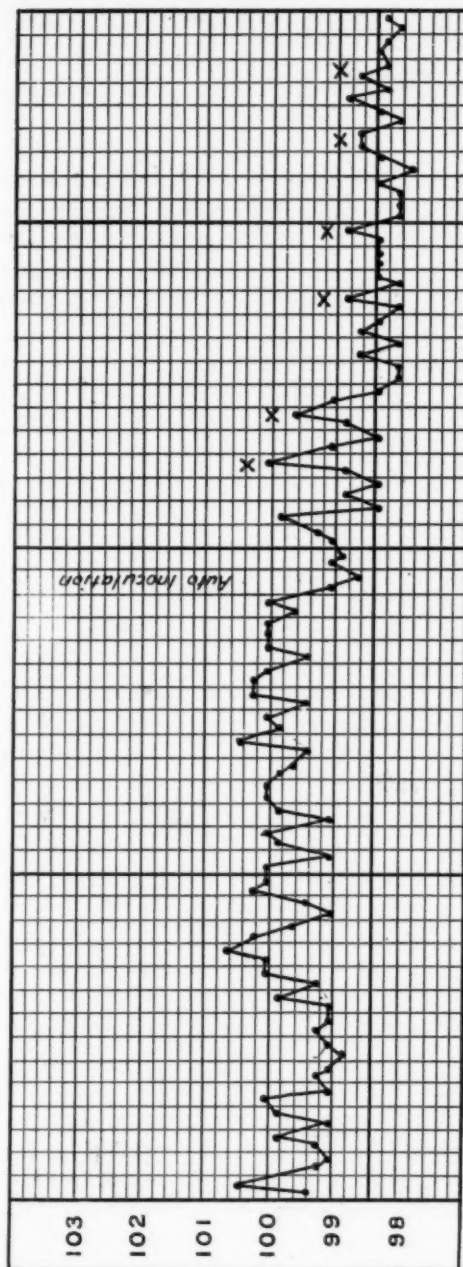


CHART 21.

UOFM

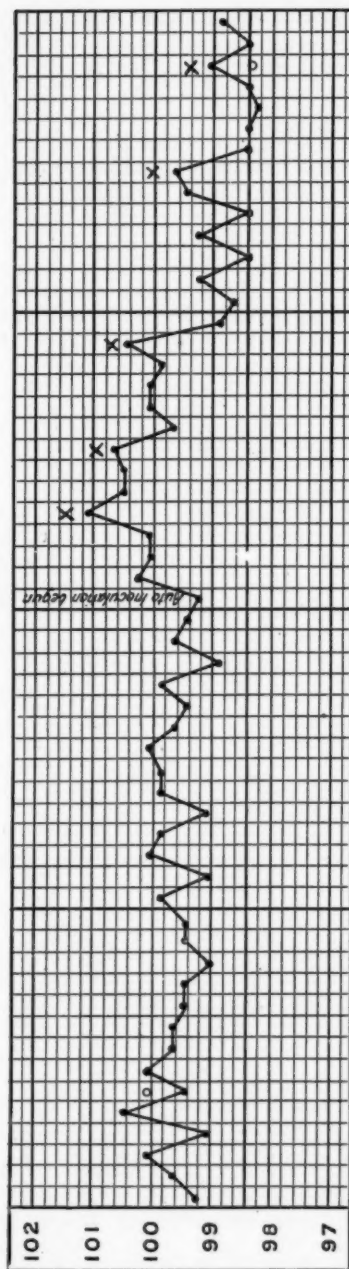
Chart 22 is that of a severe case of pulmonary tuberculosis. For two months the patient and her temperature chart were carefully watched without any form of specific treatment being given. During these two months she steadily lost ground in spite of absolute rest in bed. At the point marked in the chart auto-inoculation was at last carefully begun. Improvement had been as marked as before it was absent. The patient is still under treatment and there is good hope of eventual arrest. For three weeks after this chart was completed the temperature has improved still further. On two occasions she has in these three weeks been inoculated under the skin with $\frac{1}{500}$ mgr. tuberculin T.R. In both cases the injection has been absolutely ignored so far as clinical manifestations, including temperature, go, as I have frequently observed to happen where the temperature is at or near normal in cases approaching quiescence.

Chart 23 is worth showing to illustrate the effect of artificial auto-inoculation in a case of influenza complicated by a touch of pneumonia. At the time the artificial auto-inoculations were given the temperature was on each occasion rising. A further rise occurred, apparently as a result of the inoculation, followed in each case by fall. The curve of declining indices after the second inoculation is well shown. The physical signs in the lung rapidly cleared, and though convalescence was slow the ultimate result was good.

At this point I wish to summarize the remarks so far made by showing three curves, one of a patient doing well, a second of a patient barely holding his own, and a third of a patient getting progressively worse.

Chart 24 represents a curve moulded from the continuous evening temperature chart of a case of pulmonary tuberculosis that did extremely well without artificial specific aid. In this chart we note that at the point A there has occurred an intoxication followed by a rise of temperature. The intoxication is followed by an increase of antibody production. This has effectively neutralized the examination and a fall of temperature results, as seen in B—C. But the increased protective content called out by the intoxication has done more than merely neutralize it. It has also neutralized or diminished the activity of the area of the focus of the disease which I represent diagrammatically by the large circle A1. When, therefore, the excess production of antibody elicited by the intoxication has worn off, the influence of the toxic activity of the infective focus again reasserts itself, but it now proceeds from a smaller and less active focus. The result, therefore, is that the succeeding intoxication beginning at C produces a less rise of temperature than that beginning at A. The same phenomenon is repeated throughout the curve.

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BRONCHO-PNEUMONIA.

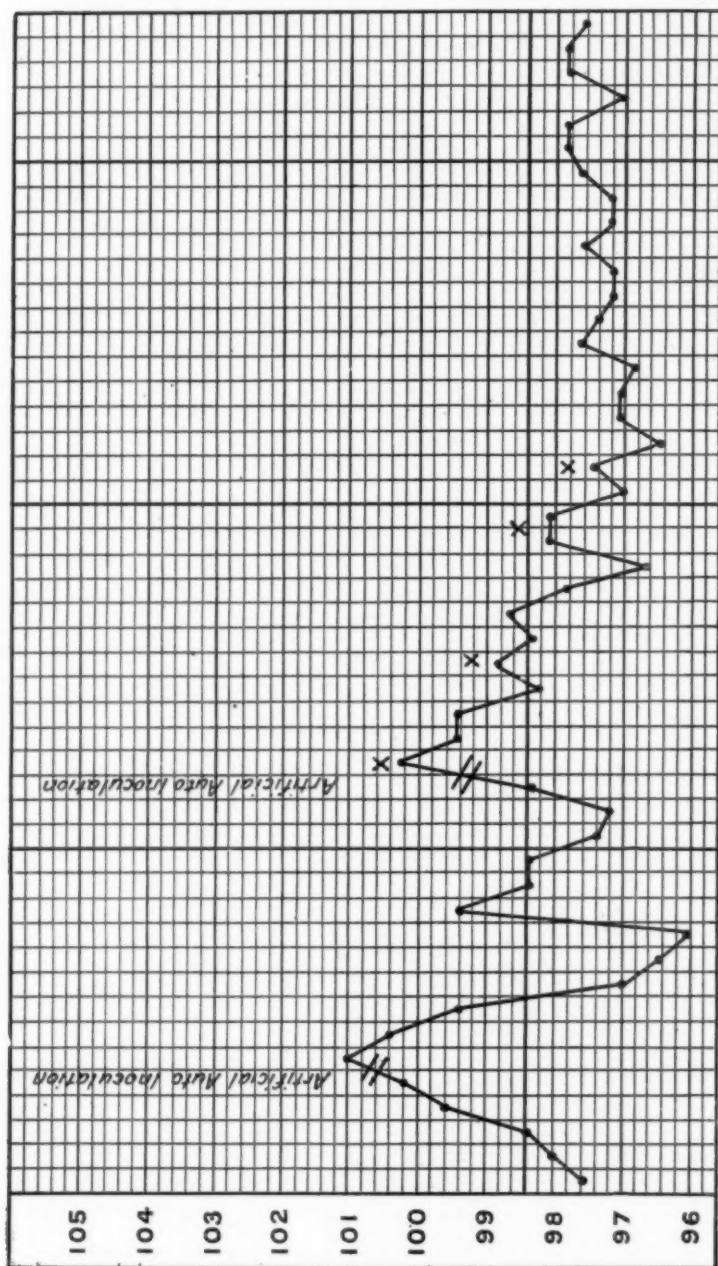


CHART 23.
Three-hourly chart.

PULMONARY TUBERCULOSIS.

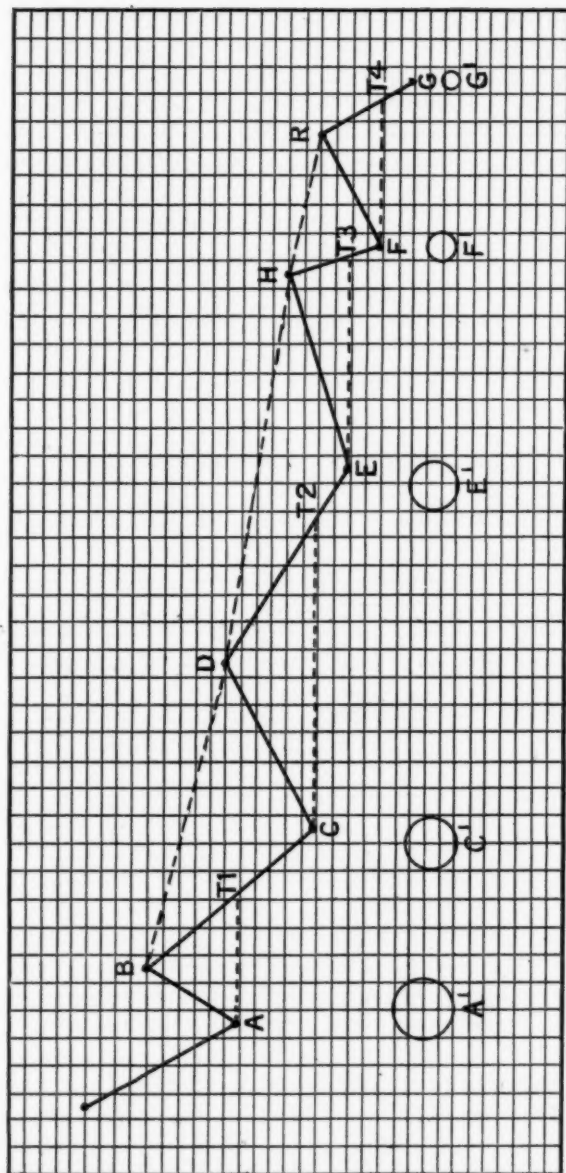


CHART 24.
Intoxication-response curve from continuous evening chart.

Chart 25 represents a similarly moulded curve from a case of pulmonary tuberculosis in which for many weeks no improvement took place in general condition or in physical signs. The disease, on the other hand, was not markedly advancing. In this chart we find no increase of antibody production sufficient, as in Chart 24, sensibly to affect the focus of disease. The only effect of the intoxication is to cause the temperature to rise at A, and when the influence of the antibody production thereby elicited has worn off the temperature returns to its own level. But, as we have seen, there is an insufficient amount of antibody sensibly to reduce the focus of infection. Hence this remains constant, and the temperature again rises to the same height as before. The patient, in fact, is at this stage at a standstill, and unless we can devise some method of eliciting a larger immunity response we cannot look for cure.

PULMONARY TUBERCULOSIS.

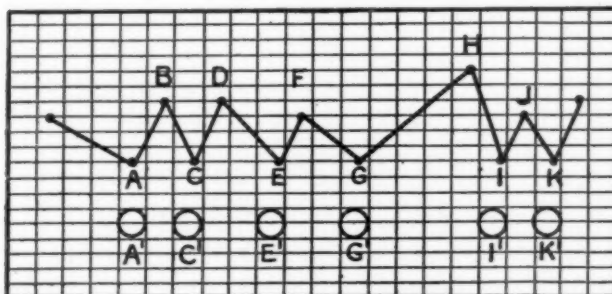


CHART 25.

Intoxication-response curve from continuous evening chart.

Chart 26 represents a curve from a case rapidly going from bad to worse. Here the immunity response has been less than in the two preceding cases. That is, the temperature has not been sensibly reduced in response to the intoxication at A. The focus of disease therefore proceeds to develop unchecked, which is graphically represented in the widening circles drawn. When, therefore, the slight excess of antibody production has worn off, the temperature rises higher than ever from unchecked activity of the disease focus expressed in acuter intoxication.

This patient died with a pre-mortem fall of temperature, whilst the patient to whom Chart 24 belongs recovered.

In these charts this extension, diminution, or constancy of area of disease is, of course, purely diagrammatic.

It is not claimed for Charts 24, 25, and 26, that they are anything more than three main types, or that such perfect curves as these will be generally found, if temperature charts are treated in the manner suggested. There are, of course, infinite gradations between the types, and in any one curve there may be from time to time one or more phases of one or other type. I suggest, however, that by careful study of a temperature chart treated in this way, not only in tuberculosis, but also in many other diseases, a predominant tendency to any one type may be of the utmost value in determining both prognosis and treatment.

Charts similar to Charts 1 to 26 may be found in the wards of any hospital, or in the office of any hospital registrar. Perfect examples of the broken curve of declining indices, such as shown, will, of course, only be found as a rule in cases that have been cured, or considerably improved, or at the time of examination are undergoing improvement. The exceptions have already been referred to. Neither, of course, will they be found in cases in which the focus of disease is neither increasing nor proceeding towards arrest, nor in those cases going downhill. Each of these classes have their own peculiar curves. Though the best examples are to be found in chronic tuberculosis, typical charts may often be seen in broncho-pneumonia, measles, smallpox, and many other infections. The very prevalence of the type argues strongly in favour of the interpretation suggested. In the acute infections it is necessary to examine three- or four-hourly charts, or otherwise, in a short infection, the characteristic features may be missed. When the experiments which are being conducted by Professor Gamgee to perfect his method of continuous registration of temperatures are completed, invaluable aid in reading a thermometric immunity curve may be expected to be forthcoming.

I will here summarize the points I wish to bring out from these charts by saying that estimations of the opsonic index in pyrexial cases, particularly of tuberculosis, are unnecessary, because the temperature will often reveal:—

(1) An overdose, too small a dose, or the correct dose, of an inoculation, whether spontaneous or artificial.

(2) That a patient effectively inoculating himself requires no artificial specific treatment, and is in fact better without it, because response is already larger than intoxication.

(3) That a patient in whom intoxication is in excess of response must be severely left alone unless you can demonstrate by a trial inoculation that increased stimulus is capable of inducing increased response.

(4) That a patient in whom intoxication and response are fairly balanced is the ideal subject for artificial auto-inoculation, so long as you carefully gauge its effects, not by isolated phases, but as far as possible by continuous curves.

If it be true that examination of temperature charts in the manner described can give reliable information as to presence or absence of response to inoculative stimuli it will be clear:—

(1) That when taken as a whole the auto-inoculative stimuli, though apparently a succession of isolated inoculations, are in reality potentially continuous. If it were not so the restraining influence of the antibodies, called out by intoxication, would be overcome at some point other than it actually is. In other words, the temperature would at such point rise instead of fall.

(2) That pyrexial cases progressing towards cure afford in many instances an inimitable model for directing our treatment in cases not so progressing.

(3) That cases exhibiting effective response to spontaneous auto-inoculation are not necessarily in need of extraneous specific treatment, whether by tuberculin or by auto-inoculation. Further, that such interference may result in the utmost harm.

(4) That in cases that are obviously going downhill the danger of extraneous artificial treatment is infinitely great, and may only precipitate disaster. In such cases, if the exhibition of trial doses of tuberculin or of auto-inoculation does not rapidly transform the dangerous type of curve into the type of declining indices, further specific interference ought to be absolutely prohibited.

(5) That cases doing neither well nor ill are pre-eminently the ones that call for artificial specific aid. In such cases it is a matter of great difficulty to know at what point to insert an inoculative dose. Examination of isolated phases is useless. Even if the results of blood examinations were to be absolutely relied on they can, in pyrexial cases, be of little value in determining at what point to inoculate unless a curve be plotted out of observations extending over several weeks in such a chronic disease as pulmonary tuberculosis. The possibilities of a thermometric immunity chart may be gauged by the following curve, which is moulded on the temperature chart of a man who had been under observation for many weeks. As Chart 27 shows, he had barely held his own from

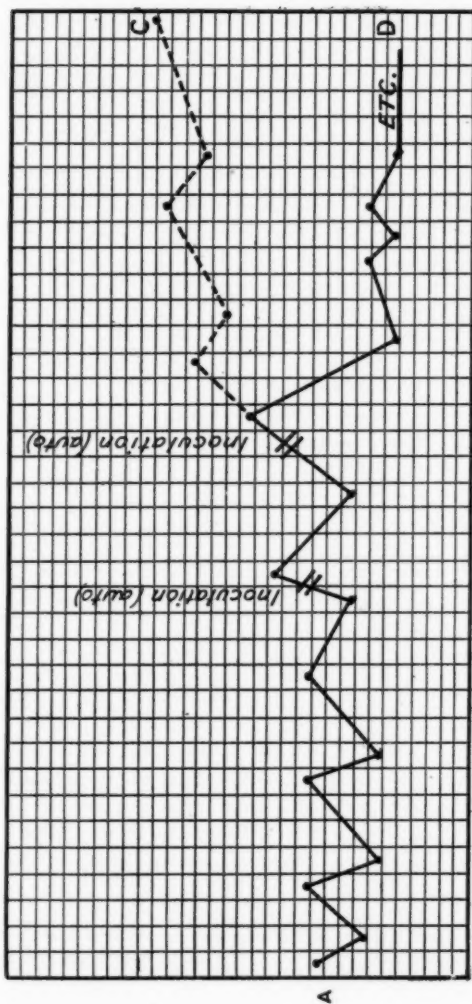
A to B. Clinically, this was fully borne out in every respect. It was obvious that he could not go on indefinitely in the same way. He was bound to get either better or worse. At the points T^1 , T^2 , therefore, I inserted two artificial auto-inoculations by inducing, as in other cases referred to, artificial hyperæmia of the lungs by making him expire for fifteen minutes through a special form of spirometer adapted for the purpose. The result is admirably reflected in the curve and in the great improvement that ensued. It has since been maintained. The point, however, on which special stress should be laid is this: If the auto-inoculation had not been inserted at the correct phase he might well have been precipitated into the curve represented by the dotted line B—C. The main object of this chart is, therefore, to show that if the inoculative stimuli T^1 , T^2 , had not been followed by the curve B—D, to continue with inoculations at any other point than the one selected would have most probably been useless or disastrous. It appears, in fact, if these curves can be taken as a guide, that to insert a stimulus in the response-phase can do no good, though as to this there is not sufficient evidence as yet to make such an assertion definite.

(6) That in a case, as in Chart 24, that exhibits the declining curve of broken indices and is, therefore, as a result of effective spontaneous auto-inoculation, apparently doing well, such terms as negative and positive phases cannot be applied. The reason for this is that according to such curves each response is an actual positive phase, and each succeeding intoxication a potentially positive phase, as may be seen from its achievement, and therefore only relatively negative.

GENERAL CONSIDERATIONS.

What applies in the preceding and following remarks to auto-inoculation applies also *mutatis mutanda* to hetero-inoculation. To get a correct idea of the amount of response called out by any given inoculative measure, examination of isolated phases in a temperature or inoculation chart is of no value. Interpretation of any phase is only possible in the light of many previous and subsequent phases. It is most important that a naturally broken curve of declining indices denoting effective response to spontaneous auto-inoculations should not be mistaken for the results of artificial inoculation. What is often regarded as an instance of correct inoculation is frequently in reality one of hypo-inoculation or no inoculation at all. What is sometimes regarded as an instance of hyper-inoculation may actually be one of correct inoculation. The

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T¹ T² B
CHART 27.

appearance after inoculation of transient clinical signs, such as headache, malaise, &c., is not necessarily indicative of an excessive dose. When such occur in conjunction with fever, they may be merely the necessary accompaniments of an intoxication essential to effective response. The absence of fever does not argue the inverse. A patient may very well be inoculating himself to good purpose, especially in apyrexial cases, without any change being observable, either clinically or by blood examination. The absence of response as reflected in the blood-stream is said by the opsonists to denote absence of inoculative stimuli. This dictum must be taken with reserve, especially if there is amongst opsonists wide divergence of opinion as to what the opsonic reading of any given serum actually is. In cases of pulmonary tuberculosis doing so well as to justify the view that the focus of disease is relatively quiescent, it can obviously not be possible to exclude the continuance of spontaneous inoculations if improvement in what physical signs remain is observed. In such cases, in fact, there must be auto-inoculations going on, in spite of the absence of demonstrable blood-change. It is frequently stated that a rise to 99° F. after manual labour or other exercise indicates hyper-auto-inoculation, particularly if the rise be maintained for an hour or more. This statement must be received with caution, as it may mean nothing of the kind. On the contrary, some of the cases of pulmonary tuberculosis which do best are those which for weeks have had high but gradually declining fever as a result of their own auto-inoculations. This point is well shown in Charts 1 to 5. The benefits of artificial auto-inoculation are, in fact, deliberately confined by some observers to those patients whose temperature is practically normal or even subnormal. In other words, to many of those who are effectively inoculating themselves without artificial specific aid. Many such cases require no artificial inoculative treatment. The majority of cases of pulmonary tuberculosis without fever do well. There is no difficulty in detecting the minority. Not every rise above 99° F. represents an auto-inoculation. Many intoxications produce a fall in temperature.

It is said that cases of pulmonary tuberculosis in which arrest has occurred are more liable to relapse if they are not then treated with tuberculin than if they are so treated. This may be true, but it is not therefore necessary to submit such cases to treatment with artificial tuberculin. Those whose fortunate experience it has been to watch many patients partially immunize themselves without artificial specific aid, and after return to active life apparently completely immunize themselves

against relapse or reinfection by the sole use of their own tuberculin and cell-contents, will agree that hetero-inoculation is in such cases an uncalled-for procedure. The argument advanced in favour of the use of artificial tuberculin in such cases is based on a fallacy. The argument is that in a focus nearly or relatively quiescent auto-inoculations are no longer incitable, and that response must be elicited, therefore, by artificial tuberculin. The fallacy is that, if a lesion be so insulated that auto-inoculation is no longer possible, response to tuberculin administration cannot possibly be conveyed, without further procedures, to such lesion. The truth appears to be that until a focus is absolutely insulated, and therefore quiescent, auto-inoculations may still be induced. So long as further progress towards arrest is feasible, as evinced in improvement of physical signs, so long is response not only possible but probable. There comes a time, of course, in which ordinary auto-inoculations cannot be induced, though unusual effort may incite demonstrable stimulus and response. As Paterson has well pointed out, the ideal of attainment is the point at which no reasonable labour will provoke inoculation.

ADVANTAGES OF ARTIFICIAL AUTO-INOCULATION.

The merits of artificial auto-inoculation in the treatment of established infection may be thus summarized :—

- (1) It is based on a natural model of spontaneous auto-inoculation which appears to be responsible for the cure of the vast majority of all cases of infection.
- (2) It incites the production of cellulo-tropic bodies, as well as of bacterio-tropic.
- (3) It incites the production of antagonizing bodies to the toxic products of morbid cell-changes.
- (4) It incites the production of bodies specific to the particular strain of organism responsible for any given case of infection. For instance, in the case of pulmonary tuberculosis there may be many strains of Koch's bacillus, and therefore a standardized emulsion of new tuberculin or other preparation is not necessarily the correct form to use.
- (5) It incites the production of bodies specific to the particular form of cell-enzyme concerned in any given case of infection. Inasmuch as the difficulties of isolation of enzymes and cells for preparation of cellular vaccin are extreme, all such difficulties are obviated by the use of natural vaccins.
- (6) If there be a real distinction between bovine and human sources

of tuberculosis, the use of artificial auto-inoculation removes both the necessity for establishing the distinction and for meeting the increased difficulties of treatment.

(7) Whenever mixed infections are present it allows for the opportunity of the exhibition of specific antagonizing bodies to each and all of the various organisms concerned.

(8) It is free from the danger of adding fresh toxins to those already existing.

(9) It involves no danger of accidental sepsis as with inoculation with artificial vaccins.

(10) It entirely meets the natural and often strong aversion exhibited by the public to artificial inoculative procedures.

(11) It is simple because it obviates the necessity of determining all the organisms concerned in any infection.

It is cheap, because the costly manufacture of often multiple vaccins is avoided.

(12) In co-ordinates all the vast mass of work that has been done in the field of serum and vaccine therapy, and its guiding principle is the same as in methods that have received the sanction of extended trial, though its rationale has not been fully appreciated. Since the practice of hetero-inoculation has become extensive, approved methods of general treatment are in some danger of being deliberately discarded. Auto-inoculation reveals the value of them all, mainly by reason of the attention it calls to the supreme importance of general methods of improving cell-nutrition and cell-restraint.

(13) It renders the difficult problem of dosage incomparably easier than when such agents are laboratory products.

This last statement might be regarded as a paradox, which is, however, more apparent than real. Exponents of the method of inoculation with dead bacilli maintain that by inserting a measured dose of so many million organisms it is possible to get a correct idea of response. Up to a certain point this is no doubt true in infections that are strictly localized and insulated. But whenever artificial vaccins are injected into pyrexial cases, whatever the infection, the case is very different. For in these there is a continuous or intermittent stream of tropins from the infecting bacteria and the cells they have damaged. To add a measured dose of bacteria to an unmeasured dose of bacteria and toxin and an unmeasured dose of cellular toxin, and then expect to get back even a rough response to the whole triad of infection, is surely to place expectation high. If, however, it be true, as I submit it may be

true, that in pyrexial cases inoculation can be more or less accurately measured by a temperature chart, the measurement of response observed records the sum-total of anti-tropin incited both by damaged cells and their products, and by bacteria.

Such, then, are my reasons for maintaining that artificial auto-inoculation is the correct method of treatment to adopt in pyrexial cases of tuberculosis, and that it can be satisfactorily controlled by thermometric readings if interpreted in the manner described. In cases, however, in which fever is slight or altogether absent, some other guide is needed. Such guide appears to be available in the antitryptic index, by which is meant the ratio of variation of the antitrypsin content of pathological serums to the normal content of healthy serums. The subject of the antitryptic index is at present in a developmental stage, and I can therefore only refer to it very briefly. The experimental results obtained by Dr. Golla in determining variations in the index in tuberculosis, and of his method of estimation, are about to appear, and you will therefore be able to judge of their value. In the meantime, he kindly allows me to give some of his results of examination of serums of my own cases, in so far as they bear on my views on the importance of recognizing the intracellular element in infection, on the information to be gained from temperature charts, and on the value of artificial auto-inoculation.

The history of the gradual recognition of the importance of determining the antitrypsin content of pathological serums is briefly as follows :—

Quite early in the work on intrinsic tissue-change note was taken of what is now a well-established fact. The blood-serum of man, monkeys, sheep, horses, and other animals was found to exhibit in marked degree a varying resistance to the action of autolytic enzymes. This resistance was found to disappear on heating, prolonged standing, and on the addition of dilute acids. The rate of autolysis *in vitro* was found to be markedly lessened if the autolysing organ was placed in fresh unheated horse-serum. The inhibitory body on which this resistant and protective body depends is effective against trypsin, and was therefore called antitrypsin. It is also effective against pepsin and other secreted enzymes, and also against all tissue enzymes, except against an acid solution of HCl pepsin, which destroys it. According to von Eisler it is not specific in the sense of being more specific to auto-enzymes than to hetero-enzymes. Apparently it is less so. It seems to act not by destroying enzymes, but by inhibiting or paralysing their action.

Weinland's observation of the protective power of antitrypsin secreted by certain intestinal worms, as demonstrated by the absence of their digestion in active secretion, is familiar. Delezenne holds that the antitryptic action of normal serum is due to antikinase, a view contended by Bayliss and Starling, who maintain the direction action of antitrypsin on the activated body. Cathcart declares that the antitrypsin of normal serum is attached to the albumin-fraction of the serum, from which it cannot be detached even by prolonged washing, and not to the globulins. The antitryptic content of normal serum can, as I have elsewhere shown, be clinically demonstrated. If normal horse-serum, for example, be added to wounds or ulcers bathed in the strongly tryptic fluid of their own infected secretions, the tryptic action of the fluid is inhibited to such an extent that a wound treated in this way rapidly cleans. In favourable cases this is followed by rapid healing. By stripping one sample of normal serum of its globulins and adding to it the serum albumin content of another sample, it was found possible so to raise its total antitrypsin content that the rise could be demonstrated both hæmatologically and clinically. This, in fact, is the basis of preparation of the so-called antilytic serum which is now being extensively used in the treatment of gastric and duodenal ulcers.

In connexion with the antitryptic content of normal serum, the following observations are of interest. Fluctuation, and therefore the site of the most advanced softening in an abscess, is found in the centre of an abscess only—*i.e.*, furthest away from the restraining influence of the antitryptic lymph-stream. The antitryptic content of lymph from a recent healthy wound is high; that of lymph from a wound showing no tendency to heal is, even in the absence of infection, low. An infarct undergoes central autolytic softening sooner than a peripheral, no doubt for the same reason. In old exudates it is also less than in recent ones, and hence autolysis of the former proceeds more rapidly than does that of the latter. In lobar pneumonia is seen an excellent example of unrestrained autolysis. The contents of the alveoli, being removed from the inhibitory action of the lymph-stream, undergo rapid autolysis, whilst the cells of the alveoli, being constantly bathed in lymph with a high antitryptic content, escape.

The first observers to clearly demonstrate the antitryptic action of normal serum appear to have been Camus and Gley in the year 1900. The first serious attempt to measure this action was made by Professor George Dean, who in 1901 published his paper on

"Immunity in Relation to the Pancreas and its Ferments." The subject was extensively followed up in 1902 and 1904 by Glaessner, Cathcart, Levene, Stookey, Opie, and others. Professor Dean's method was to estimate the rate of digestion by trypsin of measured quantities of albumin, and the degree of inhibition of digestion by normal serum. He then adopted a method suggested by Fermi of estimating the inhibitory power of normal serum of digestion by trypsin of measured columns of gelatin. This method, with substitution of egg albumin for gelatin, now known as the method of Mette, was, as may be seen by reference to Dean's paper, independently elaborated by the latter. Methods of estimating the inhibition of tryptic digestion of albumin and gelatin in this manner are, however, of little practical value.

Various attempts were then made to estimate the antitryptic power of the blood-serum in disease, though until 1908 without any considerable measure of success.

Most of the work previous to that date had been done by the method of Müller and Jochmann, which consists in the addition of varying quantities of serum, measured in loops of platinum wire, to a drop of pus in a Löffler plate. The appearance or non-appearance of pitting after incubation was taken as a sign that the amount of inhibitory serum added had been insufficient to neutralize the pus-ferment present. Such a method of estimation is obviously far too coarse to afford even an approximately accurate criterion of the antitryptic content of normal serum, and the earlier work of Hedin had already shown that complete neutralization could never be obtained. The contradictory results of the work of the authors of this method, and their followers, have in fact shown that it cannot be accorded serious consideration. The evaluation of ferment activity and, reciprocally, of the inhibitory power of serum can only be determined by some method which allows the velocity reaction of the ferment to be measured. Hence the results obtained by the serum-plate method, or by the end-point methods of Cross, Bergmann and Meyer, are both contradictory and inaccurate. Estimation of the incoagulable nitrogen formed at different points in a tryptic digestion, and through this of the antitryptic content of an inhibiting serum, is impracticable because of the amount of serum required. For these reasons, as Dr. Golla has just shown, this observer has adopted the method of electro-conductivity of Henri, which has been recently elaborated by Bayliss. By this method Dr. Golla states that he is able to gauge the relative antitryptic power of

the blood with great accuracy, and in the course of his investigations he has discovered :—

(1) That a rise of this antitryptic activity frequently takes place in tuberculosis.

(2) That such rise is often associated with a bad prognosis.

(3) That hetero-inoculation with tuberculin induces a rise of antitryptic activity in tuberculous subjects.

(4) That accurate determination of such rise affords a reliable guide to the efficacy of tuberculin injection, and that by this method it is possible to determine the point at which a dose of tuberculin should be exhibited.

In twenty of my own cases of tuberculosis a rise of the antitryptic index was found in all but one. The majority of these were pulmonary; the remainder, six in all, were renal, glandular and peritoneal.

Of these twenty cases the only two that died had persistently high indices. Six, when injected with tuberculin, showed a further rise in an index already raised. In one of the cases referred to active disease was associated with a normal index. He subsequently did very well.

In cases where the guide afforded by a temperature chart, on the lines I have referred to, is absent, either from too small a variation of temperature or entire absence of such, it appears :—

(a) That tuberculous patients with a high index react little, if at all, to artificial inoculation.

(b) That tuberculous patients with a low index show marked rise after similar stimulus.

A high index such as occurs in some cases with high fever, conforming to the type shown in Chart 26, is often associated with a bad prognosis. An index which was high when the temperature was raised in two cases approached, under artificial auto-inoculation, the normal, when marked clinical improvement set in, and became actually normal when the temperature remained undisturbed and when the disease was judged to be arrested. In one case the index did not again rise after tuberculin injection. In another case, with low index, response to inoculation was marked, and as the patient improved the response tended to diminish. In a third, who got progressively worse, the index was very high and tended to rise, whilst the response to inoculation was only comparatively slight, as evidenced by further rise of the index.

These observations, though at present incomplete, tend in some

measure to confirm some of the views I have propounded, based on the variations in temperature that I have shown to follow auto-inoculation. In particular they support the contention that artificial inoculation of all kinds is contra-indicated in cases where intoxication without adequate response is great.

Apart from tuberculosis my cases show that variation can be demonstrated in such diverse conditions as carcinoma of breast, tongue, mouth, and cervix, rodent ulcer, acute septicæmia, leprosy and suppurative nephritis. Variation in the antitryptic content of the serum was found in twenty cases of tuberculosis of various organs, in four cases of syphilis, in two cases of rodent ulcer, in seven cases of carcinoma, in one case of acute septicæmia, in one case of suppurative nephritis, and in one severe case of leprosy. These serums were from cases in my own clinique or from such hospital cases as members of various hospitals were good enough to place at my disposal. The majority of the serum examinations were kindly undertaken for me by Dr. Golla.

It is also claimed by Brieger and Trebing that they have been able to demonstrate variations in the index in cases of carcinoma, pernicious anæmia and nephritis. Variation in the antitryptic content of the serum in a case of lobar pneumonia was also demonstrated in 1903 by Ascoli and Bezzola.

In view of these facts it appears that marked variation from the normal may be as much a response to cellular changes in no way associated with bacterial invasion as to changes directly connected with such, as well as to bacterial infection without extensive cell-change. That such aberrations of tissue metabolism as the index appears to indicate can be reflected in the blood-stream so as to be capable of ready measurement must be of profound significance.

As regards the interpretation to be placed on variation in the Antitryptic Index in disease little at present can be said.

For the purposes of this paper I am content for the moment to submit that its estimation cannot but have the highest value as a gauge of immunity response, because it appears to reflect, as I have shown, presence or absence of such response not only to bacteria and their products, but also to the factors of morbid enzymic activity and the toxic products of perverted cell-change.

If such submission be sound blood examinations exclusively directed to measurement of phagocytic activity and bacterial engulfment, however induced, can clearly, even if reliable, afford only a relatively insignificant gauge of immunity response as a whole.

To conclude as I began, I submit :—

(1) That when Nature cures infection she converts tissues and bacteria into auto-inoculating agents, and thereby incites both cellular and bacterial restraint.

(2) That to provoke the last and ignore the first is too often to aim at half and expect the whole.

(3) That, whenever practicable, auto-inoculation is the best method to employ when artificial aid is needed, because, being the natural method, it considers all the factors concerned, and

(4) That in thermometric charts and in measurements of the blood's inhibitory powers we have most useful gauges of presence or absence of many kinds of stimulus and response.

Full conviction I can hardly hope to carry on the fragmentary evidence here produced, but the attempt will not be fruitless if the attention of makers of modern Medicine be for a moment diverted to issues other than the merely bacterial.

I wish to express my thanks to the following gentlemen, whose courtesy has enabled me to collect from their respective hospitals much of the clinical material that forms the groundwork of this paper: Mr. J. E. Lane, at the Lock Hospital; Sir Hugh Beevor, at the Victoria Park Chest Hospital; Mr. C. H. Leaf, at the Cancer Hospital; Sir Malcolm Morris, at the Seamen's Hospital; Mr. Lenthal Cheatle, at King's College Hospital; and Dr. Daman, at the Lincoln Hospital. I also wish to express my indebtedness to Dr. Rolleston and to Mr. Cheatle for their great assistance in preparing this manuscript, and to Dr. Venning for continuous help in collection of the various serums referred to, and in many other ways, and in particular to Dr. Golla, for his unfailing and generous help. Finally, I wish to convey my deep obligation to the anonymous philanthropist whose single-minded devotion to the highest aims of medical research has made this preliminary inquiry possible.

DISCUSSION.

Dr. D. LAWSON (Nordrach-on-Dee) said if there was one indictment which the man in the street had brought against the medical profession which members of that profession found it difficult to answer, it was the charge that they were liable to be carried away by one idea, and to be blinded to all others which bore on the central point. There were many examples of that in the history of the treatment of tuberculosis in the last twenty-one years. There was the era of Koch, with his tuberculin, and in very recent times that of Wright and his opsonic index. They had been invited by Wright to discard their clinical guides; the surgeon was asked to put on dark spectacles, because he need not now look at the appearance of a wound, since he had, in the opsonic index, a sufficient guide for everything. Within the last six months several papers had appeared in which medical men were advised to relinquish the opsonic index, substituting for it as a guide the temperature chart. It was in regard to the elucidation of this point referred to in Dr. Hort's paper that he would like to utter a word of caution in reference to accepting temperature records as an indication of the protection which had been afforded to the body against attack. Three years ago, in a paper read in that room, thirteen instances were brought forward by Dr. Stewart and the speaker to show that in cases where the temperature had been taken four-hourly, and the opsonic index taken every day, there was no direct relationship between the two. Little had been heard of that since, until the appearance of Dr. Latham's recent paper. In this he brought forward certain evidence which seemed to point to the temperature and opsonic curves bearing an inverse relationship to each other. Dr. Hort seemed to accept Dr. Latham's view on that point, and therefore he (Dr. Lawson) wished to ask if the author could explain three or four clinical facts which seemed to have a bearing upon the new view. One found a certain type of tuberculosis which came into a sanatorium. Pulse and temperature were carefully taken, and often the temperature was found to be normal, but the pulse-rate might be anything between 90 and 120, extending over a period of weeks. That type of case, of which he had seen many in the last nine years, invariably went wrong, and if the pulse-rate was not restored nearer to the normal the patient died. In such a case the pulse appeared to be the guide, not the temperature, as to the advance of the intoxication. Secondly, there was the chronic fibroid type, in which the temperature was subnormal, in which there might be a rapid pulse; there, again, the pulse was some guide, but elevation of temperature was no guide. Thirdly, it had been put forward that a temperature reacting to exercise was presumptive evidence of the presence of tuberculous disease. If one set such a chronic fibroid case to walk half a mile it would cause a fall and not a rise in the temperature, and yet everything showed there was increased intoxication in that body. No rise of temperature took place to show that the supposed change had taken place. Living, as he

had been, with cases, and taking four-hourly temperatures every day, he was convinced that the temperature in itself was not a reliable guide as to the advance or retrogression of a lesion. And he wished to utter his protest against the trend in recent days to exalt temperature in cases of tuberculosis to the position of a fetish. He was sure that in a few years it would have slipped back to the position which it occupied before, and that clinicians would again place their faith in the broad outlines of clinical facts observed in tuberculosis as their guide to treatment.

Dr. F. PARKES WEBER said that amongst the habits which were supposed to exert a prophylactic action by increasing the resistance of the organism towards tuberculosis (and towards some other diseases), one of the most generally admitted was the taking of muscular and respiratory exercise as a regular daily habit, especially open-air exercise (walking, climbing, &c.). Exercise helped to keep persons healthy and to maintain their resistance towards tuberculosis, but in spite of open-air exercise and a usually hygienic mode of living it sometimes happened (as post-mortem examinations showed) that (apparently) healthy persons were in reality affected by quiescent (latent) tuberculosis. In such cases, doubtless, the repeated auto-inoculation brought about by daily muscular exercise had the effect of further increasing the resistance of the body and thus of preventing enlargement of the tuberculous focus, and likewise in many cases of gradually transforming a quiescent focus into an obsolete one. In other words, exercise (in moderation) not only rendered the body in healthy persons more resistant towards infection, but it likewise in the early (latent) stages of chronic pulmonary tuberculosis acted as a powerful agent in arresting the insidious progress of the disease and in producing obsolescence of tuberculous foci before such foci became recognizable by ordinary methods of clinical examination.

Medical Section.

April 27, 1909.

Dr. T. HENRY GREEN, Vice-President of the Section, in the Chair.

Clinical Observations on the Influence of the Vessel Wall on (so-called) Arterial Blood-pressure Readings.

By O. K. WILLIAMSON, M.D.

THE results deduced from instrumental blood-pressure readings, more especially those in which the method of circular compression of the arm is adopted, have claimed a prominent place in the medical literature of the past few years. It has become, therefore, a matter of no inconsiderable importance that we should seek to determine once and for all whether this method really records the true blood-pressure, or, on the other hand, whether the resistance due to the arterial wall of that part of the limb which is compressed is more than a negligible factor and has therefore to be reckoned with.

In this method of circular compression a rubber bag encircling the limb and encased in a cuff of some strong material is inflated with air so as to obliterate the pulse in the distal portion of the limb. The pressure (as registered by a manometer connected with the interior of the bag) at which the pulse reappears is noted. This is taken to be the systolic arterial blood-pressure. The diastolic pressure is considered to be the reading of the lowest point of maximum oscillation of the index of the manometer on still further reducing the pressure. The method of circular compression is due to Leonard Hill and Harold Barnard and to Riva-Rocci. It is the systolic pressure as measured in this manner which is usually believed to yield the most accurate results, and is accordingly in general use. It is, therefore, this method of measuring systolic pressure which comes under consideration in this

paper. Obviously there are three factors which may influence the blood-pressure reading, viz. :—

- (1) The tissues superficial to the artery which is compressed.
- (2) The arterial wall.
- (3) The blood-pressure within the artery.

The influence of the tissues superficial to the artery may be dismissed in a few words, for von Recklinghausen has shown conclusively that with a sufficiently broad cuff (12 cm.) the diameter of the limb compressed (within the limits ordinarily met with) does not affect the reading. His results have been substantiated and are generally accepted [4].

As regards the second factor, however, it must be admitted that nearly all the skilled observers who have investigated in this branch of clinical medicine have until recently somewhat unaccountably assumed that the resistance due to the arterial wall is a negligible factor. This is perhaps largely the result of some post-mortem observations of von Basch, who showed that the pressure required to close the normal empty radial artery scarcely amounts to 1 mm., and even for sclerotic arteries is not much above 5 mm. [1]. Indeed so prominent an authority as Janeway says "that a sclerotic vessel may offer considerable resistance to compression is a common belief which I do not think is justified" [2].

Dr. William Russell was, I believe, the first observer to bring forward any reasons in favour of the arterial wall (especially as the result of hypertonic contraction) exerting an important influence on blood-pressure readings, and the arguments he adduces, although not of the nature of proof, are most suggestive; and indeed there can be no doubt that the important and illuminating work of this physician has not received the attention which it deserves. He points out that "it is a matter of common knowledge that the compressibility of a tube depends upon the thickness of its wall, and the relation between that and the size of its lumen" [5]. By means of an artificial schema of the circulation in which the arteries were represented by rubber tubes Russell showed that a very definite amount of pressure beyond that necessary to overcome the pressure of the contained liquid was necessary to obliterate the lumen of such a tube [6]. He arrives at the conclusion that "the two factors in the determination of arterial pressure or compressibility, as measured by the instruments in use, are (1) blood-pressure, and (2) the thickness of the wall and the proportion it bears to the lumen" [7]. Dr. George Oliver inclines to the same view, for he states: "My

observations have demonstrated to me that the thickened arterial wall in arterio-sclerosis has a greater effect in disturbing the accuracy of the readings of the arterial pressure obtained by the armlet method than is usually supposed" [3]. He arrived at this conclusion partly owing to discrepancies in the readings in the arm and forearm in cases of arterio-sclerosis, and in part from the fact that in these cases the hæmodynamometer and armlet method yield different readings of systolic pressure.

At a recent meeting of the Section Drs. Herringham and Womack read a paper on "Experiments on the Value of the Sphygmomanometer as a Test of the Blood-pressure," the experiments being made on various arteries removed from dead bodies. They concluded that "when using the sphygmomanometer we must bear in mind that the resistance of the wall of the brachial artery may vary from 4 mm. to 34 mm. Hg.; that the readings of the instrument represent the sum of the blood-pressure, together with the resistance of the artery, and that we have as yet no clue which will enable us to analyse this total into its two component parts." It should be obvious, however, that the results of observations on post-mortem arteries cannot be conclusive, for the matter is not less a vital than a physical one.

If high blood-pressure be a cause of arterio-sclerosis we should expect to find that in cases of this condition the arteries of the lower extremities, which are, owing to the influence of gravity, subjected to a far higher pressure than those of the arms, would present more advanced changes than those of the arms. Indeed, Dr. Savill has found that this is the case. He states that "the media of the arteries of the lower extremity were nearly always more hypertrophied (or presented a more advanced stage of degeneration) than those of the upper extremities. . . . The major distribution in the lower extremities suggests the explanation that gravity throws more strain upon these latter" [8].

In the following investigation clinical observations by the method of circular compression were made on the leg and arm of the same patient in a number of cases of high blood-pressure.¹ In all of these cases in which the arteries of the leg and foot could be felt these were found to

¹ It might be asked what is the evidence of high pressure in these cases, granting the conclusion arrived at in the present paper, viz., that the instrumental readings are vitiated by alterations in the arterial wall. I think, however, we may take it as certain that in cases such as those of my series, in which the readings are markedly above normal, there is high blood-pressure, and that the record cannot be solely due to increased resistance of the arterial wall. There were, moreover, in most of my cases, the clinical evidences of high arterial pressure.

be abnormally resistant, being more so than those of the upper extremity; and there was also in nearly all of them marked thickening in the arteries of the upper extremity.

A certain number of cases of normal or low blood-pressure, in which the arteries presented no clinical evidences of disease, were investigated in the same manner in order to afford a basis of comparison; and, lastly, a few patients in whom the superficial vessels were obviously thickened, but in whom the pressure was not high, were likewise observed.

I have to thank Drs. J. K. Fowler, W. Pasteur, W. E. Wynter, and A. F. Voelcker for permission to publish the records of those cases which were under their care. I have also to thank my friend and colleague, Dr. E. H. Colbeck, for many valuable suggestions as regards the subject-matter of the paper.

CONCLUSIONS.

These observations show that resistance due to the arterial wall may markedly influence the readings, for the following reasons:—

(1) In cases of definitely high arterial blood-pressure observations taken in the leg yield in nearly all cases markedly higher systolic readings than those taken in the arm of the same patient (the limb in both observations being, of course, at the level of the heart); further, the most marked differences between the systolic arm and leg readings occur in the cases of highest blood-pressure. On the other hand, the diastolic readings in the leg are identical with those in the arm.

(2) Such a difference between the systolic arm and leg readings is the exception in cases of normal or low blood-pressure without obvious arterial thickening.

(3) Inasmuch as the blood-pressure in the leg cannot be higher than that in the arm (the limb in both cases being at the level of the heart), and seeing that the influence of the tissues superficial to the artery may be neglected, it necessarily follows that the difference between the arm and leg readings can only be due to some abnormal condition of the arterial wall, and is, in fact, a numerical measure of the amount of force necessary to overcome the resistance of that wall.

(4) The conclusion that the abnormal condition of the arterial wall is a direct result of the increased blood-pressure would seem to be inevitable, for the only obvious difference between the conditions to which the arteries of the lower extremities and those to which the arteries of the arms are exposed in daily life is that the former are subjected to greater hydrostatic pressure from a higher column of blood than are the latter.

DETAILS OF METHOD OF INVESTIGATION.

The instrument used for the majority of the observations was the latest form of compressed-air hæmomanometer of Oliver, in which the Riva-Rocci method is adopted. A few observations were, however, made with Stanton's modification of the same method (*viz.*, Nos. 1 and 10 of the high-pressure cases, and No. 4 of the series of arterial thickening without high blood-pressure). In all cases Oliver's improved armlet (12 cm. breadth), of which the outer cover is composed of three sections, so as to be capable of adjustment to the fusiform-shape of the forearm, was used, with the single exception of No. 1 of the high-pressure cases, in which Martin's armlet (12 cm. breadth) was employed. The observations on the lower extremity were made on the calf, the pulse in the anterior or posterior tibial artery being felt.

As before stated, care was taken that the part of the limb on which the observation was being made was at the level of the heart, so as to eliminate any error due to the influence of gravity. The patients were invariably in the recumbent posture. Care was also taken that oedema was absent, as the presence of this condition vitiates blood-pressure readings. The results are recorded in millimetres of mercury.

The numbers in the last column are those showing the difference between the mean systolic pressure reading in the arm and forearm, and that in the leg. In those cases in which the pressure reading in the leg was lower than that in the arm this number is given as a minus quantity. Abbreviations: syst. = systolic pressure; diast. = diastolic pressure.

Of the twenty-one high-pressure cases all except two (Nos. 11 and 12) show a higher systolic reading in the leg than in the arm, the average difference being upwards of 32 mm. If we consider the nine cases in which the average arm readings are above 185 mm., the result is even more striking, the average difference actually reaching the high figure of 44 mm. This increased difference in the cases of highest blood-pressure is precisely what we should expect on the assumption that arteriosclerosis owes its origin to high blood-pressure, for the height of blood-pressure being more pronounced in these cases, its more accentuated effect on the arteries of the lower than on those of the upper extremity (as revealed by the more marked difference between the hæmomanometer readings) would naturally result, owing to the greater hydrostatic pressure to which the former are exposed.

In regard to the diastolic pressures it will be found that the average reading in the leg measurements is practically identical with, being

slightly less (1.9 mm.) than, that in the arm. This is in accordance with what one would expect, seeing that the diastolic reading can be in no way affected by arterial resistance.

The ten cases of normal pressure with no arterial change stand out in marked contrast to the high-pressure series. It is true that in four instances (Nos. 1, 4, 5 and 9) the systolic reading in the leg is higher than that in the arm; but in only two (Nos. 4 and 9) of these is the difference marked, and in one (No. 5) it is quite insignificant. The average difference between the arm and leg readings of this series is less than 2 mm.

Last of all, in regard to those cases which showed clinical evidence of arterial thickening, but no high blood-pressure. In these no general excess of systolic reading in the leg above that in the arm is observable, for No. 4 is the only case to show a marked difference. Inasmuch as the arterial thickening in these cases is not the result of high blood-pressure, there is no reason why it should exhibit any special preference for the leg vessels as opposed to those of the arm; rather one would anticipate that it would be irregular in its distribution; and this is entirely in accordance with the results.

I submit that the results of this investigation point overwhelmingly to the conclusion and, indeed, can be explained by no other hypothesis than that the blood-pressure readings are markedly influenced by the condition of the arterial wall; and, further, that the fact of the more marked arterial changes (whether these be due to actual sclerosis or to hypertonus, or to a combination of these two factors) in the lower than in the upper extremities, as revealed in high-pressure cases, affords weighty evidence that high blood-pressure is a cause of arterial thickening.

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TABLE I.—CASES OF HIGH BLOOD-PRESSURE.

Case No.		LEG		UPPER EXTREMITY				Difference between syst. of arm and leg
				Forearm		Arm		
		Syst.	Diast.	Syst.	Diast.	Syst.	Diast.	
		mm.	mm.	mm.	mm.	mm.	mm.	mm.
1	Male, aged 39; examination of vessels not made	208	105	173	110	—	—	35.0
2	Female, aged 59; radials and brachials feel normal, anterior tibials abnormally firm	180	100	165	100	163	100	16.0
3 ¹	Male, aged 57; marked thickening and tortuosity of brachials and radials	175	100	147	120	153	100	25.0
4	Male, aged 68; marked thickening and tortuosity of left radial, thickening of left brachial and ulnar; right arm in condition of atelectosis; dorsalis pedis on both sides very markedly thickened and tortuous	190	90	160	100	160	90	30.0
5	Female, aged 50; radials, brachials, anterior and posterior tibials thickened but not tortuous	258 240	130 180	228 230	130 130	236 232	130 130	17.5
6	Female, aged 43; brachials and radials thickened, radials rather tortuous, anterior tibials abnormally hard, tortuous, posterior tibials not felt	225	120	200	120	199	130	25.5
7	Male, aged 58; marked thickening and extreme tortuosity of radials; brachials also thickened, left one markedly so, both very tortuous; anterior tibials abnormally firm, not tortuous	220	105	190	90	173	100	38.5
8	Male, aged 45; radials and brachials markedly tortuous and thickened, anterior tibials abnormally firm and rigid	267	120	210	115	220	125	52.0
9	Male, aged 43; radials and brachials distinctly thickened and rigid but not obviously tortuous, posterior tibials abnormally rigid	188	105	168	120	170	110	19.0
10	Male, aged 44; radials thickened and rather tortuous, brachials markedly thickened and very tortuous, dorsalis pedis on both sides abnormally firm and tortuous	262 300	130 150	203 234	130 130	185 240	130 140	65.5
11	Female, aged 49; radials and brachials show no obvious changes (difficult to feel)	148	120	155	120	148	100	— 3.5

¹ The average systolic readings in the upper extremity, though on the occasion of the comparisons between the arm and leg pressures only 150 mm., were found to be on the average, as the result of numerous other observations, much higher. The case, therefore, naturally appears among the high blood-pressure cases.

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TABLE I.—CASES OF HIGH BLOOD-PRESSURE (continued).

Case No.		LEG		UPPER EXTREMITY				Difference between syst. of arm and leg
				Forearm		Arm		
		Syst.	Diast.	Syst.	Diast.	Syst.	Diast.	
		mm.	mm.	mm.	mm.	mm.	mm.	mm.
12	Male, aged 56; radials feel rather hard, but no other obvious evidence of arterial change	125	100	170	110	143	90	-31.5
13	Male, aged 71; very marked tortuosity of radials and brachials, also thickening; arteries of feet difficult to feel, but seem to be abnormally hard	258	110	163	120	165	100	94.0
14	Male, aged 73; radials and brachials markedly thickened and tortuous, right posterior tibial abnormally hard, left one not felt, left anterior tibial abnormally hard	170	90	170	95	153	90	8.5
15	Male, aged 41; radials and brachials tortuous and markedly thickened, posterior and anterior tibials abnormally firm	221	150	185	—	190	140	33.5
16	Male, aged 46; radials thickened but not tortuous, brachials thickened and tortuous, posterior and anterior tibials abnormally firm, right anterior tibial tortuous	200	100	144	100	181	110	37.5
17	Female, aged 58; radials thickened and contracted, brachials not felt, anterior tibials and left posterior tibial abnormally rigid, right posterior tibial not felt	186	100	173	120	180	120	9.5
18	Male, aged 53; radials and brachials thickened and slightly tortuous, anterior and posterior tibials abnormally rigid, one of anterior tibials tortuous	256	90	218	110	203	110	45.5
19	Male, aged 42; radials thickened but not tortuous, brachials hard to feel, posterior tibials seem fairly normal, right anterior tibial abnormally rigid, left one not felt	254	140	220	140	224	140	32.0
20	Male, aged 70; radials and brachials markedly thickened and tortuous, anterior tibials abnormally rigid	276	100	225	100	220	90	53.5
21	Male, aged 55; radials thickened and markedly tortuous, brachials markedly thickened and tortuous, right posterior tibial abnormally rigid, left one not felt, anterior tibials abnormally rigid	265	80	198	80	182 190	80 80	75.0

Mean difference between systolic readings in *brachio* and leg, 32.3 mm.; mean difference between diastolic readings in arm and leg, 1.9 mm. (leg reading lower than arm reading).

TABLE II.—CASES OF NORMAL OR LOW BLOOD-PRESSURE.

Case No.		LEG		UPPER EXTREMITY				Difference between syst. of arm and leg
		Syst.	Diast.	Forearm		Arm		
				Syst.	Diast.	Syst.	Diast.	
		mm.	mm.	mm.	mm.	mm.	mm.	mm.
1	Male, aged 31 ...	115	70	102	67	—	—	13.0
2	Male, aged 16 ...	110	60	115	80	—	—	5.0
3	Male, aged 46 ...	130	80	137	75	—	—	7.0
4	Male, aged 22 ...	123	60	88	60	—	—	35.0
5	Male, aged 28 ...	110	70	103	65	—	—	7.0
6	Male, aged 21 ...	107	80	118	100	130	85	17.0
7	Female, aged 49 ...	100	85	126	100	125	95	25.5
8	Male, aged 22 ...	105	80	106	90	115	90	5.5
9	Female, aged 28 ...	113 123	85 90	93 89	80 80	— —	— —	27.0
10	Male, aged 25 ...	86	80	91	75	—	—	5.0

Mean difference between systolic readings in arm and leg, 1.7 mm.; mean difference between diastolic readings in arm and leg, 2.2 mm. (leg reading lower than arm reading).

TABLE III.—CASES OF ARTERIAL THICKENING WITHOUT HIGH BLOOD-PRESSURE.

Case No.		LEG		UPPER EXTREMITY				Difference between syst. of arm and leg
		Syst.	Diast.	Forearm		Arm		
				Syst.	Diast.	Syst.	Diast.	
		mm.	mm.	mm.	mm.	mm.	mm.	mm.
1	Male, aged 36; radials and brachials thickened and tortuous, radials markedly so	153	96	153	96	—	—	0
2	Male, aged 51; radials tortuous, rather thickened; brachials difficult to feel, also arteries of lower extremities; pulse of lower extremities difficult to feel	132	100	120	70	150	75	3
3	Male, aged 62; marked thickening and tortuosity of right radial, less so of left; right brachial markedly thickened, left less so; right anterior tibial abnormally rigid, left not felt	148	80	140	90	140	85	8
4	Male, aged 76; radials and brachials thickened and tortuous; right dorsalis pedis feels abnormally firm, tortuous, left normal; left posterior tibial normal, right not felt	183	110	145	110	129	110	46

¹ This case, although having on the occasion when the leg arm comparisons were made a forearm reading of 153 mm., had an average reading in the upper extremity of well below 140 mm., and so is naturally classified among these cases.

DISCUSSION.

The CHAIRMAN (Dr. T. H. Green) said the pressure of the blood within the vessels was such an all-important factor in clinical medicine, and the difficulty of estimating that pressure appeared to be so great, that contributions such as had been heard were specially valuable. On behalf of the Fellows, he congratulated Dr. Williamson upon his work and thanked him for his paper. It was very important, as the author said, to determine how far the pressure readings were influenced by the condition of the vessel wall. On that question there was no very definite agreement among the various workers on the subject. Much had been done in the last few years, but there yet remained much to learn. And while waiting for that further information, many of them had to be guided by their unaided senses. When the blood-pressure was much increased, and the arteries were markedly thickened, the facts could be recognised by the unaided finger. He thought there was rather a danger at the present time of depending too exclusively on instrumental aids, and he thought they would be wise not to neglect unaided clinical observation. Even when they had got the most perfect instruments, without clinical skill they would fall far short of what they ought to be. He was sure Dr. Williamson wished his work to be criticised, and there were several well-known authorities who were competent to do so.

Dr. W. P. HERRINGHAM reminded Fellows that he recently contributed a paper on the subject to the Section (p. 37), and it was one in which he took much interest. On that occasion Dr. Leonard Hill suggested that many of Dr. Womack's and his (Dr. Herringham's) results were due to a mistake; that although the arteries on which they worked were removed from the body, they were not really dead, and still remained contractile, so that much of the resistance of the arterial wall was due to a vital contractility remaining in the artery. He allowed, at that date, that he had not taken sufficient trouble to eliminate that; his impression had been that that factor would have been eliminated twenty-four hours after death, and it was only shortly before he read his paper that he discovered that such was not the case. He had not then sufficient time to institute fresh proceedings, but since then he had been experimenting with arteries which, first, had been removed from the body twenty-four hours after death, and, secondly, had been kept for three days in a solution of 1 per cent. sodium fluoride, as recommended to him by Professor Macwilliam, who had done a great deal of work on the subject and who thought the arteries might then be regarded as dead. And he found that even arteries so treated still gave in some cases, just as they did in his previous experiments, high-resistance readings. He had arteries which gave the following readings: 8, 13, 5, 4, 6, 12, 15, 22 mm. Hg. He thought that was enough to show that there was something in the artery itself quite apart from contractility, because it was not every case which gave a high resistance due to what he took to be the arterial wall; it was so in two cases out of seven:

one with 18 mm. and the other with 15 mm. of mercury, due to what he claimed was the resistance of the arterial wall. Secondly, he had recently been experimenting, in a way which Dr. Leonard Hill showed him, with simultaneous readings from the arm and leg, testing the two pulses at the same moment. And he found, just as Dr. Williamson found, that when there was a very high resistance, 200 mm. or thereabouts, the leg resistance was much higher than that of the arm; that the legs took 30 mm. or 40 mm. more to compress them above that necessary to compress the arms. He was much puzzled by the whole thing. Therefore he thought it necessary to take every precaution to see whether that particular experiment was correct. Dr. Leonard Hill had made experiments which seemed to point in the opposite direction. He (Dr. Herringham) therefore tried with the sphygmograph to see whether it was due to his being unable to feel the radial pulse, and whether it was really persisting longer than he thought it was. But it was the same with the Mackenzie sphygmograph, which he regarded as the best, it having a big lever the smallest movement of which could easily be detected. He found it stopped at the point he thought it stopped at before, but he could still feel the pulse going on in the foot as plainly as possible after the sphygmograph had failed to register the pulse in the wrist. At his invitation Dr. Williamson examined a patient at St. Bartholomew's Hospital, and he had kindly allowed Dr. Herringham to see the arm and leg readings which he made. They showed a difference between left arm and left leg of 36 mm., while the difference between the right arm and leg was 72 mm. That man died, and he took out his arteries, and, after keeping them three days in the solution referred to, found there was a difference of over 60 mm. of mercury between the leg and arm on the right side, but much less on the left. It was very much what Dr. Williamson had arrived at during life. The experiments were rough, but the results were such as to be striking. He had another patient, who was now dying, and in whom he had been able to estimate the difference between the arm and leg during life. It would be interesting to him to see whether the same difference held in the arteries after death. If so, he thought it went some way to prove that the results they were getting were not the results of bad observation. He thought the results after death were valuable corroborations of the clinical data during life. It was only one case, and it was only interesting in so far as a single case could be.

Dr. LEONARD HILL, F.R.S., said he desired to lay before the meeting two points of view. The first was as to whether the difference between the arm and leg really existed. The determination of that really required a great deal of work, because there were differences between the results of various observers which needed explanation. Did a difference really exist between the arm and the leg, or was it a matter of experimental error in the observations? In the patient whom Dr. Williamson brought up for test, the first time Dr. Herringham raised the pressure, when the leg was stretched out forcibly in a line with the body, the pulse in the leg persisted after it had disappeared in the arm. There seemed to be a difference of 20 mm. to 30 mm. between the two. Then he (Dr. Hill) put the leg in a more relaxed position, so as to be

sure there was no muscular spasm, and on doing that he got conflicting readings—at one minute it seemed that the leg was persisting when the arm had ceased, and at another the reverse was the case, as the pressure oscillated up and down, and it seemed that there was no definite conclusion to be drawn from that series of observations. It was most important that there should be absolute muscular relaxation. During the last few weeks he had not been able to do any work on that question, but Dr. Holtzmann, of the London Hospital, had been carrying out work for him. Dr. Holtzmann said that when he first started taking cases of high pressure—those above 140 mm. or 150 mm.—sometimes the leg reading was the higher, sometimes the arm. To test his instruments he tried taking simultaneous readings of right and left arms and found one of his armlets always gave a higher reading than the other. He traced that to the fact that there was tubing of a more resistant type on one armlet than on the other, so that part of the pressure was exerted in dilating the tubing rather than the bag. That showed how easily instrumental error might arise. Having got the instruments in order, he found that the arm and leg pressures were practically equal when the patient was horizontal, in cases below the normal, and normal, and above the normal. But in all those cases there was obvious arterial degeneration; in one case of very high pressure there was a marked difference in the two readings, agreeing with Dr. Herringham and Dr. Williamson—arm 240, leg 300 (about). But Dr. Holtzmann said he did not think the case proved anything, as the readings were difficult to take, because long before the pressure reached 300 the pain produced caused muscular contractions, which sometimes even emptied the mercury out of the manometer, so that very high-pressure readings must be taken rapidly, owing to the pain caused; it was uncomfortable to have the circulation stopped with the pressure so high. He (Dr. Hill) had insisted that all the readings must be taken simultaneously; to take them successively on the general blood-pressure was not any good. The result of obstructing the leg might be less or more than that of obstructing the arm. Again, in these cases of degenerate arteries, the systoles were constantly varying in strength; they did in the patient now present, and Dr. Holtzmann found the same thing; there might be a difference of 40 mm. or 50 mm. between different systoles. The way to carry out the simultaneous readings was to have one armlet on the posterior tibial and one on the brachial, both connected to one manometer, to put the pressure in both armlets up at once, and note when the disappearance of the pulse took place, seeing that the limbs were in the relaxed position. The first reading must not be taken, because it might possibly require more pressure to open up one bag than the other. He read the pressure several times, putting it up and down by a few millimetres till the pulse just disappeared or reappeared. The sustained compression of the artery led it to relax and diminished the chance of error coming in from the rigidity of the wall of the artery. The readings taken by Dr. Williamson on normal subjects were of a puzzling character. He (Dr. Hill) had tested large numbers of normals, but had never found those differences in arm and leg. It was a stock experiment among his students in the laboratory

to demonstrate the influence of gravity on the circulation. They first found that the pressure in the arm and leg in the horizontal position was the same; then they stood upright and noted the difference between arm and leg, which, roughly, equalled that of the column of blood which separated the two points. That difference always came out within a few millimetres of mercury. But Dr. Williamson showed differences of 20 mm. and 30 mm.—sometimes a positive and sometimes a negative difference—and adopted the unscientific proceeding of allowing the positives and the negatives to neutralize each other, and so bring out an average difference of 1.7 mm. He had not himself come across any large differences in high-pressure cases and cases of degenerated arteries. He had tested a fair number of cases with one arm up and one down, and he found that the gravity difference came out: one pressure was greater than the other by the pressure of the column of blood separating the two armlets. Probably the difference between himself and Dr. Williamson turned on the question of method. There was another way of testing whether the sphygmomanometer was correct or not, namely, by the venous method which he (Dr. Hill) had suggested; they had done that on several cases of high pressure, recently on one of 190, which gave correct readings. As he described to the Section two or three months ago, the method was to put on one armlet and raise the pressure until obliteration of pulse was brought about. Suppose 150 mm. was found to obliterate the artery and stop the pulse, he lowered the pressure in that armlet to 145 and kept it at that figure; the blood then continued to come through and the veins filled up, and they could be felt to be getting more tense as the pressure in them rose. He wanted to measure what the pressure would rise to in the veins. No one would suggest that it required any pressure worth mentioning to flatten out a superficial vein; while keeping the first armlet at 145 he put on a second one, and raised the pressure in this to above 145; then, selecting a suitable vein above this armlet, he stroked it empty as far as the next valve, then rapidly released the pressure in the armlet until the vein just filled from below. The moment the vein filled his colleague read the pressure in the manometer; by that means the pressure in the veins was read, and it was found that the pressure there rose just to the blocking pressure—145 mm. It could not rise beyond—that would be contrary to all the laws of physics. As 150 was found to obliterate the artery, clearly the obliteration pressure was only in error 5 mm. That would be found to be a corroborative method of testing whether the sphygmomanometer was in error. So far he had found no reason to doubt the general accuracy of the readings. Except in one case, Dr. Herringham's figures taken after death bore no relation to the very great differences which Dr. Williamson brought out. Post mortem, one could throw an artery into an extraordinary degree of contraction. He had taken the carotid from an ox and scratched it, and took another and froze it and relaxed it; and one had been 5 mm. or 6 mm. in diameter, while the other had shrunk to 2 mm. An artery injured by a surgeon at operation shrank up, and the same occurred from injury during life. Dr. Williamson's figures were held to show that it occurred in life, and he supposed that was

what Dr. William Russell suggested, and that therefore the instruments were in error. At present, however, he (Dr. Hill) failed to accept that conclusion. His venous test supported his contention that the instrument read correctly, and he could not admit at present that those differences occurred between arm and leg pressures taken in the horizontal posture; but he did not deny that they might occur, and he was still looking for them. If they did occur, it did not prove that the instrument was in error, because it was well known from experiments carried out in the dog that the systolic pressure, as measured by Hürthle and others, was higher in the femoral than it was in the carotid; the crest of the wave was higher. He did not know why, unless it was an instrumental error; possibly the femoral could kick up the manometer better than the carotid. If it was not an instrumental error, how could one explain the systolic wave being higher in the femoral than in the carotid? It had been suggested that the primary positive pulse wave went down to the periphery and was a swing back as a reflected wave, and that in the femoral the crests of those two waves met, and one was piled upon the other. In the carotid the crest and trough met. Another suggestion was that the arteries had a propulsive power; that the aorta might force on the blood by the propulsive power of its own coat. There was no evidence of that. He relied on his venous test and gravity tests. He asked that the gravity difference might be taken with the subject first in the erect, then in the horizontal posture, and lastly with the legs elevated, and if that came out he thought it showed that the instruments were reading truly. The gravity difference came out well in the laboratory, and students constantly obtained it.

Dr. HERRINGHAM desired to add a word of explanation. Dr. Leonard Hill said none of his (Dr. Herringham's) results came out anything like those given on the board. That was true, but he did not select his cases; he took them just as they happened to come into the post-mortem room. Only one of the cases was of very high pressure, and there he did get a very marked difference. But Dr. Williamson's were selected high-pressure cases. That explained why his (Dr. Herringham's) post-mortem readings did not show differences to that extent.

Dr. C. O. HAWTHORNE said he supposed all would admit that the condition of the arterial wall must play some part in determining the reading of the manometer. The question was as to whether this factor was or was not of clinical importance. If Dr. Williamson's readings were correct, the state of the arterial wall played an important part in the determination of the reading. Dr. Williamson would agree that his observations at present were limited to a comparatively small number of cases, and that they showed results so perplexing as to demand confirmation by other observers. He had said that the condition of the superficial tissues might be disregarded, but he (Dr. Hawthorne) doubted that. The absence of contractions from the muscles of the limb is not easy to guarantee, and to state the exact moment when the pulse disappeared from the wrist is not always easy. It is still more difficult to be quite certain of the exact point of disappearance when trying to feel the pulse

in the lower limbs; therefore from these observations errors due to the personal factor could not wholly be excluded. Further, as Dr. Leonard Hill had said, the strength of the systole is not always a constant quantity. Consequently, what was one to say was the actual blood-pressure if the point of disappearance of the pulse in different systoles varied? He showed a pulse tracing which exhibited typical *pulsus alternans*, the vessel needing for complete suppression of the pulse 175 mm. of mercury, while the weaker wave disappeared at a pressure of about 140 mm. If the sphygmometer measured the blood-pressure, what was the blood-pressure in this individual patient? He also showed two contracted sphygmograms—one a typically high-tension pulse, and the other a typical low-tension or dicrotic pulse. Yet the measurement of these by means of the ordinary blood-pressure instrument was identical—namely, 175 mm. of mercury. In another typically dicrotic pulse which Dr. Hawthorne showed the reading was 130 mm., while a tracing from a case of Addison's disease had the typical high-tension characteristics, yet the sphygmometer reading was only 80 mm. The differences of opinion expressed on the matter by different physicians made one ask whether the discussion as to the contribution made by the vessel wall to the reading of the sphygmomanometer was really of much clinical importance. In defining blood-pressure in the terms of the sphygmomanometer, they were presenting, at best, a partial and incomplete picture. For whilst possibly the sphygmometer might record the maximum distending force acting on the inner surface of the arterial wall, it contributed nothing to the question whether the strain on the artery with such cardiac systole was of long or short duration. It left out of account, in other words, the state of the peripheral arterioles. This question of the peripheral arterioles was in danger of being forgotten in the concentration of energy on the other factors which had been discussed. He urged that if the circulatory conditions were to be properly presented in a numerical or graphic fashion, it was necessary to have not only the reading from a sphygmomanometer, but also to present a sphygmograph tracing. And it was here that the educated finger found its value and justification, for while being less impressive and, perhaps, less exact than the two instrumental methods, it was able to appreciate at one and the same time both those features of the pulse of which the sphygmomanometer and the sphygmograph gave separate records.

Dr. LEONARD HILL desired to say, in reference to Dr. Hawthorne's remarks, that the influence of posture, together with the sphygmomanometer, enabled one to get a recognition as to the condition of the circulation—not only as to the systole, but as to the peripheral circulation. The pressure in the brachial in the normal young man was maintained at about the same level in the erect, inverted, and horizontal postures. By studying this in patients information could be gained as to the condition of the vascular system.

Dr. WILLIAMSON, in reply, said he did not say, as Dr. Hawthorne stated, that the superficial tissues could be disregarded, but that with a sufficiently broad band they could be disregarded. There were only three influences which could bear on the results of his observations by the Riva-Rocci method—namely,

the arterial pressure, the resistance of the superficial tissues, and the resistance of the arterial wall. The first two could be excluded, as everyone agreed, so the question was simply whether or not his observations were accurate. Dr. Leonard Hill had criticized some of his observations from the point of view of there being a great difference between the leg and arm observations (in normal cases). But the observations on those people were not taken simultaneously. The fact that the average diastolic readings of arm and leg, in both high and low pressure cases, differed by not more than 2 mm. sufficiently proved the accuracy of the observations. But since he wrote the paper he had taken simultaneous arm and leg observations on eight high pressure cases, and these bore out the results he obtained before. His clinical assistant and he took several observations; first one reading the manometer and the other observing the pulses, and then changing the other way round. The armlets were also changed, so that there could be no error due to possible differences in these. The results obtained were consistent. In one case there was a difference of considerably over 20 mm. between the arm and the leg, and several observations brought out the same result. Five of the cases gave distinct differences on one or both sides. In one case the arm readings came out higher than those of the leg, but in every single case his clinical assistant confirmed his observations. He would be glad to allow any Fellow to examine his cases if he wished, and he would be specially glad if Dr. Hill would do so. He had on several occasions observed that at one time there would be a very great difference between the arm and leg readings, and at another time a much less or no difference, and that was particularly so in cases of marked and obvious arterial degeneration. Dr. William Russell had laid stress on the fact that sclerosed vessels were specially prone to hypertonic contraction, and his observations confirmed Dr. Russell's in that respect. Dr. Hill, Dr. Herringham, and he, observed the left arm and left leg readings of one patient, and they all agreed there was no marked difference on that side. Next day he (Dr. Williamson) examined the right arm and leg of the same patient, and there was an average difference of 70 mm. between the readings. He was glad to hear that Dr. Herringham had confirmed that by post-mortem examination. In answer to Dr. Savill, he compared the condition in the arm and leg vessels by digital examination. In practically all the cases the arteries of the leg felt distinctly more rigid than those of the arm. Dr. Hill said that often one armlet might give a higher reading than the other, and he (Dr. Williamson) obviated that by changing the armlets over.

Dr. Williamson would like to say that he had originally been distinctly biased against the view that the arterial wall exercised any appreciable influence on the blood-pressure readings, and it was chiefly as the result of a conversation with his colleague, Dr. E. H. Colbeck, that he devised the above method of proof.

Medical Section.

May 25, 1909.

Dr. NORMAN MOORE in the Chair.

Nine Cases of Typhoid Fever treated with an Anti-endotoxic Serum.¹ The Ophthalmic Reaction in Typhoid Fever.

By R. TANNER HEWLETT, M.D.

It is now generally recognized that an anti-typhoid serum prepared by the injection of the horse with cultures of the typhoid bacillus is useless in the treatment of typhoid fever. Such a serum is anti-microbic and is analogous to the anti-streptococcic, anti-pneumococcic, and anti-plague sera. It is true that Chantemesse² has prepared a serum which, he claims, exerts a very favourable action in cases of typhoid fever. This serum, however, differs much from an anti-toxic serum, and Wright suggested, and Chantemesse has adopted the suggestion, that it really is a vaccine. On account of the relatively feeble curative properties of anti-microbic sera generally, the late Dr. Macfadyen³ conceived that if the bacterial cells be ground up so as to obtain the bacterial cell juices, such juices, being the specific poisons or endotoxins of bacteria which form little or no extracellular or excreted toxin, would, on injection into the horse, produce anti-endotoxic sera comparable to the anti-toxic sera obtained by the injection of bacterial toxins—*e.g.*, those of diphtheria and tetanus—and that such anti-endotoxic sera might possess curative properties. Dr. Macfadyen showed⁴ that an anti-endotoxic typhoid serum, obtained by injection of a goat with typhoid cell juice experimentally, did possess active neutralizing

¹ Part of the expenses incurred in the preliminary experimental work connected with the preparation of the serum was defrayed by a grant from the British Medical Association.

² See *Bericht über den XIV Internat. Kongress f. Hyg. und Demographie*, i, 1908, p. 195.

³ See "The Cell as the Unit of Life," Churchill, 1908 (Bibliography).

⁴ *Centralbl. f. Bakt.*, Jena, 1906, I Abt. (Original), xli, p. 266.

power against the typhoid endotoxin, 0.02 c.c. of the serum neutralizing thirty lethal doses of the typhoid endotoxin. At the time of his lamented death Dr. Macfadyen had immunised a horse to a high degree with the typhoid endotoxin, and the first four typhoid cases here recorded were treated with his serum. The remaining cases were treated with a similar serum, prepared under my direction, and I am much indebted to the facilities generously afforded me by Mr. Henry Wellcome, at the Wellcome Physiological Research Laboratories, for the preparation of the serum, and to the kind co-operation of members of the staff (Dr. Dale and Dr. Südmersen). I am also indebted to Mr. Thompson, laboratory assistant, whose assistance has been invaluable. All the cases were under my care at the Seamen's Hospital, Greenwich, and gave a positive agglutination (Widal) reaction. No case was treated which did not appear undoubtedly to be one of typhoid fever.

The serum prepared by Dr. Macfadyen possessed protective power, tested on guinea-pigs, as indicated in the following table:—

PROTECTIVE POWER AGAINST LIVING VIRULENT TYPHOID BACILLI (SERUM AND CULTURE INJECTED INTRAPERITONEALLY, ONE IMMEDIATELY AFTER THE OTHER). (MACFADYEN.)

Amount of typhoid culture		Amount of serum (c.c.)		Result
15 lethal doses	...	0.00002	...	Survived
90 "	...	0.02	...	"
15 "	...	0.00001	...	"
120 "	...	0.02	...	"
40 "	...	0.01	...	"
80 "	...	0.02	...	"
160 "	...	0.04	...	"

The anti-endotoxic power was such that 0.01 c.c. serum neutralized at least eight lethal doses of the typhoid endotoxin. The curative power was such that 0.5 c.c. serum saved the life of a guinea-pig injected intraperitoneally with fifteen lethal doses of typhoid culture six hours previously.

RECORD OF CASES.

Case I.—A. P., able seaman, aged 18, admitted February 26, 1907; period of disease between the fourteenth and twenty-first day; rose spots on abdomen; patient very ill; temperature 103° F. to 104° F. There was no sign of decline of temperature until the serum was given, when there was an immediate fall. Whereas the patient was sponged six times on the day previous to injection (as the temperature was above 103° F.), after the first injection the patient was sponged only once. The serum was given on eight successive days and once more, and

eleven days after the treatment was commenced the temperature reached the normal. Convalescence was uninterrupted, save for a rise of temperature, with serum rash, on the nineteenth and twentieth days after commencement of treatment. (*See Chart 1.*)

Case II.—School-boy, aged 9; admitted July 9, 1907; patient ill seven days; rose spots on abdomen. Although temperature not high, the patient was very ill and delirious. Serum commenced fifth day after

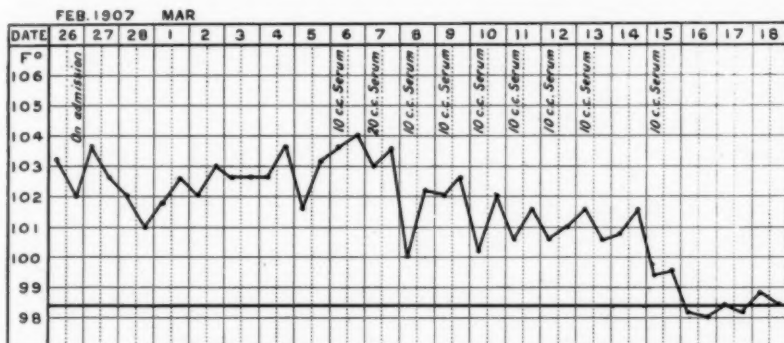


CHART 1.

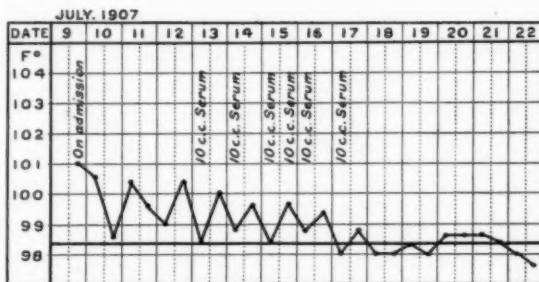


CHART 2.

admission—*i.e.*, about twelfth day of disease. Immediate and marked improvement in general condition after second injection of the serum. Six injections were given in five days, then the temperature reached the normal. Convalescence was uninterrupted. (See Chart 2.)

Case III.—A Russian fireman, aged 37; admitted November 22, 1907. No history obtainable. Patient ill and slightly delirious. Serum treatment commenced on the fifth day after admission and continued for

nine days, and two more; but although there was an improvement in the condition and the temperature became lower, this did not reach the normal until about three weeks after the treatment was commenced. Serum rash was, however, troublesome between the seventh and twelfth days of treatment. On the sixteenth day the temperature seemed to be rising again and two more injections were given, after which the temperature became normal. Convalescence uninterrupted. (See Chart 3.)

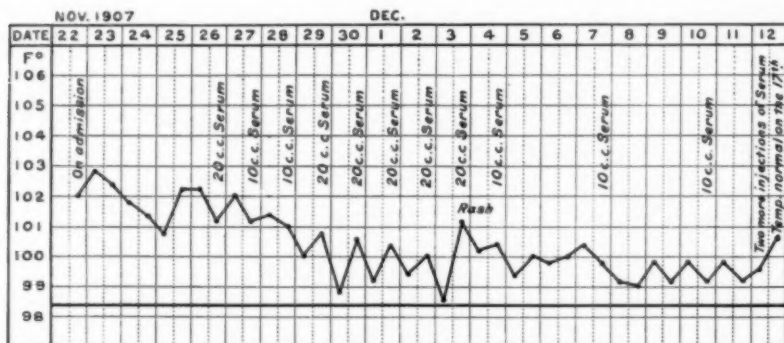


CHART 3.

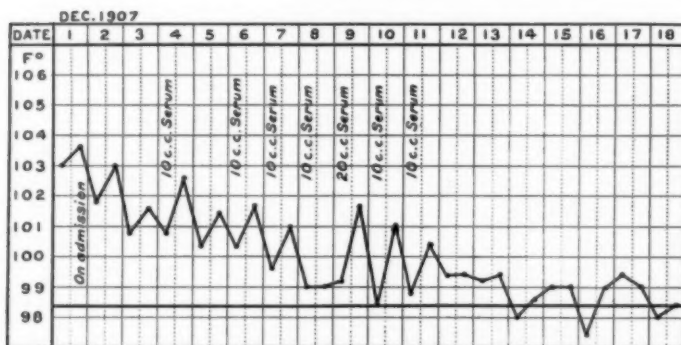


CHART 4.

Case IV.—A Greek, aged 26, gunner on a gunboat; admitted on December 1, 1907. Reliable history not obtainable, but period of disease was probably the end of the first week. Serum treatment commenced on the third day after admission and continued for eight days. Temperature reached the normal a fortnight after admission. (See Chart 4.)

The remaining cases were treated with the new serum prepared under my direction. The horse at this period was by no means fully immunized, but it seemed worth while to try the serum. The serum (tested in the same manner as Dr. Macfadyen's serum) possessed the following protective power on guinea-pigs:—

Amount of typhoid culture		Amount of serum (c.c.)		Result
15 lethal doses	...	0.00001	...	Death
15 "	...	0.0001	...	"
15 "	...	0.005	...	Survived
45 "	...	0.001	...	Death
45 "	...	0.01	...	Survived
90 "	...	0.05	...	Death
90 "	...	0.1	...	"
90 "	...	0.2	...	"
90 "	...	0.2	...	Survived
90 "	...	0.5	...	"
150 "	...	1.0	...	Death
150 "	...	2.0	...	"
150 "	...	3.0	...	Survived

The serum, therefore, was not so active experimentally as that prepared by Dr. Macfadyen. It is to be noted, however, that this horse had not received nearly so much typhoid endotoxin as that of Dr. Macfadyen's.

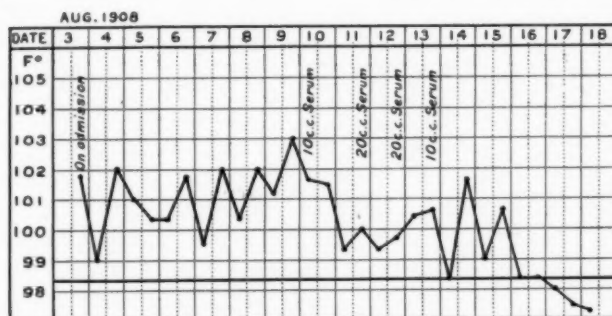


CHART 5.

Case V.—French able seaman, aged 23; admitted August 3, 1908; ill eight days; rose spots and typhoid stools. Serum treatment commenced a week after admission; during this period the temperature was tending to rise. Serum given on four days, and on seventh day from the commencement of treatment temperature reached the normal. Convalescence uninterrupted. (See Chart 5.)

Case VI.—Swedish fireman, aged 18; admitted September 5, 1908. Patient stated that he had felt ill for only two days before admission.

250 Hewlett: *Serum Treatment of Typhoid Fever*

Rose spots appeared and spleen felt enlarged four days after admission. Period of disease probably end of the first week. Serum treatment commenced on the third day: four injections in all. Temperature reached the normal on the eleventh day after admission. Convalescence uninterrupted. (See Chart 6.)

Case VII.—Swedish seaman, aged 19; admitted August 26, 1908. Stated to have been ill eight days. Patient very ill and violently

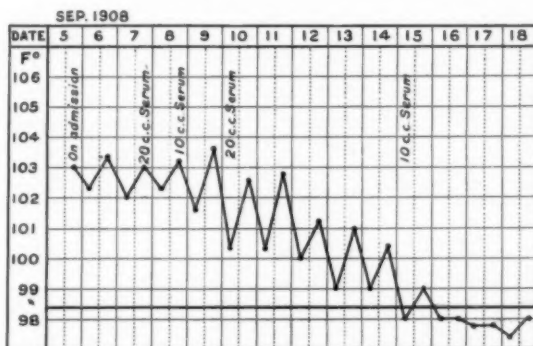


CHART 6.

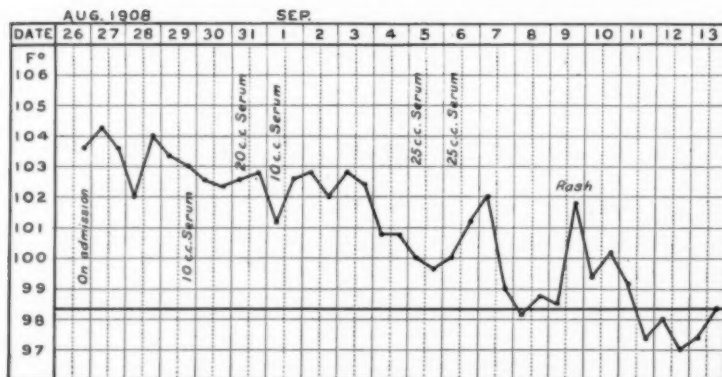


CHART 7.

delirious; pulse rapid (100 to 112), weak, and irregular. A few rose spots. Intestinal hæmorrhage on August 27, August 28, and August 29. Serum treatment commenced on the fourth day after admission; five injections in all. Temperature reached the normal on the thirteenth day after admission. A very bad case. Convalescence uninterrupted. (See Chart 7.)

Case VIII.—Russian seaman; admitted September 24, 1908. Does not speak English. Said to have been ill two days. Serum treatment commenced third day after admission; five doses. Temperature reached the normal a week after admission. Convalescence uninterrupted. (See Chart 8.)

Case IX.—Bargeman, aged 24; admitted September 21, 1908. Ill fourteen days; slight diarrhoea; spleen enlarged and tender; few rose

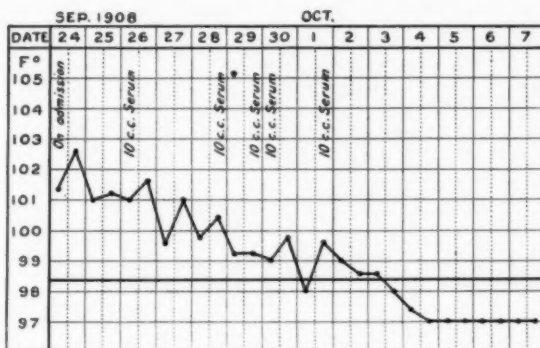


CHART 8.

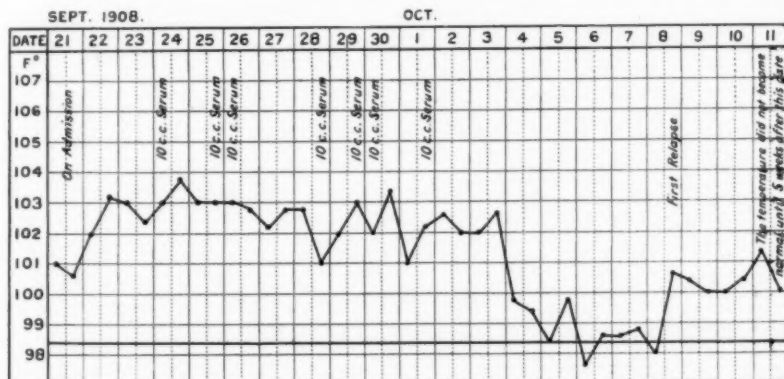


CHART 9.

spots on abdomen. Serum treatment commenced fourth day after admission, and continued for six days with one day's interval: 70 c.c. in all. Temperature became normal on October 5, but soon rose again, and patient had two relapses. Eventually he recovered. The typhoid bacillus was isolated from the blood during the second relapse. (The supply of serum had run out, therefore the treatment was not continued.)

DISCUSSION OF RESULTS.

Nine cases are, of course, too few a number on which to base any general conclusion. I may, however, give the impressions which I gained from the serum treatment of these cases. In only one case did the serum prove to be an absolute failure—viz., in Case IX. Even in this the primary attack may have terminated as a result of the treatment.

Case VII was a very severe case, and seemed immediately to react to the serum, and the temperature reached the normal by the end of the third week.

Case III, in which, however, no history was obtainable, also seemed to react to the serum, but the normal temperature was not reached until three weeks after the commencement of the treatment.

Case I, another severe case, also seemed to react immediately to the serum. The period of the disease was, however, between the fourteenth and twenty-first day.

Cases II, IV, V, VI, and VIII seemed undoubtedly to have been benefited by the serum.

Case II was a severe case, and the serum treatment was commenced at about the tenth to the twelfth day. As a result there was an immediate amelioration in the symptoms, and the temperature became normal by about the sixteenth day.

In Case IV, in which the treatment was commenced at about the tenth to the twelfth day, the temperature became normal by about the twentieth day.

Case V was in hospital a week before the serum was used, and during this period the temperature was tending to rise. Immediately after the first dose of serum (given about the sixteenth day) there was a marked fall in the temperature, which became normal about the twenty-first day.

Case VI is most striking. In this the serum treatment was commenced probably at the end of the first, or beginning of the second, week, and the temperature became normal by about the fourteenth day.

Case VIII is another striking case. Here, again, treatment was probably commenced by the end of the first week, with the result that the temperature became normal by the end of the second week.

My impression is, therefore, that several cases were benefited by the serum, and that in two at least the disease was cut short by its use. Typhoid fever is, however, such a variable disease that it is extremely

difficult to estimate the value of any form of treatment unless a large number of cases is available. In no instance did the serum appear to do any harm.

Unfortunately, cases of typhoid fever usually only come under observation when the disease has existed for a week or longer. If the disease has existed for some time, no *immediate* effect can be hoped for from the use of a serum, however efficient this may be. Moreover, the existence of secondary infections, which often occur in typhoid fever, must limit the value of any serum. As regards dosage of the serum, my impression, gained from these few cases, is that the serum should be given freely—viz., 10 c.c. to 20 c.c. daily until the temperature has reached the normal, and perhaps in smaller doses for a week subsequently.

Another point is that the preparation of an anti-endotoxic serum is at present in the experimental stage. We do not know for how long the horse should be immunized, and I am inclined to think that it is possible to over-immunize; that is, the serum does not increase in potency after a certain number of doses of the typhoid endotoxin: in fact, harm may result from continuing the injection of the horse. The horse seems to be extremely susceptible to the typhoid endotoxin, and a period of eight months elapsed before a dose of 30 c.c. could be given. In a second horse which I am immunizing the commencing dose of toxin was 0.1 c.c., and even this small dose caused so much disturbance that it had to be repeated five times at weekly intervals before the dose could be increased.

THE OPHTHALMIC REACTION IN TYPHOID FEVER.

In 1907 Chantemesse¹ stated that he had succeeded in preparing a substance from cultures of the typhoid bacillus which, instilled into the eye, gives in cases of typhoid infection a reaction analogous to the ophthalmic reaction of Calmette in tuberculosis. Chantemesse prepares his substance by a complicated treatment of agar typhoid cultures involving emulsion, heating, desiccation, trituration with salt, extraction with water, a second heating, and precipitation with alcohol. The alcoholic precipitate is dried and dissolved in water, and this solution forms the test liquid.

I thought it would be of interest to test whether the typhoid endotoxin would give an ophthalmic reaction. For this purpose some typhoid endotoxin was carefully prepared, so as to eliminate as far as

¹ Bericht über den XIV Internat. Kongress f. Hyg. und Demographie, i, 1908, p. 205.

possible extraneous substances, and dried *in vacuo*. The solid was then handed to Dr. Goodall, who kindly offered to make some tests with it. He will tell you that the substance gave a marked reaction in several cases of typhoid fever. It also gave a reaction in one or two cases of other diseases, but did not give a reaction with several cases not typhoid. The interest of the observation is that the substance producing the reaction is thus shown to be an intracellular constituent of the organism.

A Short Account of Twenty-six Cases treated at the Eastern Fever Hospital with Anti-typhoid Serum.

By E. W. GOODALL, M.D.

TWENTY-SIX patients have been treated with the serum, twelve males and fourteen females; their ages ranged from 3 to 41, thirteen being over and thirteen under 20. Three died—two from perforation and peritonitis, and one from toxæmia (Cases 5, 14, and 16). Nearly all were clinically cases of enteric fever, and in all, with one exception, the agglutination-reaction was positive; this case (No. 5 in the table) was a case which was fatal from perforation.

A reference to the table will show that the serum was supplied in six different batches (*see* Column 4). The first two—E 176 and E 177—were prepared under the direction of the late Dr. Allan Macfadyen, the other four under that of Professor Hewlett. The serum was administered by subcutaneous injection in the usual way. The dose varied from 20 c.c. given in one injection to 200 c.c. given in eight injections spread over a period of eleven days. I was led to try single doses from reading Professor Chantemesse's pamphlet on the treatment of enteric fever with a serum he had prepared by immunizing horses with a toxin obtained by growing virulent typhoid bacilli in ox-spleen bouillon.¹ I admit, however, that the single dose I used was much larger than that given by Chantemesse, which was never larger than five drops.

Before discussing the value of the treatment I must point out one fact which, before I treated a single patient with serum, led me not to be too sanguine as to its results. I refer to the advanced period of the disease at which the patients were, in most instances, sent to hospital.

¹ "Sérothérapie de la Fièvre Typhoïde," Paris, 1907.

This is a fact which militates against any form of treatment, and is, I believe, one of the chief reasons why, in my hands, the bath treatment, for instance, has not achieved the excellent results obtained by other physicians. In ten only of the cases had the patient been ill for less than a fortnight, and only in five for less than ten days. Again, when patients are admitted late, it is difficult to be certain whether or not the disease is already on the mend.

No.	Sex	Age	Reference No. of serum	Day of disease	Amount of serum in c.c.	Number of injections	Other treatment	Complications
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1	M.	19	E 176 and E 177	15	120	6	None	Serum reaction
2	F.	4	E 177	8	47	2	Sponging	Serum reaction
3	M.	36	E 177	22	50	2	None	Serum reaction, albumin- uria
4	M.	15	E 177	15	100	4	None	Post-febrile dementia
5	M.	24	E 177	11	75	3	Sponging (after)	Serum reaction, perfora- tion; died
6	M.	28	E 177	14	200	8	None	Serum reaction
7	M.	13	A 242	8	80	4	Sponging (after)	Hæmorrhage, albuminuria
8	M.	26	A 242	22	20	1	None	None
9	F.	13	A 242	14	20	1	Sponging	None
10	F.	18	A 309	13	20	1	None	Serum reaction, laryngitis, otorrhœa
11	M.	15	A 309	11	20	1	Sponging (after)	None
12	M.	40	A 309	19	20	1	None	Serum reaction, albumin- uria
13	F.	12	A 309	24	40	2	None	None
14	F.	35	A 309	Late	42	2	None	Serum reaction, hæmor- rhage, albuminuria, relapse, perforation; died
15	F.	20	A 309	14	44	2	None	Serum reaction, albumin- uria
16	F.	36	A 309	19	20	1	None	Ulceration of larynx; died
17	M.	14	E 325	13	40	2	Sponging (after)	Serum reaction
18	F.	16	E 325	17	40	2	Sponging	None
19	F.	17	E 325	12	40	2	Sponging	Serum reaction
20	M.	17	E 325	9	60	2	Sponging	Pyuria, peritonitis
21	F.	23	E 325	14	40	1	Sponging	Hæmorrhage, necrosis of tibia
22	F.	37	A 422	6	80	2	Sponging	Serum reaction
23	M.	33	A 422	10	40	1	None	None
24	F.	41	A 422	21	120	3	Sponging	Serum reaction
25	F.	3	A 422	6	20	1	None	None
26	F.	39	A 422	13	80	2	None	None

In Column 8 is noted whether any treatment in addition to the serum was employed. This refers to such treatment as hydrotherapy,

intestinal antiseptics, and so forth, and not to the treatment of special symptoms or complications. The word "after" means that the treatment was commenced after the last dose of serum had been given. In Column 9 the words "serum reaction" mean that there was an anti-toxin rash or pains in the joints, usually with pyrexia, due to the serum.

In seven of the cases there was reason to think that the serum had a beneficial effect—viz., Cases 1, 2, 4, 23, 24, 25, and 26. I produce the charts of these cases (Charts 1 to 7). In Case 1 the temperature was lower the day after the fourth dose, and fell to normal three days later; in Case 2 the temperature fell very abruptly two days after the second dose, and was normal on the third day; in Case 4 the temperature fell to normal the day after the second dose, but did not become permanently normal until eight days later, two more doses having in the meantime been given; in Case 23 the temperature, which seemed to be falling when a single dose of serum was given, fell to normal within a few hours and remained there. This is a most unusual termination in enteric fever. This and Case 2 are the only ones amongst these which at all resemble Chantemesse's cases. In Case 24 the temperature was fairly normal five days after the first of three injections; in Case 25 the temperature was normal four days after the only injection given; in Case 26 the temperature fell to normal the day after the second of two injections and was permanently normal a few days later. It is possible, also, that the favourable termination of the illness in Cases 3 and 15 was due to the serum, but in none of the remaining cases was any effect apparent. I show charts of these cases (Charts 8, 9).

It is noteworthy that, of the nine cases in which there were reasons for supposing that the serum was beneficial, four were treated with one lot of serum (E 177) and four with another lot (A 422). The largest doses, however, were given in the cases treated with these lots. It should be pointed out that Cases 2, 4, and 25 were children, in whom the prognosis is usually favourable, and that though in Cases 2, 23, and 25 the treatment was commenced fairly early, yet in Cases 7 and 22, in which the treatment was resorted to as early, no beneficial result was apparent. It will be observed that in Cases 1, 2, 3, 4, 15, 23, 24, 25, and 26 the dose varied from 20 c.c. given in one injection to 120 c.c. spread over three injections in one case and six in another.

Enteric fever is a disease which varies in severity to a considerable degree within short periods of time, and the number of cases recorded above is very small, so that it is impossible for me to speak with certainty as to the efficacy of the treatment. I can only say that it

appears to have done good in a few cases and that it is worthy of further trial.

In the charts the black line represents the 6 a.m. and 6 p.m. temperatures. The dotted line represents the course of the temperature, taken every four hours, when it rose and fell or fell and rose between two consecutive 6-o'clock temperatures (morning and evening or evening and morning). The crosses with numbers indicate the days when serum was given, and the amount in cubic centimetres; in most cases the serum was given between noon and 2 p.m.

Day of Disease.

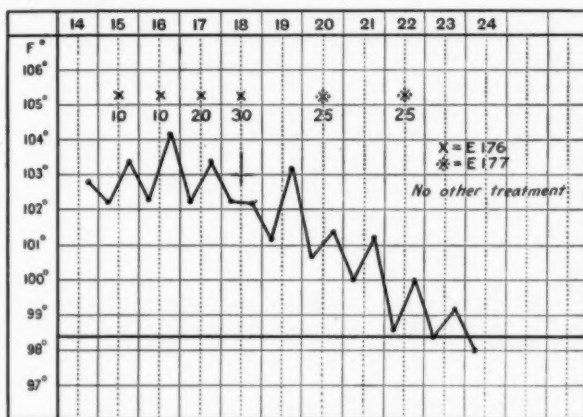


CHART 1 (Case 1).

NOTE ON THE "OPHTHALMIC REACTION" OBTAINED WITH TYPHOID ENDOTOXIN.

Professor Hewlett has asked me to relate the results I obtained with some specimens of typhoid endotoxin which he kindly gave me for ophthalmic reaction. I tried three specimens. The first was supplied to me in solid form, and I made a 1 per cent. aqueous solution; a few drops of the solution were instilled into the lower conjunctival sac of six cases of enteric fever, three of scarlet fever, and of one normal person. One of the enteric cases had a marked reaction in twelve to eighteen hours, and five had a slight reaction. One of the scarlet-fever cases had

a very marked reaction (swelling of eyelids); in this case the reaction was slight next day, gradually became more intense, and was at its height twelve days later. The other two cases of scarlet fever and the

Day of Disease.

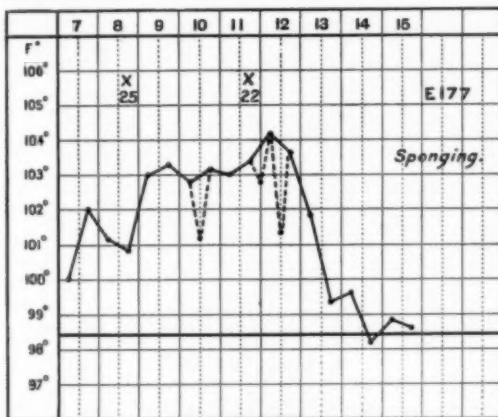


CHART 2 (Case 2).

Day of Disease.

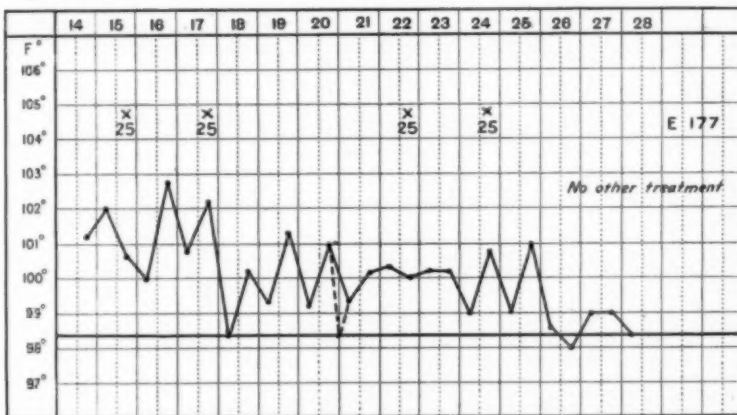


CHART 3 (Case 4).

normal person showed no reaction. The other two specimens were supplied to me in 1 per cent. watery solution. One (Endotoxin A) gave a marked positive reaction in three cases of enteric fever, a slight reaction

in one case of scarlet fever, and no reaction in another case of scarlet fever. The other (Endotoxin B) gave a marked reaction in one case of enteric fever, a slight reaction in two cases of scarlet fever, no reaction

Day of Disease.

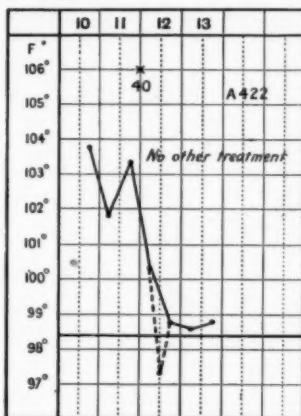


CHART 4 (Case 23).

Day of Disease.

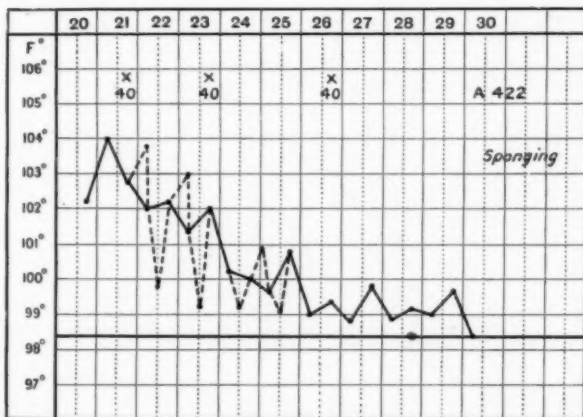


CHART 5 (Case 24).

in one case of scarlet fever, and no reaction in a case of indefinite pyrexia.

Putting the results obtained with the three specimens together, it will

be seen that of ten cases of enteric fever in which the test was applied, in five the reaction was marked and in five it was slight; whereas of ten cases in which the person tested was not affected with enteric fever, in one case

Day of Disease.

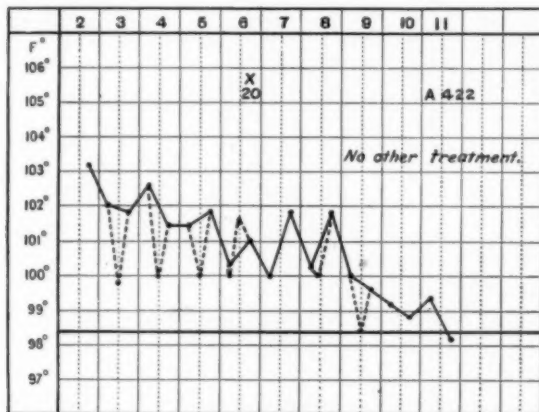


CHART 6 (Case 25).

Day of Disease.

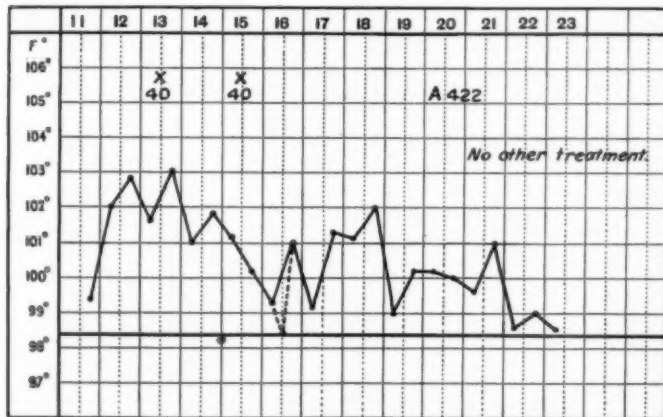


CHART 7 (Case 26).

the reaction was very marked (a case of scarlet fever), in three (all cases of scarlet fever) it was slight, and in six (four cases of scarlet fever, one case of indefinite pyrexia, and one normal person) there was no reaction.

I cannot help thinking that, in the case of scarlet fever in which a very severe reaction occurred, some other form of irritant must have been applied to the eye—for instance, friction. The height of the reaction was

Day of Disease.

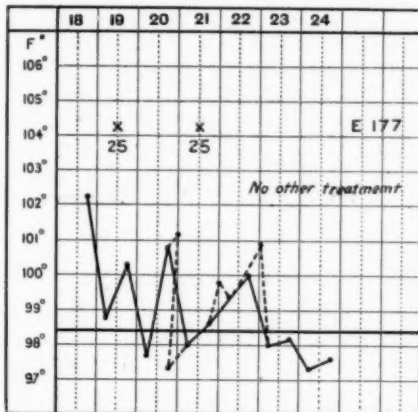


CHART 8 (Case 3).

Day of Disease.

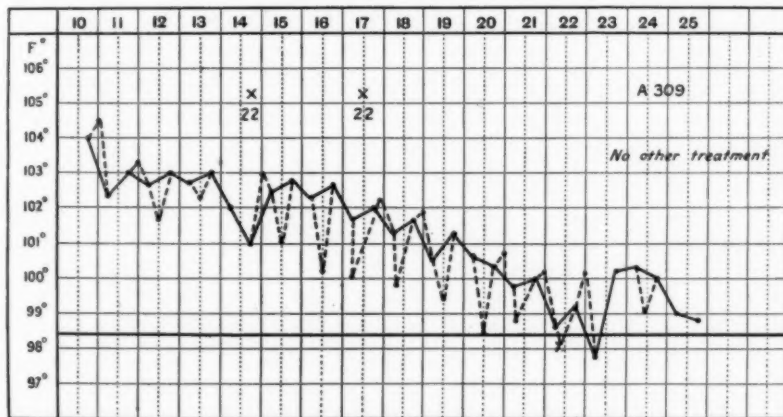


CHART 9 (Case 15).

reached several days after the endotoxic solution was applied, whereas in all the other cases in which the reaction was observed it was never severe, and was all over in twenty-four to thirty-six hours. The

scarlet-fever patients were all convalescent and had no complications at the time.

The occurrence, therefore, of the ophthalmic reaction, when obtained with a 1 per cent. solution of the endotoxin, is not diagnostic of enteric fever, though it is in favour of it.

Note on Five Cases of Typhoid Fever treated with Anti-endotoxic Serum.

By R. M. BRUCE.

THE typhoid serum sent us for trial by Professor Hewlett was used in five cases, of which two were severe and three moderately so. In one of the latter, in which treatment was commenced on the eighth day, the temperature fell to normal on the seventeenth day; in the other two the treatment was commenced on the seventh and eighth days respectively, the temperature falling to normal on the twentieth and twenty-eighth days. In the two severe cases the first injection was given on the fifth day and in the third week respectively, and the fever subsided on the twenty-sixth and twenty-ninth days respectively.

The number of cases treated was, of course, too small upon which to base any reliable conclusion, but my impression is that two at least were benefited by the injections, and that one case, in which the temperature subsided on the seventeenth day, would not improbably have run a more severe and prolonged course if the serum had not been used. I may add (1) that in one case a rash appeared on the fourteenth day after the first injection of the anti-typhoid serum, accompanied by joint pains and a recurrence of fever for five days, but beyond this no ill effects were observed to follow its use; and (2) that in four cases convalescence was uninterrupted. In the remaining case a relapse occurred on the twenty-seventh day, which lasted sixteen days and was followed by a periosteal swelling on the clavicle.

DISCUSSION.

The CHAIRMAN (Dr. Norman Moore) said he was sure it would be the unanimous wish of the Section that he should express their thanks to Professor Hewlett, Dr. Goodall, and Dr. Bruce for the interesting notes which they had contributed. Perhaps Professor Hewlett would kindly give a little more information as to why he did not give any more serum when the patient relapsed.

Dr. McCULLOCH asked whether Dr. Goodall had had any experience with Chantemesse's vaccine, and, if so, how far his results compared with those of the anti-endotoxic serum treatment. Also, would he say whether, with the former serum, there were the untoward symptoms such as pain in the joints and a rash. Sir Almroth Wright, who had visited Professor Chantemesse's wards and had seen the cases under treatment there, had admitted the results achieved were "brilliant," although, as Professor Hewlett had stated, this serum was to be looked upon more as a vaccine in disguise. So-called "vaccine" was not the resultant of the reactive processes to an infection elaborated *in vivo*, and it was not vicariously elaborated, as was an anti-serum. The injection or mucous administration (*per os*, *per rectum*, or *per conjunctivam*) of a suspension of dead micro-organisms doubtless gave rise to the formation of antigens after reaction. He submitted that the term "pro-vaccine" was scientifically more comprehensible when speaking of vaccines.

Dr. LLEWELLYN PHILLIPS asked Dr. Goodall how early in the disease he had tried the ophthalmic reaction, as it seemed to him that if the reaction occurred early it would be beneficial for diagnostic purposes before the Widal reaction occurred. He asked also about hypersensitization. If the serum was given at daily intervals, or after two or three days, was there the same risk of producing hypersensitization as though an interval of a fortnight or three weeks elapsed between the dosages of the serum? Also, had Dr. Goodall any idea as to how long that condition of hypersensitization lasted? It was conceivable that if serum were given prophylactically for diphtheria, and if, after that, diphtheria occurred, a further dose of the serum might have a deleterious effect.

Professor HEWLETT, in reply, said no more serum was given to the patient who relapsed because of the effect known as hypersensitization. If a patient had been treated with serum, and then after the lapse of an interval more serum was given, it sometimes happened that severe symptoms ensued, apparently due to some toxic effects in the serum. Any serum would do it. On that account he did not care to repeat the serum after so long an interval. Speaking generally, so far as he could judge, he had been favourably impressed with the value of the serum. The nine cases referred to seemed to be undoubted cases of typhoid; no case was treated with the serum unless it had all the clinical symptoms and showed the agglutination-reaction.

Dr. GOODALL, in reply to several questions on the subject of supersensitization, said that occasionally very unpleasant effects were produced by a second injection of serum given three weeks, or longer, after the first. He had observed several cases in which a profuse rash, accompanied by a rigor and a high temperature, had come out within a few minutes, or an hour or two, after the second injection. In one case there had also been convulsions. In consequence of the risk of these untoward results he was, at one time, chary of giving a second injection of serum after an interval of three weeks or longer. But during the last three or four years he had found that a second injection had

only very rarely been followed by the symptoms he had mentioned. Why this was he did not know ; but certainly Dr. Cartwright Wood, who was responsible for the serum supplied to the hospitals of the Metropolitan Asylums Board, had succeeded in depriving the serum of most of its noxious qualities. The phenomena of supersensitization were not produced if serum was injected daily even for a long period. An interval of several days, not often less than three weeks, was necessary. On the other hand, the interval might be very long, certainly up to five years. He had no experience of Professor Chantemesse's antityphoid serum. It was not to be obtained in this country, nor, he had heard, in France, except by the personal favour of Professor Chantemesse. From the published account of the cases, it appeared that Chantemesse's serum cut short an attack of enteric fever in a most abrupt fashion. The speaker had not tried the ophthalmic reaction in any patient who had been ill of enteric fever for less than a fortnight.

The CHAIRMAN said the experiment was a very important one, and the result, small as was the number of cases, was a very valuable one indeed, encouraging a further trial of the method.

Medical Section.

June 22, 1909.

Dr. SAMUEL GEE, President of the Section, in the Chair.

Gastro-intestinal Crises from Effusion into the Bowel Wall.

By G. A. SUTHERLAND, M.D.

THE occurrence of unexplained hæmorrhage is more common in childhood than in adult life. Even in the earliest days of life this tendency is present. The site of the hæmorrhage is an important one in determining both the nature of the symptoms and, in many cases, the gravity of the affection. Amongst the different parts of the body which may be affected, the bowel wall is of considerable importance because of the grave and alarming symptoms which may develop and the difficulties in diagnosis and treatment which may confront the physician. A condition of acute abdominal disease is frequently present, and the problem as to whether operative interference is or is not called for would appear to have taxed the diagnostic powers of many. Not infrequently mistakes in diagnosis have been made, as in other forms of acute abdominal disease. In criticizing some of the recorded cases it is hoped that the writer of this paper will not be regarded as assuming a superior position. Such would be untenable, for he has apparently made more mistakes in connexion with this special affection than anyone else. This is his only claim to speak on the subject.

This special type of disease is most frequently associated with the name of Henoch, who was the first to describe it clinically. Henoch's purpura is a variety of the hæmorrhagic diathesis characterized by attacks of severe abdominal pain, vomiting, and the passage of blood and mucus by the bowel. It is only because of the abdominal symptoms, on which he laid special emphasis, that it is to be distinguished from other forms of purpura hæmorrhagica. It is to the occurrence and nature of these acute abdominal attacks in connexion with purpura that I wish to

direct attention. Osler [16] has described a number of such cases in a series of articles on "The Visceral Manifestations of the Erythema Group of Skin Diseases." He specially emphasizes the "gastro-intestinal crises," colic with vomiting and usually diarrhoea, which occurred in most of them. He associates together both serous and sanguineous effusions on the ground that they manifestly proceed from the same cause, as they are found alternating in the same individual during the course of the illness. Clinical evidence will also be found to support the view that sometimes the intestinal effusion is serous and sometimes it is sanguineous. Other writers have described similar attacks as being due to angioneurotic oedema, there being no evidence of hæmorrhage in any part of the body, but definite attacks of fleeting oedema having occurred. Although the pathology of purpura hæmorrhagica is probably very different from that of angioneurotic oedema, effusion into the bowel wall may occur in either affection and produce very similar symptoms. Clinically the abdominal symptoms may be identical, but it may often be possible to make a differential diagnosis from the previous history, or from the evidences of disease in other parts of the body.

Amongst many valuable papers on the subject under consideration may be mentioned those by Scheby Buch [3] on "Joint Affections in Hæmorrhagic Diseases," Guinon and Viellard [10] on "Painful Abdominal Paroxysms in the Course of Purpura," and Emil Döbeli [6] on "Purpura Abdominalis."

The following cases illustrate some of the clinical and post-mortem aspects of this condition:—

Case I.—J. R., aged 3 months, passed a blood-stained motion four days before admission, and since then the bowels had not acted. At the beginning of the illness he was sick, and vomiting had continued quite irrespective of food. He had been crying as if in pain. The abdomen had been increasing in size. His previous health had been good and he had been breast-fed entirely. On admission the child was collapsed and looked moribund. The abdomen was much distended. There were no petechial hæmorrhages. An enema was followed by the discharge of some blood and mucus. Vomiting of a brown-coloured fluid occurred at intervals. A second enema was followed by the passage of some fæcal matter, blood, and mucus. The child seemed too collapsed for any treatment save stimulation, and in spite of this he died a few hours after admission. At the necropsy the only lesion found in the abdomen was in the lower part of the small intestine. The last inch of the ileum was

healthy in colour, but above that there was an area of bowel wall thickened and dark coloured for about 2 in. Above this again the bowel was greatly distended, the walls were thin, and there were no evidences of congestion or hæmorrhage. The colon was collapsed and empty. On opening the bowel the dark-coloured part showed a similar discoloration of the mucous surface and some purpuric spots in the adjoining parts. There was no definite obstruction in any part of the bowel and no hæmorrhage elsewhere. The mesenteric vessels appeared healthy.

Case II (Mr. H. Burrows's case) [4].—A boy, aged 11, was admitted to hospital with a history of violent pain in the abdomen, vomiting which had become stercoraceous, and the passage of blood from the bowel. (It was afterwards learned that he had had previously pains in the limbs and red spots scattered about the body and extremities.) The abdomen was nearly motionless, and the muscles were rigid, especially in the lower part, and there was general tenderness on palpation. There was no distension, or tumour, or tenesmus. Peritonitis was diagnosed. The abdomen was opened and the peritoneum appeared to be healthy. The cæcum was not distended, the appendix was inspected and found to be normal, and no lumps or other abnormalities were discovered. The greater part of the small intestine was empty, but a portion a few inches from the ileo-cæcal valve was found to be congested. The bowel here showed several small petechial hæmorrhages and some irregular patches of congestion from which the blood could be expressed. On the following day a careful search was made for other petechiæ, and some small purpuric spots were found on the backs of the elbows, on the buttocks, and on each leg. Later, a general eruption of purpura ensued, and a diagnosis of Henoch's purpura was made. This was fully confirmed by subsequent attacks of acute abdominal pain with melæna and purpuric eruptions. Recovery eventually took place.

Case III (Dr. Theodore Fisher's case) [8].—A boy, aged 4, was admitted suffering from severe abdominal pain and vomiting. The possibility of intussusception was considered. Under chloroform nothing abnormal was detected. The acute symptoms soon subsided and he left the hospital apparently quite well. Five weeks later the symptoms returned, the pain being severe, and the abdomen tender but not distended. He died collapsed at the end of three days. There had been no melæna. At the autopsy the mucous membrane of the whole length of the small intestine, of the cæcum, and of the ascending colon were intensely engorged with blood, and the intestinal wall was about three

times its normal thickness owing to hæmorrhage into its coats. There was no ulceration. The upper part of the small intestine contained a considerable quantity of blood in clots, and in the ileum and the ascending colon blood was present in a more fluid state. There was no thrombosis or embolism of the mesenteric vessels.

Into the pathology of purpura hæmorrhagica and angioneurotic œdema we need not enter here, but the pathological changes which have been found in the bowel wall during life and after death afford an explanation of the clinical symptoms. Hæmorrhage may take place into the mucous layer, the submucous layer, the muscular layer, or the peritoneal coat. It is probably only when considerable effusion has taken place into the tissues of the bowel wall generally that severe symptoms are produced. The hæmorrhage, if of sufficient extent, produces paralysis in the part of the bowel affected. On examination the bowel wall will be found to be deeply congested or hæmorrhagic in appearance. It is thickened and has a tough, leathery feel. The hæmorrhage in some cases is sharply limited to a small area of the bowel wall, three or four inches in length, and its appearance may be compared to that of an intussusception which has been successfully reduced. In other cases large areas of the small and large intestine may be involved. The thickening of the bowel wall may be so great as almost to occlude the lumen and so produce partial obstruction. Scattered purpuric spots may be found on the peritoneal coat or in the mucous layer of the bowel. At an autopsy one may find many superficial ulcers on the internal surface due to sloughing at the site of these hæmorrhages. In other cases there may be extensive ulceration of the bowel due to profuse hæmorrhage into the wall and necrosis of large areas of the mucous and submucous tissues. The ulceration may extend so deeply as to lead to septic peritonitis, with or without perforation. While hæmorrhage may undoubtedly occur from these small or large ulcers in the later stages of an attack, the early appearance of mælena would suggest that the blood effused into the wall of the bowel had ruptured into the lumen.

Microscopic examination of the bowel wall in the areas affected shows the existence of extensive hæmorrhage. Dr. Emery, who has kindly examined two cases for me, both with localized hæmorrhage, thought there was some evidence of pre-existing inflammation in the bowel wall. The following were the pathological findings on microscopic examination of the affected part of the bowel in two cases: "Inflammatory changes apparently of some standing. Excess of small

round cells separating glands in mucous coat, and to some extent in the submucous and subserous layers. Considerable amount of hæmorrhage, the chief deposit being in the submucous coat, where the blood is intimately mixed with the tissues of the part. Areas of pure blood are also present, unmixed with cells or fibres. The greater portion of the hæmorrhage lies between the muscularis mucosa and the muscular layers, but in some parts it seems to have penetrated the muscularis mucosa and to have infiltrated the connective tissue between the glands. No appearance of hæmorrhage in the solitary follicles." It is interesting to note that Mr. D'Arcy Power found a similar condition to exist in cases of intussusception, *i.e.*, the chief hæmorrhage in the bowel wall was usually into the submucous coat.

The clinical symptoms in connexion with effusion into the bowel wall are vomiting, pain, and the passage of blood and mucus. Constipation is usually present, but in some cases diarrhœa of a severe type is a marked symptom. In the milder cases the attacks are short, but tend to recur. They present the appearance of gastro-intestinal disturbance with colic, but do not suggest acute abdominal disease. It is to be noted that hæmorrhage into the lumen of the bowel does not of itself produce any pain, so that the onset of pain at once suggests effusion into the bowel wall. Pyrexia is not a symptom of the disease, but febrile disturbance may be present from reflex causes or from complications, especially in young subjects.

The pain is of the nature of colic and is intermittent. It may be so severe that the patient rolls about in agony or is thrown into a state of opisthotonos. Pressure on the abdomen either with the hand or by lying on the face seems sometimes to afford relief. The act of vomiting or the passage of some blood and mucus may give temporary relief. In the case of infants violent screaming occurs during the attacks of pain. The tossing and rolling about of the patient is evidence against the presence of an acute inflammatory lesion such as appendicitis or peritonitis. The site of the abdominal pain varies in different cases, but it is usually referred to the umbilical, the epigastric, or the right inguinal region. Abdominal tenderness is not usually present, but some cases are recorded in which it was a marked feature. Such attacks are acute enough and call for the administration of opiates, but between the spasms of pain the patient's appearance is satisfactory. Another stage is reached when the condition becomes one of intestinal obstruction, partial or complete; the whole aspect of the patient changes, and one recognizes the typical appearance of acute abdominal disease.

The nature and cause of the pain have been explained in various ways. According to Scheby Buch the colic may be directly caused by hæmorrhage into the serosa of the intestine. Osler thinks the abdominal pain is associated with a localized effusion—serous or sanguineous—of the gastro-intestinal wall, comparable to an urticarial lesion elsewhere. Both these views assume that the pain is due to swelling and pressure. If, however, one considers the nature of the pain, which is griping, spasmodic, and intermittent, and the usual accompaniment, obstinate constipation, it appears most natural to assume that there is paralysis of the affected part of the bowel with overaction and spasmodic contraction of the bowel wall above. The increased peristalsis is an attempt to overcome what is practically a local obstruction, and pain results. In some cases diarrhoea sets in, usually accompanied by offensive and blood-stained motions, and by a diminution, although not a cessation, of the pain. A sudden attack of violent abdominal pain may be the first sign of illness, and may simulate very closely the onset of acute intussusception.

Vomiting usually comes on at the onset of an abdominal attack or follows soon after; it may become so marked that everything swallowed is promptly rejected. The vomited matter is at first composed of food materials and later may be bilious. Vomiting of blood may also take place, and is a valuable aid in the diagnosis as suggesting the hæmorrhagic nature of the abdominal lesion. Blood and mucus are usually passed *per anum* in hæmorrhagic cases. The amount of blood varies from a mere tinging of the mucus to 6 oz. or 8 oz. The patient may be rendered anæmic by the profuse intestinal hæmorrhage. At the same time blood is not necessarily present, even in hæmorrhagic cases, as the effusion may be confined entirely to the wall of the intestine. In cases of pure serous effusion there will not of course be any blood passed *per anum*. The abdomen is usually retracted in the early stages and milder cases, but with the increase of symptoms of obstruction it tends to become distended. Some thickening of the bowel wall will occasionally be felt, especially if the patient is under an anæsthetic, but as a rule the effusion does not produce a palpable tumour.

The cases as they present themselves clinically may be divided into two classes: (1) Those in which the intestinal attack marks the onset of the illness, and may constitute the whole illness. Curiously enough, it is frequently infants or children who have previously been in the best of health who are suddenly seized with this form of hæmorrhagic disorder. (2) Those in which the intestinal attack occurs in the course of an

illness characterized by hæmorrhages, or serous effusion in other regions. In the latter class the diagnosis will be rendered much easier than in the former. When the abdominal attack is the first symptom the diagnosis will be aided by attention to the following points: (1) A previous history of similar attacks, or of purpura, or of other evidences of a hæmorrhagic diathesis, or of angioneurotic œdema. In a family history of angioneurotic œdema which Osler has reported, all of the cases throughout five generations had colic with the outbreaks of œdema. (2) The presence of purpuric spots; these are frequently unnoticed by the patient, but a careful examination of the whole surface of the body and extremities may reveal their presence. (3) Pain, tenderness, and effusion into various joints occur in a certain number of cases.

The severity of the gastro-intestinal crises in this affection may be estimated by the various diagnoses which have been made in different cases; amongst these may be mentioned appendicitis, peritonitis, acute intestinal obstruction, intussusception, gallstone colic, renal colic, lead colic, and perforated gastric ulcer.

In my own experience the hæmorrhagic type of the affection is much the more common, and, associated as it so often is with hæmorrhages in other parts of the body, with nephritis, arthritis, and endocarditis, one cannot but assume that some infective process is at work. From the point of view of the abdominal symptoms it must also be recognized that a serous effusion into the bowel wall will produce similar gastro-intestinal crises. Attacks of intestinal colic identical in all respects with those accompanied by the passage of blood, but without melæna, are not uncommon. These may be accompanied by œdematous or urticarial swellings in other parts of the body, which are evidence in favour of effusion into the bowel wall. Dr Alexander Don [7] has recorded the case of a boy, aged 11, who was seized with sickness, vomiting, and abdominal pain. There was no melæna, but there was a purpuric rash, and œdematous circumscribed swellings appeared over the back, the thigh and leg, the back of the hand, and the dorsum of the foot. He regarded the case as one of Henoch's purpura associated with angioneurotic œdema. Osler, in discussing the visceral manifestations of the erythema group of skin diseases, says that the colic is probably due to localized œdema of the intestinal wall. Dr. Harrington has had an opportunity of seeing this localized œdema in a case where laparotomy was performed for suspected gallstones.

Case IV (Dr. Harrington's case) [12].—A young woman, aged 26, presented symptoms of gallstones, for which laparotomy was performed.

The intestines were engorged with blood and so red that a mild peritonitis was at first suspected. There were no hæmorrhagic areas in the intestinal walls, but at a point within a short distance of the ileo-cæcal valve a cylindrical enlargement of the ileum $2\frac{1}{2}$ in. long was seen, entirely surrounding the bowel and increasing its circumference to twice the ordinary size. The swelling was evidently in the bowel wall, elastic to touch, and did not pit upon pressure. No other lesion was found. She had a comfortable convalescence. This patient had had many previous attacks of angioneurotic œdema, affecting the face, hands, and feet, accompanied at times with abdominal pain.

It is certainly not very common in childhood to find this serous form of effusion in the bowel wall, so far as one can judge from published cases, and, speaking generally, I think one may say that the younger the patient the more likely is the effusion to be hæmorrhagic.

There would appear to be a special site of election for this form of intestinal hæmorrhage—namely, the ileo-cæcal region. Out of twenty cases in this paper in which the site of hæmorrhage was determined either during life or post mortem, the effusion was marked in the ileo-cæcal region in fifteen cases. The lower end of the ileum, the cæcum, and more rarely the appendix, are the parts affected. It is difficult to explain the special tendency to the effusion in these parts, although the stagnation of the bowel contents about the cæcum may be a cause of local irritation and congestion. The presence of the ileo-cæcal valve may also explain the early development of symptoms of acute obstruction, as effusion and swelling in this narrowed part of the bowel will produce more disturbance than in other parts of the intestine. Certainly the clinical fact remains that in recorded cases a preponderating number show the affection specially marked in this region. It is also a well-known fact that over 70 per cent. of cases of intussusception in infants occur in the ileo-cæcal region. May it not be possible that the sudden effusion of serum or blood in the ileo-cæcal region is often the unsuspected cause of an intussusception? Illustrations of the involvement of the ileo-cæcal region in intestinal effusion are supplied in the cases here recorded. The following two cases show that the vermiform appendix alone may be involved.

Case V (Dr. Jacobson's case) [13].—A woman, aged 37, had suffered for three days from severe pains in the right side of abdomen, associated with some general abdominal disturbance. When first seen the abdominal pain was very severe and tenderness was marked over the

site of the appendix. She complained also of pain in the region of the gall-bladder, and located a third painful point to the right of the umbilicus. On opening the abdomen the bleeding was very profuse. The appendix was five inches long and presented a very red appearance. Later, upon section, it was found to present numerous areas of hæmorrhage into its substance. There was no free bleeding into the cavity of the appendix, which was distended by a mucoid discharge. The hæmorrhage was entirely interstitial. Four days after operation the patient developed a hacking cough and brought up a quantity of bright red blood. This was followed by profuse nasal hæmorrhage, and next day by an outbreak of purpura on the skin. Petechiæ appeared on the left leg above the head of the fibula and on the abdomen, while on the calves numerous ecchymoses were found. At the sites of hypodermic injection free bleeding occurred. There was never any hæmatemesis or melæna. The patient eventually made a good recovery after a somewhat prolonged attack of purpura hæmorrhagica.

For the following case I am indebted to the kindness of Mr. Arthur Edmunds, who has not yet published it.

Case VI (Mr. Arthur Edmunds' case).—A boy, aged 3, was suddenly seized with acute abdominal pain and vomiting while at dinner. For the previous ten days he had had occasional attacks of "stomach-ache." The vomiting was repeated six times during the day, and blood was passed *per anum* on three occasions. On admission the same night an indefinite swelling, cylindrical in shape, was felt half-way between the umbilicus and ensiform cartilage. Intussusception was diagnosed and the abdomen was opened. No tumour was found, but on examining the cæcal region the appendix was found to be hæmorrhagic and was removed. The boy made a good recovery without further symptoms. On examination of the appendix the wall was found to be swollen and infiltrated with blood, involving the submucous layers, the follicles, and the muscular layers. No other lesion was present.

A diagnosis of intussusception is very readily made in cases of intestinal effusion. The resemblance of the two affections may be so striking that on reading the recorded cases I am still unable to find any one point which will serve to distinguish them. The difficulty is specially marked when the intestinal effusion attacks a healthy child, produces obstructive symptoms, and is not accompanied by any effusion or hæmorrhage in other parts of the body. The following cases illustrate the difficulty in diagnosis between intussusception and effusion into the bowel wall.

Case VII.—A. V., a male infant, aged 8 months, was admitted to hospital after three days' illness characterised by vomiting, constipation (complete, save for the passage of some blood and mucus), and great thirst. He took the breast ravenously, but was almost immediately afterwards sick, and the vomiting had become almost continuous. Abdominal distension had been increasing. The child was well nourished but pale. The abdomen was distended and resonant, and nothing abnormal could be detected on palpation combined with rectal examination. A soap-and-water enema was given without any result save a little blood-stained mucus. During the following twenty-four hours the abdomen became more distended, and the vomiting was persistent even after sips of hot water. Crying as if from pain was present, and there was a marked abdominal facies. Another enema was followed by the passage of some faecal matter, blood, and mucus. The abdomen was opened and some free fluid escaped. The small intestines were generally distended. There was no intussusception or peritonitis, but the last few inches of the small intestine were deeply congested and hæmorrhagic in appearance. Above this area were some smaller patches of hæmorrhage in the bowel wall. The abdomen was closed. The symptoms of obstruction were not relieved and vomiting again recurred. A very small amount of faecal matter was passed. The child sank gradually and died two days later. The necropsy revealed a dark, almost black, mass in the right iliac region, which proved to be the cæcum with its walls greatly thickened by an effusion of blood. The lumen was almost completely occluded. The mucous surface of the cæcum was of a uniform deep purple colour, and a similar appearance was present in patches in the first 6 in. of the colon. Here the hæmorrhages were of a purpuric character.

Case VIII.—A boy, aged 5, was seized with severe abdominal pain and vomiting, which continued intermittently for four days and then ceased. Two days later the same symptoms recurred with greater severity and persistence. The bowels had acted irregularly until the day before admission, when the motions were at first blood-stained, and later only pure blood in large amounts was passed. The boy looked ill, and was suffering from almost constant attacks of abdominal pain, in which he rolled about. The vomiting had passed off entirely. The abdomen was extremely distended and resistant, so that an examination was impossible without an anæsthetic. Under chloroform nothing further was discovered. The rectum was empty save for some blood-stained fluid. No faecal matter had been passed for some days. A

diagnosis of intestinal obstruction, probably from intussusception, was made and the abdomen was opened. The sigmoid flexure and part of the descending colon were enormously distended, possibly from paralysis, as no obstruction could be found. On going over the small intestine a part of the bowel about 5 in. long was found which was dark in colour, evidently from extravasated blood, and with thickened walls. Separated about $\frac{1}{2}$ in. from this was a circular band of red colour, evidently a more recent hæmorrhage into the bowel wall. The dark, thickened portion of the bowel felt exactly like an intussusception which had been reduced, and it was thought at the time that this reduction had taken place during the manipulation. During the night following the operation the patient complained at intervals of abdominal pain, but it seemed less severe. The bowels acted three times, the motions being loose and offensive. The further course of the case was that of a severe attack of Henoch's purpura, with profuse melæna and hæmorrhagic nephritis, from which he eventually made a good recovery.

Case IX (Mr. Greig's case) [9].—A boy, aged 9, had an attack of vomiting and diarrhœa, accompanied by pains in the legs. Some blood and mucus was noticed in the motions. A few days later he was suffering from severe abdominal pain, with vomiting and diarrhœa, blood being passed *per rectum*, and tenesmus being present. A few purpuric spots were present on the legs. Intussusception was diagnosed. Under chloroform a small, firm, resistant tumour was felt in the right iliac region, below and to the right of the umbilicus, freely movable and slipping about on manipulation. On opening the abdomen a tumour was easily delivered, and was found to be due to a great congestion of the lower end of the ileum, which projected as a distinct collar into the cæcum. There was no obliteration of the lumen, for the finger could easily be entered through it by inverting the bowel. On the following day the left side of the face was much swollen, and later the swelling became hæmorrhagic. Purpuric spots also appeared on the body generally. He made a good recovery and the abdominal wound healed well.

Case X (Dr. Still's case) [18].—Baby, aged 8 months. Tender swelling about the lower end of the right femur and some scorbutic swelling of the gums. Passing blood and mucus frequently from the bowel. Had vomited twice on the day when first seen. Abdomen flaccid and hollow. In the direction of the transverse colon an elongated sausage-like tumour, firm, and seeming to vary in hardness at intervals during palpation. "Had there been no scurvy I think no one would

have doubted there was an intussusception." Vomiting continued, but the bowels were open. Fluid injection failed to reduce the swelling. Operation postponed, as child's general condition seemed desperate. Under antiscorbutic treatment child improved and tumour gradually disappeared. "I suppose there can be no doubt that in this case there was a mass of blood-clot surrounding the bowel, perhaps in the wall of the bowel."

Case XI (Dr. Porter Parkinson's case) [17].—Male child, aged 5, was admitted with history of four days' illness characterized by severe abdominal pain, vomiting, and melæna. Trunk covered with measly non-purpuric rash. Motions offensive, with blood and mucus. Patient very thirsty and drowsy. On the following day vomiting, blood, and mucus passed, but no proper stool, and great abdominal distension. Intussusception suspected and abdomen opened. No intussusception found, but great distension of the colon, congestion of the descending part, and a few scattered subperitoneal hæmorrhages. Some evidences of recent peritonitis. Vomiting continued, only flatus passed *per rectum*, and a hæmorrhagic rash appeared over the body. Death on second day after operation. At the necropsy general peritonitis with matting of the intestines. On small intestine thirteen extravasations of blood scattered widely, about the size of peas, and projecting on both outer and inner surfaces of the bowel. About a foot from the ileo-cæcal valve a collection of much larger hæmorrhages. Distension of small intestine, and of large, as far as the splenic flexure. At the latter part it was dark coloured from a diffuse hæmorrhage into the wall of the bowel and much thickened. About the middle of this part gut definitely constricted, and beyond it was collapsed and empty. No intussusception present. Many recent hæmorrhagic infarcts in the lungs.

In a recent paper Mr. Hugh Lett lays stress on the presence of a tumour as distinguishing intussusception from this affection. But even a tumour closely simulating invaginated bowel may be present, as in Mr. Greig's and Dr. Still's cases. Further, it cannot be admitted that a tumour is necessarily to be felt in every case of intussusception. I have seen a case in which, at the necropsy, an intussusception was found lying under the ribs on the left side, which could not be felt during life even when the patient was anæsthetised. Tenesmus, opisthotonos, and all the rarer signs of intussusception may be present in cases of hæmorrhage into the bowel wall, so that at present we do not seem to possess any reliable means of distinguishing the two affections. They present themselves as cases of intestinal obstruction, and each one must

be considered in the light of all the symptoms present. Some surgeons say the indication clearly is to open the abdomen and settle the question by inspection. This is probably the best course to pursue in cases where a differential diagnosis is otherwise impossible, where the signs of obstruction are urgent, and where there is no evidence of a general hæmorrhagic tendency. In the latter case it is not advisable to operate, owing to the risk of the wound becoming hæmorrhagic and sloughing.

The diagnosis between intussusception and intestinal effusion is further complicated by the fact that invagination may take place as the result of effusion into the bowel wall. The occurrence of intussusception may be an early or a late complication in the course of an illness characterized by intestinal effusion. This fact is not at present generally recognized, and ignorance of it may lead to the overlooking of a very serious complication in this form of hæmorrhagic trouble. The following cases illustrate the occurrence of intussusception in association with intestinal effusion.

Case XII.—A girl, aged 7, was admitted suffering from Henoch's purpura. She had a prolonged illness, characterized by recurrent attacks of abdominal pain, vomiting of blood, melæna, bleeding from the nose and gums, purpuric eruption, albuminuria and hæmaturia, but was discharged cured at the end of three months. There was a recurrence of the symptoms some months later, and during this attack tenesmus developed and continued irregularly for some days. This was accompanied by the passage of blood from the bowel in larger quantities than before, the amount on one occasion being 6 oz. Thirst and vomiting became marked, but there was no intestinal obstruction, the motions being frequent, loose, and offensive. A severe convulsion occurred, and the girl passed rapidly into an unconscious condition, which lasted until death three days later. The urine and fæces were passed involuntarily, the latter containing blood, and just before death vomiting of stercoraceous matter took place. At the necropsy general septic peritonitis was present, and an intussusception was found composed of the cæcum and part of the ileum. The whole of the intestine involved was much thickened, black, hæmorrhagic, and gangrenous in appearance. The small intestine was distended, and the lower bowel was collapsed and empty, with the exception of the rectum, which contained fæces. The mucous surface of the colon showed a number of clean-cut superficial ulcers. This was a case of intussusception occurring at a late period of the illness, due probably to the effusion of blood into the bowel wall, and was a complication which at that time I was not acquainted with.

Case XIII (Dr. de Havilland Hall's case) [11].—A boy, aged 5, suffered from pains in the limbs, a swelling in the left popliteal space and purpura. At first purpura was diagnosed, and later rheumatic fever. Nine days after the onset pain and tenderness were present in the lower part of the abdomen, followed by vomiting and constipation. After admission two enemas were given and brought away some hard faeces. The child got worse and faecal vomiting came on. There was great abdominal distension, and the child cried out in attacks of pain. The abdomen was opened and an enteric intussusception with a piece of gangrenous intestine was found. Resection was performed. The vomiting continued, and two or three times a considerable quantity of almost black liquid material was vomited. Death followed two days after the operation. There was no necropsy. Commenting on this case, Dr. Hall says the diagnosis made at first was Henoch's purpura, but he thinks this may be rejected because of the faecal vomiting, the presence of constipation, the absence of blood and mucus from the stools, and the existence of an intussusception. As the recorded cases show, there is nothing in these signs which are opposed to a diagnosis of Henoch's purpura complicated by an intussusception.

Case XIV (Mr. Lett's case) [15].—Male, aged 3. There was a history of two days' illness, beginning with pain and swelling in the left knee, followed by swelling of the left elbow, the scrotum, and both legs. Purple watery blisters appeared in the left leg. On the second day recurrent attacks of acute abdominal pain set in, and blood and mucus were passed *per anum*. On admission purpuric spots were present on the ears, legs, and scrotum. A sausage-shaped swelling was present in the right lumbar region. Laparotomy was performed and an ileo-caecal intussusception was found and reduced. After the operation fresh purpuric spots appeared, which became inflamed and sloughed. Diarrhoea with bloody stools set in. Three days after operation abdominal pain was complained of again, the purpuric patches were gangrenous, and the laparotomy wound was purpuric and gangrenous. There was no vomiting. The patient died on the eighth day after operation, and at the autopsy general septic peritonitis was present, with an enteric intussusception 10 in. above the ileo-caecal valve. It was 5 in. long and irreducible.

Mr. Lett's case is particularly valuable because of the occurrence of a second intussusception in the course of an attack of Henoch's purpura. The first one was ileo-caecal and was successfully treated by operation. The second was enteric, and was situated 10 in. from

the ileo-caecal valve. The vomiting and pain ceased immediately after the operation, the bowels acted, and the child was free from all signs of intussusception for seven days. Unfortunately, when the second intussusception occurred the child's condition was such that any further operation was out of the question. In this case Mr. Lett has brought forward the most conclusive evidence yet adduced of the causal connexion between Henoch's purpura and intussusception.

It is not difficult to understand why there should be a tendency to intussusception as the result of effusion into the bowel wall. An effusion causes a temporary paralysis in the part of the bowel wall affected, and more or less complete paralytic obstruction follows, with vomiting, abdominal pain, and the passage of blood and mucus. To overcome the obstruction violent peristaltic movements occur in the part of the bowel immediately above, which is driven into the flaccid bowel and an invagination is formed.

Some cases of this affection seem to lead up very readily to a diagnosis of spontaneous reduction of an intussusception. In former years I have accepted this diagnosis without hesitation, but further experience has led me to doubt whether it is justified as often as it is made. The invagination of one part of the bowel into another is a pathological condition which, when once started, tends to increase very easily, as the course of the affection shows. But the reverse process, the means by which an invaginated portion of bowel can free itself, is not at all clear or easily understood. The late Mr. Barnard wrote that cure of an intussusception may take place "by spontaneous reduction, a phenomenon several times recorded but always dubious" [2].

In cases which are not submitted to operation, and which recover, the diagnosis must be a matter of uncertainty, but the evidence of effusion into the bowel wall is, in many cases at least, as strong as that of the spontaneous reduction of an intussusception. The sudden onset of abdominal pain in a previously healthy infant or child, along with vomiting and the passage of blood and mucus from the bowel, certainly suggests intussusception; but the subsidence of all symptoms in from twenty-four to forty-eight hours is equally suggestive of effusion into the bowel wall. Discussing this type of case, Mr. H. S. Clogg [5] writes as follows:—

"In two cases I have recently seen, both healthy babies, aged 3 and 6 months, both were reported as becoming suddenly ill, crying with pain, turning pale, and vomiting. They both passed blood and

slime by the bowel. In both cases the history was very suggestive of an intussusception, and the aspect of the infants confirmed this suspicion. The abdomen could be easily palpated and nothing was felt. So strong was the history that I examined under an anæsthetic, still with a negative result. In these two cases I could feel nothing. As both babies were a little collapsed, rectal salines were given. This brought away some further blood and mucus, and in twenty-four hours the babies were perfectly well. It is hardly possible to regard such cases as colitis; personally I should consider them as cases of intussusception reduced spontaneously."

Case XV (Dr. Still's case) [19].—Female infant, aged 4 months. Two days previously, suddenly began to scream and continued for two hours. Then vomiting and passage of blood and mucus *per anum*, without fæcal matter. Frequent attacks of abdominal pain. On the day she was seen the bowels had acted twice, once only blood and mucus had passed, and the second time the same with a trace of green fæces. Only slight vomiting during previous eighteen hours. No tumour felt. An enema of warm olive oil brought away a large quantity of mucus, but no fæcal matter. The symptoms steadily diminished and recovery followed. In this case an intussusception had been carefully looked for but none found.

These three cases may be regarded as typical of a class in which a diagnosis of spontaneous reduction of an intussusception is commonly made. On the clinical evidence, however, one can only assert that there were symptoms of obstruction of the bowel with melæna; and these may equally well have been produced by effusion into the bowel wall with temporary paralysis. More precise information is gained when the abdomen is opened and the condition of the bowel can be examined. Even under such circumstances if one is inclined to a diagnosis of spontaneous reduction of an intussusception, it is very easy to find confirmatory, if not conclusive, evidence. The condition of the bowel wall, congested or hæmorrhagic and thickened by the effusion, is very similar to that of a mechanically reduced intussusception, and the surgeon very naturally assumes that nature has simply anticipated his procedure. In a certain number of cases the further progress of the disease will show clearly that it was quite unnecessary to assume the existence of an intussusception (*see Case VIII*). The following case I accepted at the time as a clear case of spontaneous reduction of an intussusception, but now I think it may have been a sudden effusion into the bowel wall.

Case XVI.—W. M., male, aged 4. Quite well until 3 p.m. (four hours before admission), when he was suddenly seized with acute abdominal pain. Has vomited three times. Has seemed drowsy and has been having attacks of pain about every five minutes, when he rolls about in agony. Frequent desire to defæcate and straining, but nothing passed. The boy is well nourished, but lies in a drowsy condition; looks collapsed, eyes sunken, slightly sick occasionally. Frequent attacks of abdominal pain, in which he screams, tosses about, and presses both hands to umbilical region (to which pain is referred); then passes into a state of opisthotonos, with only back of head and heels resting on the bed. As pain subsides the opisthotonos passes off. Abdomen is soft, swollen, not generally tender. A hard, tender lump, about the size of a hen's egg, is felt just above and to the right of the umbilicus, and firm pressure here elicits a paroxysm of pain. At 10 p.m. laparotomy. There was no evidence of peritonitis. On following up the small intestine which presented, the last 3 in. of the ileum were found to be congested and had a thick, solid, leathery feel. This was regarded as an intussusception which had been reduced in the process of manipulation. No other evidence of disease found. Boy made an uneventful recovery without pain. There was at no time any blood in motions.

In a recent thesis for the degree of M.D., Mr. Duncan Fitzwilliams has reviewed the records of a thousand cases of intussusception, and gives the following examples of spontaneous reduction as evidenced at operation.

Case XVII (Mr. Fitzwilliams's case).—A male child, aged 3, with a typical history of thirty hours' duration. No blood had been passed, but a tumour was easily felt on the right side of the abdomen. At the operation no invagination was found, but the lower 2 in. of the ileum and the cæcum, with the adjoining mesentery, were congested and showed subserous hæmorrhages.

Case XVIII (Mr. Fitzwilliams's case).—A male infant, aged 10 months, had an indefinite history of illness for eight days and had been acutely ill for about ten hours. There had been spasmodic pain and screaming, and blood had been passed *per anum*. A tumour was easily felt. At the operation the tumour was found to be reduced, but numerous fresh hæmorrhages and petechiæ were seen on the bowel. One of the extravasations of blood was the size of a halfpenny.

Here again the evidence of a previous intussusception does not appear to be conclusive. Mr. Fitzwilliams adds a third case which had been under the care of Mr. Kellock and published by him.

Case XIX (Mr. Kellock's case) [14].—Male infant, aged 18 months; admitted with a history of two days' illness characterized by acute abdominal pain, vomiting, and the passage of blood and slime mixed with faecal matter. Cessation of symptoms for twenty-four hours and then recurrence. On admission he was evidently under the influence of some anodyne. Nothing abnormal on abdominal or rectal examination under anaesthesia. Soon after admission passed a stool containing a good deal of blood. Laparotomy. No tumour. The lower 7 in. or 8 in. of the ileum were quite collapsed and of a dull reddish brown colour, with one or two small hæmorrhages in the wall. Bowel above and below appeared healthy. Several glands in mesentery of affected part of bowel as large as green peas, and a small depression near upper end of collapsed part surrounded by a raised edge of inflammatory thickening. Rapid and complete recovery.

In this case there may or may not have been an intussusception previous to the operation. Mr. Kellock refers to the influence of the anodyne in this case as aiding the reduction by quieting peristalsis. It certainly is a striking fact that in many of the cases described as spontaneous reduction the symptoms had persisted right up to the administration of the anaesthetic. One cannot help thinking that if reduction of the intussusception did occur it was not exactly spontaneous, but due to the influence of the anaesthetic in checking the excessive peristalsis or spasm, and allowing the muscular action of the bowel to come into play in relieving itself.

Cases described as chronic or relapsing intussusception frequently show little conclusive evidence of that affection, and, so far as the symptoms go, may equally well be referred to a condition of effusion into the bowel wall. Without denying the existence of chronic intussusception, of which I have seen undoubted examples, I think that some of the cases so diagnosed are capable of a different interpretation. The following two cases appear to me to have been primarily due to hæmorrhagic effusion into the bowel wall.

Case XX (Dr. G. F. Still's case) [20].—Two days before admission a boy, aged 2, was seized with colicky pain in the abdomen, and passed a considerable amount of blood *per anum* on two occasions; vomiting also set in and had continued, while the bowels had not acted. Eight months previously there had been a similar attack, with the passage of much blood, and intussusception had been diagnosed at another hospital, where the patient was considered too ill to stand an operation; gradual recovery had followed. On admission a hard mass was felt in the

adjoining parts of the transverse and descending colon, which seemed to harden and soften. On rectal examination some bright red blood came away. The patient looked extremely ill, with sunken eyes, mucus on the corneæ, &c., and operation was not considered advisable. The provisional diagnosis was intussusception, although Henoch's purpura was also considered; gradual recovery took place, with disappearance of the abdominal tumour. Three weeks later came a recurrence of the symptoms, and a tumour was again palpable in the same situation; the child sank rapidly, and died three days later. At the autopsy there was suppurative peritonitis and a large intussusception in the site of the tumour felt during life; the layers were only slightly adherent, so that the intussusception had probably only recurred quite recently. Dr. Still regarded the case as one of relapsing intussusception.

Case XXI (Dr. Emil Döbeli's case) [6].—A boy, aged 8, was seized with acute abdominal pain and constipation; perityphlitis was diagnosed; the colicky attacks continued, diarrhoea set in, and pure blood was passed *per anum*. Eight days after the onset purpuric spots appeared on the legs, which later became general and were accompanied by swelling and tenderness in many joints. From the age of 3 the boy had suffered from similar attacks of colicky pain, which had been diagnosed as due to lead poisoning, and from joint pains, which had been regarded as rheumatic. On admission into hospital, three weeks after the onset, the boy was wasted and very anæmic, purpuric spots were present on the trunk and extremities, and the eyelids and feet were œdematous. The abdomen was retracted, and a tender swelling was palpable in the sigmoid region; this tumour lay in the line of the colon, was firm and irregular, movable, and apparently immediately under the abdominal wall; no hardening and relaxing could be felt. A diagnosis of ileus was made from the severe colicky pains, which were intermittent, the palpable tumour, and the passage of blood and mucus *per anum*. In order to remove any fæcal accumulation high enemata of oil were given, which brought away much lumpy fæcal matter; relief from acute symptoms followed. A week later came a recurrence of the purpura, œdematous swellings, and abdominal pain; the tumour was again felt, larger than before, while under palpation its shape seemed to alter. The diagnosis of ileus from coprostasis was now changed to ileus from chronic or relapsing intussusception of the sigmoid flexure. As the attacks of pain and purpura continued the patient was referred to a surgeon for operation. The surgeon could not discover any tumour, either by abdominal or rectal examination. There was no recurrence

of the abdominal pain, but hæmaturia set in and fresh purpuric spots appeared at intervals. The final diagnosis was chronic intussusception from chronic constipation, with secondary purpura. The patient eventually made a complete recovery.

There does not appear to be any necessity to assume intussusception in either of these cases. In Dr. Still's case there was intussusception, the autopsy showing evidence of a recent invagination, but the passage of blood from the bowel had been going on at intervals for eight months. Pure blood in large quantity is not usually passed in cases of intussusception, and far less in the chronic form where the circulation in the invaginated portion is not seriously interfered with. Consequently from the history of severe melæna in Dr. Still's case it may be suggested that the primary trouble was gastro-intestinal hæmorrhage, and that intussusception occurred shortly before death, as in the cases already recorded.

An examination of the histories in cases of spontaneous reduction of an intussusception and of chronic intussusception shows that the clinical signs may closely resemble those due to intestinal effusion. In favour of the latter diagnosis will be: (1) The passage of pure blood *per anum* in considerable quantity, and at intervals of some hours or days. (2) A history of similar attacks previously. (3) A hæmorrhagic appearance of the bowel wall away from the supposed site of invagination. If one sees at an operation purpuric spots scattered about the peritoneal surface of the bowel and the mesentery, there is strong presumptive evidence of the presence of hæmorrhagic disease. (4) The recurrence of melæna after operation and supposed reduction. (5) Purpura and hæmorrhages about the skin, or œdematous swellings in different parts of the body.

This form of intestinal hæmorrhage may simulate certain affections of the colon, such as mucous colitis, acute ileo-colitis, and ulcerative colitis. In fatal cases there may be found extensive ulceration of the colon due to the sloughing of the mucous membrane at the hæmorrhagic areas, which are often diffused widely over the bowel.

Case XXII.—A female child, aged 3, was seized with sudden pain in the left side of the abdomen, and passed two motions containing blood and mucus. The temperature rose to 103·4° F. There was frequency of micturition and tenderness over the descending colon. During the next twenty-four hours she passed on five occasions a large quantity of blood and mucus, and the attacks of pain were sharp and intermittent; the temperature then fell to normal. On the third day there was a good deal of mucus passed, with only a trace of blood,

and faecal matter was present. Some thickening was felt in the left inguinal region, but nothing abnormal, on rectal examination. The symptoms rapidly passed off, and there was no recurrence or evidence of hæmorrhage elsewhere.

This case may be variously interpreted, but from the attacks of pain I was led to believe that hæmorrhage had taken place into the bowel wall. I have seen similar symptoms in a young woman who was said to suffer from mucous colitis. In one of these attacks, when she was under observation, she passed large quantities of blood and mucus by the bowel, and suffered from intermittent and colicky pain in the region of the colon. At other times she suffered from attacks of angioneurotic oedema, the whole face becoming swollen and oedematous.

Case XXIII (Dr. Ashby's case) [1].—A boy, aged 9, was suddenly seized with pain in the abdomen whilst at school, followed by the passage of blood and mucus by the bowel; the symptoms continued during the night. After admission next day he passed in the course of twenty-four hours twelve stools consisting almost entirely of blood and mucus. Tenesmus and bloody stools continued, and large enemata brought away but a small quantity of faecal matter. The abdomen was not distended or tender and nothing could be felt on palpation in the way of a tumour. It was resolved to open the abdomen in order to relieve an invagination of the bowel if present. This was done, but only an intensely congested colon was found. Death followed eight hours later. At the necropsy the stomach and small intestines, to within 20 in. of the cæcum, were found normal; the last foot or two of ileum was found congested, with patches of thin membranous exudation. The mucous membrane of the colon, sigmoid flexure, and rectum was intensely injected, the changes in the lowest parts being most marked, and the rectum being hæmorrhagic. There was no ulceration. (This case was recorded by the late Dr. Ashby in his standard work as one of "acute ileo-colitis," but he wrote me that he had come to the conclusion that it was really an example of Henoch's purpura of unusual severity.)

Ulcerative colitis is not a common affection in early life, and some of the recorded cases seem rather to fall into the category of the affection we are considering. The post-mortem evidence is by no means conclusive, for it may not settle the question as to whether the ulceration was primary and the hæmorrhage secondary, or *vice versa*. In cases of Henoch's purpura I have seen definite ulceration of the colon which was clearly secondary to hæmorrhage into and sloughing of the mucous membrane. The presence of other hæmorrhages in the bowel wall is

always suggestive of a primary hæmorrhagic affection and secondary ulceration. A diagnosis of acute dysentery is also sometimes made when the clinical course of the disease can be fully explained by a condition of extensive hæmorrhagic effusion into the wall and the lumen of the bowel.

It is difficult to bring forward any evidence from the textbooks of the gravity and importance of these gastro-intestinal crises from hæmorrhage because the condition is generally ignored, and any examples are usually to be found classed under some other disorder. Without adding anything fresh, I have tried to show by collecting a number of recorded cases that the condition is one of considerable importance, and that there are various problems connected with it about which we require more information before any dogmatic statements can be made.

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DISCUSSION.

Mr. EDMUNDS exhibited a sketch of the appendix which Dr. Sutherland referred to, in which the symptoms were typically those of intussusception, and in which he found, on opening the abdomen, that the greater part of the end of the appendix was intensely infiltrated with blood. There was a little blood in the lumen and, as evidence of old inflammation, a small adhesion at the tip. The hæmorrhage had also extended into the meso-appendix. He next showed a picture representing a vertical section through the appendix wall, indicating fairly diffuse hæmorrhage into the centre of the follicles, and the submucous tissue. It also extended into the subperitoneal tissue. He believed that where there was greatest vascularity there was greatest hæmorrhage. The

next picture showed the usual condition after reducing an intussusception, in which the bowel wall, the appendix, or any other structure which had formed the intussusception became acutely congested and had an appearance similar to that which Dr. Sutherland had been describing as a spontaneous reduction of an intussusception. The bowel was not enormously swollen, and on the ante-mesenteric border there was a spoon-shaped depression. He had drawings of two cases which recently came under his notice, in which he believed he had absolutely conclusive evidence of spontaneous reduction of intussusception, with singularly little hæmorrhage into the bowel wall. He next showed a picture of a case on which he operated a month ago, in which one could see, in the same position, a spoon-shaped depression, surrounded by an œdematous area of bowel wall, lighter in colour than the rest of the intestine: the pale lemon colour represented very fairly the semi-transparent look of the actual specimen. It might be contended that that was simply a localized effusion into the bowel wall, but the two following drawings would explain that. Recently he had a child, aged 10 months, who had had all the typical clinical symptoms of intussusception, and in addition the house physician and house surgeon at the hospital had satisfied themselves that they had felt a large tumour. *Per rectum* the tumour could be felt bulging through the wall of the rectum, but not bulging into its lumen. He found a small indefinite tumour right up under the costal margin, but, as two others had felt a tumour, he opened the abdomen. He then found that all the intussusception except the last small piece had been reduced, and at the end of the cæcum there was the same depression; when he expressed this there was the same sort of condition as in the other case, *i.e.*, the small piece of intestine which had formed the apex of the intussusception had become acutely œdematous and semi-transparent. There was a ring of congestion round it, doubtless the site from which the hæmorrhage had occurred. These cases seemed to show that Dr. Sutherland was right in saying that surgeons did operate on hæmorrhagic appendicitis, or hæmorrhage into the bowel, under the impression that there was an intussusception, and also substantiated his caution in not denying that intussusceptions did occasionally reduce themselves.

Mr. HUGH LETT said he had been much interested in hearing Mr. Edmunds's remarks about spontaneous reduction, and the last case he showed seemed to be absolute proof that an intussusception might be spontaneously reduced. No doubt many cases had been seen in which the intussusception, having been at one time in the left hypochondrium, finally passed back again to the right side of the abdomen, and perhaps down to the right lumbar region, but complete spontaneous reduction was very rare. The differential diagnosis between Henoch's purpura and intussusception might be very difficult. The age of the patient might afford some assistance, for though Henoch's purpura occurred in infants, it was more frequently met with in older children and adults. Intussusception occurred largely in children under one year of age; indeed 72 per cent. occurred in that period. Fitzwilliams, out of 650 cases, found that only 6 per cent. occurred in children from 6 to 12 years of age. With regard to the question of a tumour, he thought that its presence was of

great importance, though not absolutely diagnostic of intussusception. For the tumour might be caused by a simple thickening of the intestinal wall from hæmorrhage into it, as in the case which was published by Dr. Greig. In that case the tumour was a small one, the ileum projected as a collar into the cæcum. In another case there was hæmorrhage into the mesentery; in another, coagulated blood inside the cæcum caused a tumour which was the occasion of the operation. With regard to the frequency with which a tumour could be felt in intussusception, he had operated upon thirty cases of the condition, and in every case had been able to feel a tumour before operation, with or without an anæsthetic. He agreed that in older subjects, particularly young adults, it might be very difficult, if not impossible, to feel a tumour in some cases of intussusception, but he thought that in the vast majority of cases of intussusception in infants and young children a tumour could be felt. Mr. Clogg had recently published a series of cases of intussusception, and stated that a tumour was felt before operation in fifteen out of sixteen cases. The healing of the laparotomy wound might give rise to considerable anxiety. In his own case the wound became purpuric and sloughed, so that the intestines were seen at the bottom of the wound. In another case the child died from hæmorrhage from the wound. If, in a case of possible Henoch's purpura where intussusception was suspected, a characteristic tumour could be felt, and the symptoms were sufficiently suggestive of intussusception, he would not hesitate to explore the abdomen. On the other hand, in a case which might be Henoch's purpura, or might be intussusception, where the abdominal symptoms were severe, with the passage of blood and mucus from the anus, he would not feel inclined to operate, having in view the possible serious condition of the wound subsequently, unless a tumour could be felt.

Dr. SUTHERLAND, in reply, said he had been very glad to hear Mr. Lett's remarks regarding the proper operative procedure in such cases. Physicians sometimes had to diagnose the cases for the surgeon, as the physician saw the case first, and so the question as to the frequency with which a tumour was palpable was very interesting. There was a good deal of difference of opinion on that point. Professor Henoch said he had never been able to feel a tumour in cases of intussusception, but Mr. Lett's experience was that nineteen out of twenty cases showed a palpable tumour. He had also been much interested in seeing Mr. Edmunds's drawings, and hearing his views concerning spontaneous reduction of an intussusception; because, although he did not deny the occurrence of such an event, the evidence in the literature did not seem to be very convincing. Mr. Edmunds had brought forward ocular evidence of the condition of the parts, and in addition to showing that an intussusception might be reduced spontaneously, he had enabled one to distinguish between the condition of effusion into the bowel wall and pre-existing intussusception. He would look out in future for such a definite area of pallid œdema in the coagulated bowel wall as Mr. Edmunds had shown. If that were found he would be prepared to admit that it was spontaneous reduction. If, on the other hand, one found only a uniformly congested area of bowel wall, he would be inclined to say it was a case of intestinal effusion.

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

Neurological Section.

October 29, 1908.

Dr. CHARLES E. BEEVOR, President of the Section, in the Chair.

Two Anomalous Cases of Syringomyelia.

By GORDON HOLMES, M.D., and R. FOSTER KENNEDY, M.B.

THE two cases of syringomyelia which are the subject of the present communication were of considerable interest from both the clinical and pathological points of view. In neither of them was the disease diagnosed during life, and in both complicating conditions were found in the central nervous system on post-mortem examination.

Although a satisfactory pathological definition of syringomyelia can be scarcely given, and although many cases are unjustifiably described under this title, it seems to us that the lesions present in our cases were so characteristic that they must be included in this category.

CASE I—SYRINGOMYELIA AND SYRINGOBULBIA ASSOCIATED WITH SYPHILITIC PACHYMEINGITIS.

J. S., a journalist, aged 43, was admitted to the National Hospital in September, 1907, complaining of spasticity of both legs and diplopia. At the age of 18 he contracted syphilis. Secondary symptoms were marked. Three years later he developed double hemiplegia with optic neuritis, but recovered rapidly under antisypilitic treatment. Thereafter his health remained fairly good until some years ago, when he began to complain of unsteadiness of gait, especially when in the dark. In the four years before admission these symptoms progressed rapidly and the legs became very spastic. For two years there had been diplopia and some failure of vision; some ataxia of the arms was also

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noticed. He had been repeatedly under treatment; by an eminent French neurologist the condition was diagnosed as "tabes combinée."

On examination vision was found to be reduced to $\frac{6}{18}$, and the optic discs were in a condition of post-papillitic atrophy without swelling. There were diplopia and nystagmus in looking in all directions; the pupils were small and reacted feebly to light. Beyond ataxia of finer movements there was no defect in the upper extremities. The legs were very spastic and their power was feeble for all movements. Owing to spasticity and ataxia the patient was unable to walk without help; Romberg's sign was well marked. A relative analgesia was found over the ulnar sides of both arms and on the upper part of the trunk. Tactile sensibility was less diminished over the same areas, and thermal sensibility, as far as it was tested, was intact. Joint sense and muscle pain sense were also unimpaired. There was no history of pain or other paræsthesiæ. The deep reflexes were much exaggerated, and both plantar responses were of the extensor type.

The patient died in an attack of acute pneumonia three weeks after leaving hospital.

Post-mortem Examination.—The soft membranes at the base of the brain were opaque and thickened. The spinal cord appeared shrunken and flattened; the membranes over the cervical and upper dorsal segments were greatly thickened, and in places the dura mater and pia-arachnoid were firmly bound down to the surface of the cord. On section of the cord a cavity was seen in its centre extending from the mid-lumbar to the first cervical segment, and thence into the medulla.

Microscopical Examination.—The thickening of the spinal meninges is greatest over the cervical enlargement and in the middle and lower dorsal regions. The thickening is chiefly due to dense fibrotic tissue, with here and there infiltration, especially perivascular, by small round-cells and plasma-cells. There is also considerable intimal proliferation in some of the vessels, and the walls of many of the others are hyaline. In the lowest spinal segments there is only secondary degeneration of the crossed pyramidal tracts, but in the fourth lumbar segment an area of dense gliosis appears in the left dorsal horn and in the grey commissure. In the third lumbar segment the cavity commences by rarefaction in the centre of the gliosis; and in the first and second lumbar segments it occupies the grey commissure, the ventral portion of the dorsal columns, and practically the whole of the left dorsal horn. Its walls are composed of dense neuroglial tissue

with spider cells. In the dorsal segments both dorsal horns, the commissure and part of the dorsal columns are involved by the cavity; the central canal is represented by an irregular column of ependymal cells everywhere separate from the pathological cavity. In the lowest two cervical segments there are two cavities, one in each dorsal horn, separated by the gliotic tissue which surrounds the central canal. In the fifth segment (fig. 1) communication between the cavities is again established across the commissure and the ventral part of the dorsal columns; even at this level the pyramidal tracts are severely degenerated, and there is considerable degeneration in the dorsal and ventral spino-cerebellar tracts. In the lowest portion of the medulla an excess

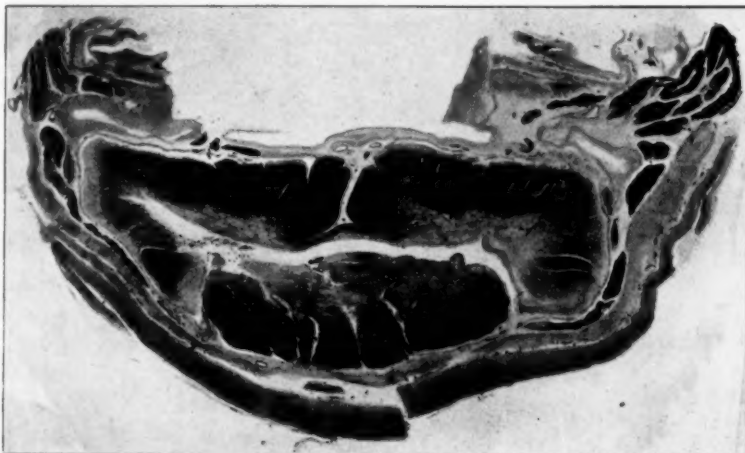


FIG. 1.

To show the distribution of the cavity in the fifth cervical segment and the extent of the meningitis which surrounds the cord. Owing to the presence of calcareous deposits in it, it was necessary to cut away the membranes on the ventral surface of the cord before a section could be obtained of it.

of sclerotic neuroglia still surrounds the central canal, and from its dorsal portion two slit-like cavities extend ventro-lateralwards towards the ventral margin of the spinal fifth root, running parallel but slightly ventral to the outgoing vagus roots (fig. 2). The disease ceases completely in about the upper third of the medulla oblongata.

There was also marked hydrocephalic dilatation of the lateral

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ventricles, in all probability the result of occlusion of the foramen of Majendie by meningitis.

On consideration of the history of the patient and of the result of microscopic examination there can be little doubt that the spinal pachymeningitis was of syphilitic origin. On the other hand, the gliosis and cavity formation in the cord and medulla oblongata are characteristic of syringomyelia. To explain the incidence of these two processes, the meningeal and the syringomyelic, in the same case, various theories may be put forward, but the most probable is that the spinal disease was secondary to the meningitis and due in some way to compression of the cord or to interference with its blood or lymph circulation.

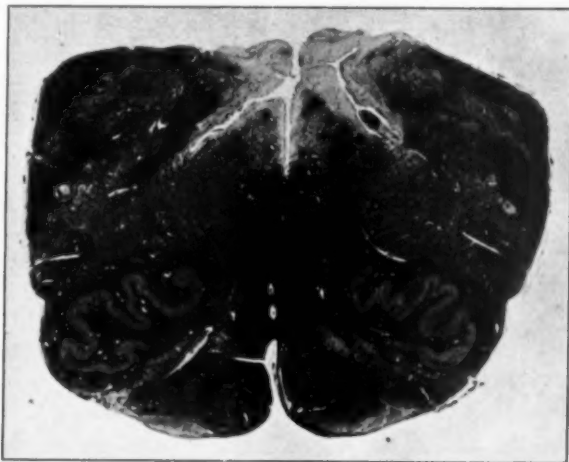


FIG. 2.

A section of the lower part of the medulla, to show the gliosis in the floor of the fourth ventricle and the cavities which extend ventro-lateralwards from it, parallel to the vagus roots.

CASE II—SYRINGOMYELIA WITHOUT SYMPTOMS ASSOCIATED WITH INTRACRANIAL AND SPINAL TUMOURS.

A. N., aged 46, was admitted to the National Hospital in July, 1905, complaining of headache and loss of sight. Besides numerous epileptiform convulsions, he had had for some months preceding admission true dreamy mental states unassociated with any convulsive seizures.

On examination there was almost complete loss of vision, with post-neuritic atrophy of both discs. Beyond slight tremor in the hands no abnormality was found in the upper extremities; all movements of the lower limbs were well performed and gait was perfectly normal. Romberg's sign was not present. The sensory system and the reflexes were natural.

Death occurred after the removal of a circumscribed tumour from the right temporo-sphenoidal lobe; on post-mortem examination it was found that in addition to the tumour which had been removed there were several others, all of which were circumscribed and firmly attached to the cerebral dura mater; they compressed but did not invade the brain. Histologically all these growths proved to be of identical nature and conformed to the structure of the psammomata; they were rich in nuclei, and throughout large areas the cells were arranged in concentric whorls, which in places had undergone hyaline degeneration, with frequent deposition of calcareous material within them; their structure was entirely distinct from that of the tumour to be described later in the spinal cord. The membranes of the cord were normal, but the cord itself was considerably enlarged in the mid-dorsal region; after hardening in formalin, sections from each segment were examined by various methods.

In the eighth cervical segment dense neuroglia appears round the central canal, and extends thence into the dorsal columns and into each dorsal horn; as yet there is no cavity formation. There is considerable dilatation of the central canal in the first dorsal segment; in the second and third the distension is still greater and the ependymal lining is less complete. A second cavity appears here in the right dorsal horn; it is evidently the result of rarefaction of a pre-existing gliosis.

In the fourth and fifth dorsal segments the cavity is much larger (fig. 3), and in the latter the central canal is almost obliterated. In the sixth dorsal segment a mass of dense neuroglia occupies the position in which the larger cavity lay in the section above; the cavity is reduced to an irregular slit, while the central canal is represented merely by a solid column of ependymal cells in its ventral wall.

The cavity ceases between the seventh and eighth dorsal segments, and there then appears the large central tumour, which was visible to the naked eye. From a study of serial sections of this region it is evident that the tumour arises in connexion with, and apparently from, the proliferated ependymal cells of the central canal. In the eighth dorsal segment the growth occupies the entire centre of the cord, and in the

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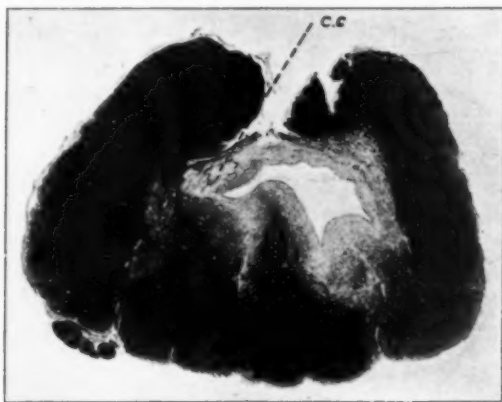


FIG. 3.

A section of the fourth dorsal segment, showing the syringomyelic cavity surrounded by gliosis, and, c.c., the dilated central canal, which is lined by ependyma.

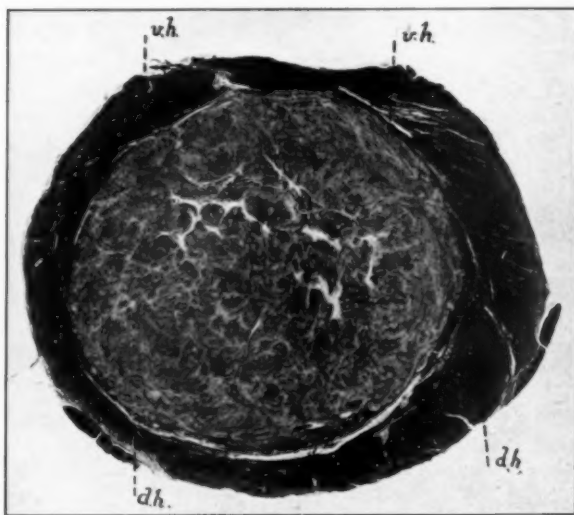


FIG. 4.

A section of the ninth dorsal segment, to illustrate the size of the central tumour; v.h. marks the position of the ventral horns and d.h. that of the dorsal horns.

next segment its cross section is found to be 113 sq. mm., while that of the whole cord is 154 sq. mm. (fig. 4). Surrounding this circular tumour there is a band of compressed, but intact, nervous tissue, varying from 1 mm. to 2 mm. in thickness. The tumour ends rather abruptly in the middle of the tenth dorsal segment, below which there are no signs of disease in either the white or grey matter. Histologically the tumour was undoubtedly a glioma, which had arisen from the ependymal cells of the central canal.

Thus in the cord were found two conditions distinct from, though possibly related to, one another; on the one hand an extensive primary gliosis with cavity formation which is typical, as regards position and structure, of syringomyelia; and on the other hand a large tumour, which was undoubtedly a glioma of ependymal origin.

It appears improbable that in this case the tumour was an etiological factor in the development of the syringomyelia, more particularly as from its structure it appears more recent than the gliosis which surrounds the cavity. More probably the two conditions arose independently of one another, though possibly dependent upon a common developmental anomaly; the existence of tumours in other tissues favours the view that there was some inherent bioplastic defect in the central nervous system and the membranes.

From the clinical point of view the complete absence of all symptoms and physical signs referable to a spinal lesion is remarkable, but it may be again pointed out that the spinal tumour displaced and compressed the nervous tissue without destroying it, and that it had not produced either ascending or descending secondary degeneration.

We are indebted to Sir William Gowers and to Dr. Ormerod for permission to make use of the clinical notes of these cases.

Sequel to a Case of "Cerebellar Atrophy."

By FREDERICK E. BATTEN, M.D.

THIS case was shown before the Clinical Section of the Royal Society of Medicine on October 11, 1907,¹ as a case presenting symptoms which I thought indicated cerebellar atrophy. The diagnosis was based on a gradual oncoming and increasing ataxia of a cerebellar type, affecting the lower limbs to a greater extent than the upper, associated with a lateral nystagmus, with a complete absence of all signs pointing either to a spinal or cerebral lesion.

The patient was kept for nine weeks under observation, and at the end of that time was in the same condition as when first seen. He left the hospital and died three months later. Fortunately a post-mortem was obtained and Dr. Holmes kindly performed the autopsy, which has proved negative both macroscopically and microscopically. It is possible to assume that the condition was purely functional, but the general symptoms exhibited by the patient did not suggest functional disease. His muscular power was strikingly good. His memory, cerebration, and mental condition were excellent, and he was an exceedingly good witness. Clinically I have never before seen the symptom-complex presented by this patient, and although, as the post-mortem has shown, I was wrong in diagnosing it as a case of cerebellar atrophy, yet I am still of opinion that the symptoms were due to some cerebellar change which by our present methods we are unable to demonstrate.

The case was as follows: J. W., aged 62, was admitted to the National Hospital in September, 1907. He had always been healthy except for gonorrhœa and syphilis, which he had in 1873. About five or six years ago he noticed a hesitancy in walking; this was more pronounced when passing anyone—he had always been nervous; he had never suffered from giddiness. He lost confidence in coming downstairs, but has had no difficulty in going up. Four and a half years ago he had much worry. One night he went to bed as usual, but next morning as he got out of bed "something happened to him"—he lost his balance and fell. There was no loss of consciousness, no dizziness; he was able to pick himself up and get into bed. He subsequently felt all right in himself, but had no power of walking, though he was able to

¹ *Proc. Roy. Soc. Med.*, 1908, i, Clin. Sec., p. 18.

stand with help. Since this attack he had found an unsteadiness in writing; this was not present in the morning but came on towards night. He had had intermittent buzzing in the left ear, but this could always be stopped by lying on that side, or even by holding the hand over the left ear. Memory was very good. For the last four and a half years he had had rather a nasal speech, but this varied much.

Present Condition.—The patient is an old man, extremely thin, with little or no subcutaneous fat. The muscles are hard and wiry and of great strength. Intelligence is excellent; answers are given readily and clearly; attention and memory unimpaired. All the special senses are normal. The optic discs are normal: no limitation of fields; the pupils are unequal, the right being smaller than the left. They react readily to light and on convergence. There is a slow jerking nystagmus on deviation to the left; that to the right is quicker and finer. Nystagmus was also present when patient looked upward. The movements of the tongue and palate were normal. The patient had a peculiar nasal voice, but articulation otherwise was normal. All movements of the upper extremities are represented and are of good power. Ataxia is present on both sides, but is more marked on the left than on the right. When the hands are held outstretched there is a marked tremor. There is no hypotonia. The abdominal muscles act well and equally, and the patient can raise himself from a lying position without the use of his arms. The legs are very strong when one considers the smallness of the bulk of the muscles, and all movements can be performed when lying. There is marked ataxia both right and left. A coarse tremor is present on both sides. There is no hypotonia. The gait is exceedingly ataxic, and he cannot walk without support. The patient is able to stand, but with difficulty, which is in no way increased by shutting the eyes; his tendency is always to fall backwards. Sensory system: there is no anaesthesia or analgesia. The muscle pain sense and joint sense are perfect, and there is no paræsthesia. Reflexes: the arm-jerks are brisk and equal. The abdominal reflexes are equal. The knee- and ankle-jerks are present. There is no ankle-clonus. The plantar response is flexor on right and left. There is no sphincter trouble.

The patient remained in hospital nine weeks; during the time he gained weight and improved in general health, but his gait was as ataxic as ever. He left the hospital on November 14, 1907. The patient lived at home from November 14 till the time he died (February 17, 1908). From the history obtained from his relations, and from the doctor under whose care he was, there are grounds for believing that he starved himself.

The autopsy was performed by Dr. Holmes at Dunchurch, near Rugby. The skull and vertebral column were normal. No disease of either the dura mater or pia-arachnoid was visible. The brain was large and well formed, and there was no evidence of any disease. The vessels were but slightly atheromatous. The cerebellum was apparently of normal size, there was no evidence of atrophy or other disease on its surface.

After hardening for seven days in 10 per cent. saline formalin solution, the weight of the whole brain to the pyramidal decussation and cerebellum was 1,555 gm.; the weight of the cerebellum and pons and medulla, 190 gm.; the weight of the cerebrum, 1,365 gm. The average weight of the male brain, as given by Bischoff, is 1,362 gm. Holmes states that the normal weight of the brain-stem and cerebellum together should amount to between 155 gm. and 180 gm. The normal proportion of the pons and cerebellum to the cerebrum should be somewhere between 1 to 6 and 1 to 7.5. The proportion in this case was as 1 to 7.2.

Pathological Examination.—The cortex of the brain was examined by the Marchi, Weigert-Pal, Nissl, and van Gieson methods. The cortex appears perfectly healthy, the cells well stained and of normal appearance. The vessels were markedly thickened throughout the nervous system, but there was no evidence of endarteritis. The basal ganglion and pons also appear normal; no pathological change could be found by any method. The cerebellum was to the naked eye perfectly normal, and on microscopical examination, except for some spacing between the granular layer and the cortex, showed no pathological change. The cells of Purkinje were well stained, and were of normal size, number, and distribution. The dentate nucleus appeared perfectly normal. By the Marchi method some fine scattered black granules were to be seen in the white matter, but they formed no definite tract, and were only such as may be found in the brain of any person of advanced age. All the cells of the cranial nuclei showed some pigmentation, but this feature is especially marked in the cells of the twelfth nerve nucleus. There is no pigmentation in the cells of the cerebellum. Spinal cord: There is slight paleness of the column of Goll in the cervical region of the cord; but apart from this the cord appears perfectly normal. The cells of the anterior horns are markedly pigmented throughout the spinal cord. Numerous amyloid bodies are to be seen, more especially in the posterior columns.

All the changes above described may be found in the nervous system of men of advanced age, without any evidence of nervous disease. It

may therefore be said that the nervous system presented no pathological change which could be associated with the symptoms presented. Clinically, the case would, on the one hand, correspond with the type described by Dejerine and Thomas under the title "*L'Atrophie olivo-ponto-cérébelleuse*," a disease exhibiting the cerebellar syndrome which comes on at advanced age and slowly progresses; or, on the other hand, it might be relegated to Class III, described by Holmes under the title "*Progressive Cerebellar Disease due to Vascular or Interstitial Lesions*." Pathologically, the case will not correspond with either of these groups. Two views are open for adoption: (1) That the case is one of hysteria, with a remarkable simulation of cerebellar symptoms; or (2) that the changes in the nervous system that gave rise to the symptoms are of such a nature that they are not discernible by our present methods. Personally I should favour the latter view, for I cannot believe that the symptoms were of an hysterical nature.

I would here record my indebtedness to Dr. Gordon Holmes, without whose help it would have been impossible to have made the examination.

REFERENCES.

All the literature will be found in Dr. Gordon Holmes's recent excellent review: "*An Attempt to classify Cerebellar Disease*," *Brain*, 1907, xxx, p. 545.

DISCUSSION.

Dr. HEAD said there was one possibility which Dr. Batten did not take into account, namely, that the patient might have had disease of the semi-circular canals. Those who suffered from disease of the internal ear did not feel giddy continuously although the gait might be unsteady. Sherrington had pointed out that the cerebellum was, after all, mainly the ganglion of which the internal ear was the afferent end-organ. He (Dr. Head) thought that in the past enough attention had not been paid to the internal ear in the differential diagnosis of supposed cerebellar states.

The PRESIDENT (Dr. C. E. Beevor) asked whether there was any deafness. Also whether Deiter's nucleus was examined.

Dr. FERRIER observed the patient had a nasal voice, and asked whether he had it before the disease began.

Dr. PAGE MAY asked whether the suprarenal capsules, the pancreas, and the deep glands were examined, with the view of throwing some light on the cause of the marked wasting, which at present remained unexplained.

Dr. L. GUTHRIE said Dr. Batten had mentioned the examination of the anterior horns, but he did not say whether the ascending cerebellar tracts were examined. It was possible that certain cerebellar symptoms might be as

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much due to lesions of the cerebellum as to lesions of the afferent tracts leading to the cerebellum.

Dr. BATTEN, in reply, said there was no deafness nor giddiness, though the patient had one attack of falling. There was buzzing in the left ear which could be prevented by turning on to the side. The nasal voice was said to have come on with his other symptoms. He had not specially mentioned the posterior columns and direct cerebellar tract, nor many other parts of the nervous system, because the examination of the nervous system was negative. The possibility of the condition being due to semicircular canal disease had occurred to him, but he was not aware that cerebellar symptoms were met with clinically apart from vertigo, which was not present in this case. No change was noticed in Deiter's nucleus or the descending tracts therefrom. The patient did not hold the head in any particular posture.

Diffuse Sarcomatosis of the Brain and Spinal Cord.

By FREDERICK E. BATTEN, M.D.

DIFFUSE sarcomatosis of the brain and spinal cord is a somewhat rare condition; the cases usually present considerable difficulty in diagnosis, and that about to be recorded was no exception to the rule.

The case presented many features which suggested meningitis, but the long duration, the marked optic neuritis, the progressive loss of sight and loss of hearing, with headache and vomiting, suggested the presence of some intracranial tumour. There were, however, no signs by which the tumour, if present, could be localized. The examination of the blood showed a leucocytosis, and it was thought by some that this might point to some chronic form of intracranial abscess. The result shows that this assumption was not justified.

The examination of the cerebrospinal fluid was suggestive of the presence of tumour, and if greater attention had been paid to the flakes of endothelial cells which were present, a correct diagnosis might have been arrived at, for on comparing these cells with those obtained from a scraping of the growth after death it was seen that they were strikingly similar, the only difference being that the cells obtained from the cerebrospinal fluid had an indefinite outline and a nucleus which was less distinct—probably due to some disintegration of the cells taking place in the cerebrospinal fluid.

The history of the case is as follows: D. M., a boy, aged 10, was the second of three children; the other two were healthy. In November,

1907, he was taken ill with influenza. He was ill for about fourteen days with sickness and headache, then he returned to school and remained there for one month; he was, however, never quite well. He was sent home from school one day because he was sick; from this attack he again recovered, but he continued to have "sick" attacks till March, 1908, when his headaches became worse and he vomited three to four times a day. As he was steadily getting worse he was admitted to the Buchanan Hospital, St. Leonards-on-Sea, in April, and the diagnosis then provisionally made was tuberculous meningitis. He remained in hospital for eleven weeks, and many times was thought to be dying, and at the end of June he was taken home by his parents.

The following is a report of his condition kindly furnished by Mr. Frank H. Shaw: On admission he was complaining of headache and vomiting. He preferred to lie with his head under the bedclothes, and was very intolerant of light; he had commencing optic neuritis in both eyes. For days he would lie huddled up in bed, and then be quite bright again and try to get hold of something to read; his intellect was in no way impaired; at times the vomiting was very persistent; he gradually got worse, and the sight failed with increasing optic neuritis; his temperature never rose above 99° F., and pulse varied from 66 to 98; all the reflexes were exaggerated. Whilst in hospital he first had diplopia, and after his discharge the eyesight failed noticeably; the failure of sight first took place in the right and then in the left eye. About the beginning of July he began to lose the hearing of the right ear, and then the left ear became affected, and he was totally deaf about the end of July. Although he could neither see nor hear he was quite conscious, and would speak when he wanted anything, or wished to micturate, or had a desire to go to stool; he asked to feel things and asked the time. Since he could neither see nor hear, in order to know the time he counts one, two, three, four, &c., and when he comes to the hour his mother touches him. He is stated to have had pyrexia in the early stages of the disease, but recently his temperature has been sub-normal (96.4° F.). He was tested with tuberculin but failed to react. He was stated to have "fits" when moved; he has had about twelve in all. During the attacks the head was retracted and turned to the left; the eyes were turned up; after the fit he was more or less unconscious; the last fit occurred whilst being moved to hospital on August 5.

On admission to the Hospital for Sick Children, Great Ormond Street, the boy was very wasted. He lay on either side with the legs and arms flexed; the neck was stiff, but the head was not retracted; he resented

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any movement; he was quite blind and deaf to the loudest sounds. Sometimes he seemed quite unconscious and delirious, but more often he was quite aware of what was happening to him; he talked quite sensibly at times. There was slight weakness of the left side of the face (of a peripheral type). It was not possible to determine any weakness of the ocular muscles; the pupils were equal, generally rather dilated, but undergoing constant variation in size; it was doubtful whether they reacted to light owing to the hippus.

There was marked optic neuritis with swelling of 4 D. to 5 D. The arms and legs were moved in all directions, and there seemed to be no paresis of the limbs; the knee-jerks and ankle-jerks were not obtained; the plantar responses were flexor; Kernig's sign was present, and the abdominal reflexes were present and equal. Sensation was apparently perfect. The pulse was feeble, slow, and regular; respiration was quiet and temperature 97° F. The ears were examined by Mr. Waugh, who reported a large perforation in the left and some retraction of the membrane in the right.

Lumbar puncture was performed and a clear fluid escaped under considerable pressure. Dr. Graham Forbes reported that on examination the fluid contained a faint trace of albumin; it did not reduce Fehling's solution, and, on examination of the centrifugalized deposit, was found to contain a scanty number of cells, lymphocytic and endothelial cells and flakes composed of endothelial cells. No organisms were seen and the fluid was sterile on culture. The examination of the fluid was in favour of tumour rather than meningitis. The blood was also examined by Dr. Forbes, who reported a leucocytosis of 26,000 per cubic millimetre.

The boy vomited on five occasions after admission to the hospital, but usually retained and swallowed his food well. He gradually passed into a drowsy condition and died on August 16, nine months after he was first taken ill.

Post-mortem Examination.—Brain: The dura mater was normal. The pia-arachnoid membranes covering the vertex of the brain were somewhat opaque, but there was no flattening of the convolution. There was no evidence of growth on the vertex of the brain. On the under surface of the brain, especially in the region of the temporo-sphenoidal lobes, there was a diffuse growth involving the pia-arachnoid membrane and the superficial layers of the cortex. On section the growth can be seen extending in along the sulci with the pia-arachnoid, but no large mass of growth occurs in the

substance of the cerebral hemisphere. There was a moderate degree of hydrocephalus.

Cerebellum: The whole surface of the cerebellum is to a greater or less extent covered by a diffuse growth of a pale pink colour. The growth is most extensive round the region of the fourth ventricle, and has the distribution which is commonly seen in posterior basic meningitis (fig. 1); the lateral areas of the cerebellum are less affected than either the superior or inferior surfaces. The superior surface of the cerebellum is covered with a diffuse growth which cannot be separated from the pia mater and invades the cerebellar substance superficially (fig. 3). On section through the cerebellum a diffuse growth is seen to occupy the middle lobe of the cerebellum, and this extends down into the fourth ventricle—in fact, the fourth ventricle is practically filled up

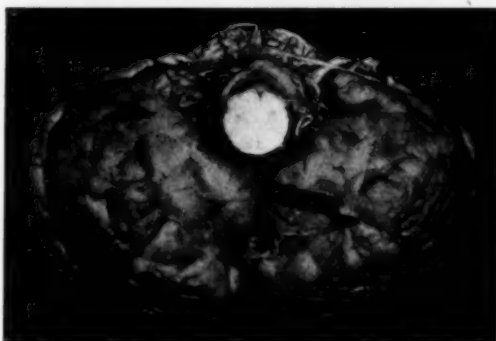


FIG. 1.

Photograph of the cerebellum, showing the growth in the region of the fourth ventricle, extending over the surface of the cerebellum and surrounding the medulla.

by growth. The ventral surface of the pons seems to be free from growth, but surrounding the medulla there is a thick layer of growth from 2 mm. to 3 mm. in thickness.

Spinal cord: Along the whole length of the spinal cord, both on the dorsal and ventral surface, but more extensive on the dorsal, there is a diffuse growth. The growth is rather less extensive in the thoracic than the cervical or lumbar region. The growth is soft and of a pale pink in colour. In parts it is nodular. On section the outline of the spinal cord can be seen fairly distinctly below the growth; but the growth

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cannot be detached from the spinal cord (fig. 2). The growth extends down into the filum terminale and involves some of the roots of the cauda equina. The dura mater showed no growth, nor were there any growths in the skull or bodies of the vertebræ or elsewhere in the body.

Microscopical Examination.—The growth consists of a groundwork of fine connective tissue, among which are situated fairly numerous round and oval cells of varying size, staining deeply with hæmatoxylin. The relative amounts of groundwork and cells vary considerably in

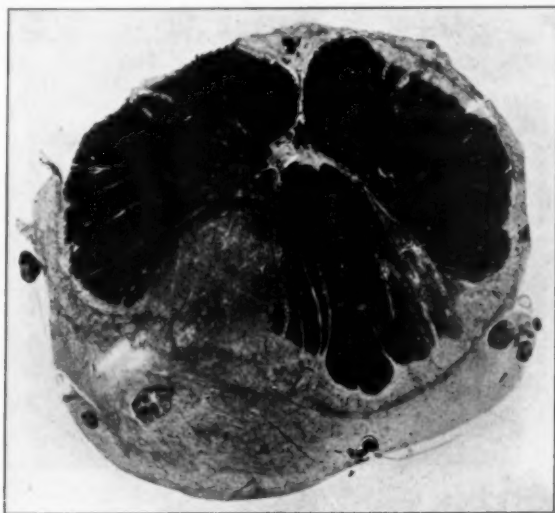


FIG. 2.

Transverse section of the cervical region of the spinal cord, showing growth surrounding the spinal cord and infiltrating the substance. (Stained by Weigert's method.)

different parts of the section; sometimes the cell elements, sometimes the areolar tissue predominates, but the growth is essentially of the same character wherever examined. The growth has the characters of a mixed-celled sarcoma. The growth tends for the most part to limit itself to the membrane, and does not infiltrate the cerebral or spinal substance, but this limitation is not absolute, and areas can be found in which the nervous tissue, cerebrum, cerebellum, and spinal cord

are infiltrated with growth. This feature is most marked in the spinal cord (fig. 2), and especially in the region of the posterior roots, and the growth can here be seen carried in along the sheaths of the vessels. At but few places is the cord extensively affected, but in some parts both grey and white matter are involved.

The case bears a most close resemblance, both clinically and pathologically, to that reported by Stanley Barnes, and the conclusions deduced from that case are applicable to the present case.

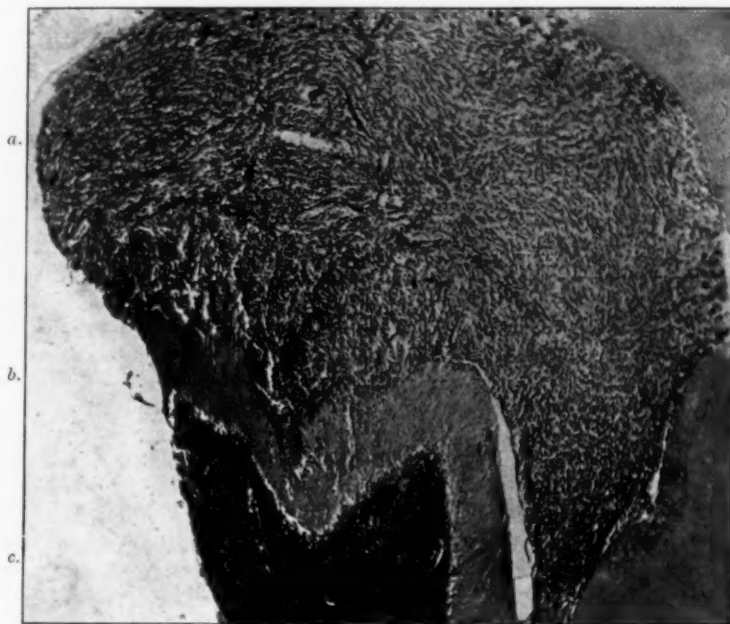


FIG. 3.

Microscopic section of the cortex of the cerebellum, showing the growth on the surface and infiltration of the cortex along the sheath of the vessels; *a.* growth, *b.* cortex of cerebellum, *c.* granular layer of cerebellum.

Diffuse sarcomatotic infiltration of the pia-arachnoid is an expression of sarcomatous infection of the cerebrospinal fluid as the result of some primary growth which lies exposed to the stream of cerebrospinal fluid. The distribution of the infection is strikingly similar to that found in meningococcal meningitis.

18 Batten: *Diffuse Sarcomatosis of Brain and Spinal Cord*

Grünbaum reported a similar case in 1906, and more recently Stursberg has dealt with diffuse metastatic sarcomatosis of the meninges, and points out that in some cases the symptoms may be those of a polyneuritis without any symptom pointing to meningeal affection.

LITERATURE.

- BARNES. *Brain*, 1905, xxviii, p. 30.
GRÜNBAUM. *Path. Soc. Trans.*, 1906, lvii, p. 313.
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DISCUSSION.

Dr. STANLEY BARNES (Birmingham) said he had had a case which closely resembled the one described by Dr. Batten. In the former the primary growth was in the frontal lobe, and had burst into the lateral ventricle; the spinal cord was enveloped with a mass of sarcoma much as in the case just described, excepting that the layer was thicker, probably owing to the patient's living longer after the dissemination occurred. In the present case there was very little involvement of the posterior root ganglia, whilst in his own the tumours there were almost as large as chestnuts, with nerve fibres running through them; this condition extended to the lowest roots of the cauda equina. Another fact which Dr. Batten had not especially mentioned was the presence of internal hydrocephalus, which had apparently been considerable, as the brain shown still had considerable distension of the ventricles; in his own case that was extreme. He had since had two other cases, specimens from which were now in the pathological museum of the University of Birmingham, showing the distribution of secondary growths through the medium of the cerebrospinal fluid. In one of them the distribution was backwards (against the normal flow of the fluid), the primary growth being in one lateral lobe of the cerebellum whilst the secondaries after the rupture into the fourth ventricle formed a series of knobs extending upwards into the lateral ventricles on both sides, even reaching forwards as far as their anterior horns. He believed the backward distribution to be due to the partial stoppage or stagnation of the cerebrospinal fluid due to the pressure of the growth upon the outlet through the fourth ventricle. The exact nature of these disseminated growths seemed to vary. Very few had been recorded in the literature, and in none had he (at the time of publishing his own case) been able to find a suggestion of the real cause, as he believed, for this dissemination, viz., that the mass of sarcomatous material around the medulla and cord had been caused by rupture of a tumour high up in the nervous system, which had thus "infected" the cerebrospinal fluid. Although he felt no doubt about it himself, it would be very satisfactory to be able to record it in other instances, especially as in the first few cases, at any rate, of this condition which had been published there had

been demonstrated merely the diffuse sarcomatosis without the ruptured and "infecting" tumour.

Dr. FARQUHAR BUZZARD asked whether any member of the Section had seen diffuse sarcomatosis of the central nervous system arising secondarily to tumour outside the bony canal which surrounded the central nervous system. It seemed strange that one not infrequently saw a diffuse sarcomatosis outside the vertebral column spreading into it along the lymphatics of the various spinal roots, but such cases did not seem to lead to diffuse sarcomatosis over the membranes, in the same way as the cases which had just been referred to. It looked as if there was much less tendency to the diffusion of the sarcomatous process when it was confined to those lymphatics of the spinal roots than when it had free access to the cerebrospinal fluid, a fact which suggested that there was no free communication between these two "fluid" systems.

The Pathology of Tabetic Amyotrophy.

By S. A. KINNIER WILSON, M.B.¹

IN this brief communication it is not intended to enter minutely into the vexed question of the pathogeny of the amyotrophy of tabes dorsalis. I wish at present merely to describe succinctly the pathological findings in two cases of tabetic amyotrophy which I have had the opportunity of examining, and, in so far as they have a bearing on the pathogeny of the condition, to indicate concisely what their significance may be. The first case was in the National Hospital three years ago, under the care of Dr. Ferrier, and subsequently died outside. I am indebted to Dr. Ferrier for permission to refer to the clinical features of the case. For the second I am under an obligation to Dr. Gordon Holmes, who kindly placed the material at my disposal.

In each of the cases, in particular in the first, muscular wasting was a pronounced feature. The first patient, indeed, presented all the clinical characteristics of the Aran-Duchenne type, beginning in the small muscles of the hands. The second case is of value inasmuch as the atrophy, beginning also in the small muscles of the hands, was very distinctly asymmetrical. The unusual nature of this change enhances the interest of the pathological result.

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CASE I.

A. M., aged 47, was admitted to the National Hospital in 1905 under the care of Dr. Ferrier. He had had syphilis twenty-two years previously. For fourteen years he had suffered from difficulty in micturition; for ten years he had been subject to lightning pains and for nine years girdle pains. About ten years ago he realized the fact that he was becoming slightly and generally emaciated. He first noticed the local wasting of the small hand muscles about seven years ago, a change which began on the right hand and subsequently spread to the left. For years his principal symptom had been an abnormal sensation of fatigue in the legs, but up to two years before his admission he could walk fairly well. Influenza intensified his troubles, and when he came into the hospital he was very weak indeed. Presenting the characteristic symptoms of double Argyll-Robertson pupil, absent deep reflexes, sphincter impairment, shooting pains, paræsthesiæ of all sorts, girdle pains, and gastric crises, he showed in addition a pronounced degree of muscular wasting. The absence of subcutaneous fat rendered the wasting of the muscles more obvious, as far as their general aspect was concerned, but there was evidence further of very definite local atrophy. Thus there was extreme wasting of the thenar and hypothenar eminences on both sides, the muscles of which had lost both their galvanic and their faradic reaction. The interossei, however, were less wasted, and contracted readily enough on electrical stimulation. The corresponding movements were, of course, exceedingly weak; the thumb movements were absent or performed inadequately by the help of the long muscles. In addition, flexion and extension at the wrist were weak, and extension of the fingers; the volume of the forearm muscles, however, was still very fair. In the legs all the muscles were small, and in addition there was marked wasting of the anterior tibial group on either side, with corresponding impairment of motility; the toe movements were good. The sensation of pain was diminished along the ulnar border of the arms and hands, and almost lost in the legs. There was a certain amount of tactile and pain loss over the trunk.

A year later the patient was readmitted considerably worse. The amyotrophy had steadily increased; all the small muscles of the hands were affected, and in addition the forearm muscles were much weaker. In the legs the condition had not progressed much, beyond an increase in the general thinness of the musculature. In spite of the wasting of the anterior tibial group dorsiflexion of the foot was possible through a

good range, though the movement was weak. The right ankle showed an incipient Charcot joint. The alteration in sensation was intensified, and there was almost complete tactile loss over the body and legs.

The patient became progressively weaker and died in the autumn of 1906.

CASE II.

F. C., aged 40, came under observation in December, 1904, with characteristic features of tabes in the shape of lightning pains, defective vision, Rombergism, ptosis, sphincter troubles, sluggish pupils, and absent knee-jerks. There was a history of a stroke two years previously in her sleep, when she awoke to find herself paralysed on the right side, with her mouth drawn to the right. For seven days she had difficulty in speaking. This hemiplegia was of a transient nature. On coming under observation she was found to present slight wasting of the muscles of the right arm and forearm, and marked wasting of the small muscles of the right hand. The right motor fifth was very weak.

Her condition progressed during the next two or three years; she became bedridden, and slight general emaciation set in, while the amyotrophy of the right hand became more pronounced. The patient died in October, 1907.

Without entering into details, I may simply state that at the autopsies the central nervous systems and many pieces of muscle and peripheral nerve were removed for subsequent examination.

In the first case the lower part of the sixth, the seventh, and eighth cervical and first dorsal segments were cut in series and prepared by Nissl's method; the first sacral segment, similarly, was examined in serial sections. In the other, the eighth cervical and first dorsal were treated identically. The sections cut were $15\ \mu$ in thickness, and the anterior horn-cells in every second section were enumerated. This was a matter of considerable difficulty, inasmuch as they were so atrophic that to decide which were to be counted and which ignored was not easy. As a rule, all cells with nuclei were counted; but many others, which consisted of the shell, so to speak, with pigment centre, were also included, as long as they preserved the appearance of a cell.

The results in the first of the two cases were briefly as follows: Throughout the segments examined the actual loss of anterior horn-cells was extreme; many sections were without any normal cell, normal either

in size or character of staining; several sections, chiefly in the lower segments, were practically cell-less (I refer, of course, solely to the cells of the anterior horns). Of 103 sections with cells in them, the average number of cells persisting in each was on the right eight, and on the left seven. The average in each segment was practically the same; it was rather lower in the inferior segments than in the superior segments. The changes were very much alike on each side of the cord. Of the grouping of cells in the different levels, all regional subdivisions were involved in the affection. Comparing my figures with those given in Bruce's "Atlas" of normal cord sections, I find that, to refer to the eighth cervical only, in the ventro-mesial group I get an average of 1·8 on the right and 1·1 on the left, instead of 4; in the ventro-lateral group, 2·5 on the right and 3·8 on the left, instead of 29; in the dorso-lateral group, 3·9 on the right and 2·8 on the left, instead of 33. In the other segments examined changes as great were discovered. In the first sacral segment the proportion of cells preserved was higher, their number being reduced by about a third or a half from the normal.

The nature of the change in the anterior horn-cells may be referred to. Speaking generally, it may, I think, be said that in the cervical and dorsal segments particularly the cell change is a "sclerotic" or atrophic one. The cells which there persist are, as a rule, small and shrunken; some are elongated, but at the same time attenuated. It is a feature of this state that they stain fairly deeply, as though shrinkage of the achromatoplasm confined the tigroid elements in a smaller space, so that the hue of the cell is dark in spite of differentiation. The nucleus takes on a bluish tint, and partakes in the shrinkage process. The nucleolus remains dark. The cell processes also are readily traceable at first, inasmuch as they, too, stain more deeply than usual. These sclerosed or atrophic cells pass by degrees into a state of pigmentary atrophy; pigment appears in the cell body, while the tigroid is clumped round the periphery of the cell and the nucleus is displaced. The cell seems to become still smaller, still "drier," if the expression may be used, till at length it may be represented by a mere shell of small chromatic elements surrounding a mass of pigment. The processes shrivel also, and the shell becomes oval or round, or spindle-shaped, instead of having the polygonal contour of the normal cell. This type of atrophic cell seems to be the outcome of an essentially chronic process.

Other cells in the cervical region, but more particularly in the lower part of the cord, seem to have undergone a change of a slightly different nature. It is a change which is met with in less chronic conditions, and

its features are familiar. The cell does not actually shrink, at first at least, but the alteration begins round the nucleus, where the Nissl bodies become powdered, the larger tigroid elements remaining unchanged at the periphery. Gradually the pulverization of the tigroid continues; the cell takes on a more or less homogeneous blue tint; the nuclear membrane shows signs of shrinkage and the nucleus usually becomes excentric; sometimes the nucleolus becomes vacuolated. The granular elements give place to pigment commonly in the body of the cell, while the nucleus may come to lie on the cell surface, and then be shed. Once the nucleus is gone the cell dies rapidly, and all that is left to indicate its former position is a little mass of pigment. Vacuolation of the cell body also occurs, in all probability as a subacute process. It is highly probable that the former of these two processes is the chief one in the production of the amyotrophy. Out of many thousands of cells counted, only four or five showed any vacuolation, indicative of somewhat acute change, and the proportion of those belonging to the second type is small. Both patients were bedridden for some time before their death, and this fact, coupled with the possibility of a terminal infection, must be taken into consideration in endeavouring to interpret the meaning of the cell changes.

Still confining ourselves to the cellular changes, the results in the second case are as follows: In sixty-one sections from the first dorsal segment the total number of persisting anterior horn-cells is on the left side 1,251, and on the right 778; the latter therefore is poorer than the former by 473 cells, an average of about eight to each section. In forty-nine sections from the upper part of the eighth cervical segment, the right is poorer than the left by 279 cells; in the lower part of that segment, however, there are rather more cells on the right than on the left. The deficit involves all the horn groups more or less equally; perhaps the ventro-mesial and the dorso-lateral are the groups that have suffered most. Compared with the figures given in Bruce's "Atlas," the number of cells on the left side in this case is well below normal, and therefore the loss on the right side is proportionately greater, the side on which, clinically, muscular atrophy was pronounced. In this second case the proportion of cells showing changes which we are accustomed to regard as somewhat acute is rather higher than in the other, but in the cervical and dorsal segments examined the majority of the cells reveal a purely atrophic alteration. It is noticeable that the degeneration of the cells is highly irregular; many fairly normal cells lie in the closest conjunction with grossly atrophic cells.

In order to complete the pathological picture it must be remarked that the changes in nerve and muscle were profound. As an instance of the former may be taken the ulnar nerve of the first case. To the naked eye it appears flattened and thin, and the nerve bundles are seen to be almost distinguishable one from the other, like fine white threads in a tract of connective tissue. Microscopically the number of fibres is diminished; many sheaths are empty; all are more or less fragmented; only a small proportion appear to present anything like a normal appearance. By way of interstitial change the endoneurium round the individual fibrils is somewhat thickened, the perineurium considerably so, connective strands passing in through the nerve and giving it the discrete appearance already referred to. In some places fatty invasion of the perineurium is manifest.

The changes in such a muscle as the flexor brevis pollicis are equally advanced. The muscular parenchyma is greatly reduced, often scarcely to be traced at all. Where muscle fibres persist, they are isolated from each other by overgrowth of connective tissue, and while frequently preserving their striation, they are not markedly unequal in size; no obvious individual hypertrophy is noticed, or hyaline change. Everywhere the interstitial modifications are profound. The walls of some of the muscle spindles appear to be thickened, although the muscle fibres within seem fairly normal. All the vessels have greatly thickened walls. Between the muscle bundles there is great overgrowth of connective tissue, with increase in nuclei, but the nuclei of the muscle sheaths are not increased to the same extent. Fatty deposits are common in the affected muscles.

The changes which have been enumerated in a very curtailed manner go, in my opinion, to support the view that the amyotrophy of tabes is of central origin. The muscular changes are not those of a primary myopathy; the sclerosed appearance of the muscles is indicative of a secondary change, replacing the parenchyma. The changes in the nerve trunks scarcely can be said to be those of peripheral neuritis—although it is hazardous to speak definitely in view of the chronic nature of the cases. In tabes a certain amount of meningitis is practically a constant feature, and it may be that some retrograde degeneration of anterior horn-cells is possible; the pathogenic importance of this meningitis, however, seems to be almost negligible. The irregular way in which the anterior horn-cells are affected, almost normal ones lying beside profoundly atrophic ones, coupled with the absence of signs of really focal change, suggests that we are dealing with a primary

atrophic affection of the cornual cells. There is obviously a degree of correspondence between the cell atrophy and the muscular wasting, but it cannot be gainsaid that the cellular change is diffuse.

Of the various views that have been propounded to explain the amyotrophy of tabes little need be here said. The results of the examination of the present material do not support the view that the affection is caused by ischæmia of the cord; nor that it is due solely to peripheral neuritis; nor that disease of afferent collaterals to the anterior horns will account for the changes; there is no pathological evidence of chronic meningo-myelitis; a primary myopathy can be negated. Whether the spinal amyotrophy is comparable with Aran-Duchenne paralysis is more difficult to settle. Clinically, indeed, it is more or less identical with the latter, but it may not be identical in its pathogeny. There is much to be said in favour of the view that the spinal amyotrophy of tabes is syphilitic in origin, and if we argue, as we should, from the pathological to the clinical, we may say that tabetic muscular atrophy is neither a symptom nor a complication of tabes, but an associated condition of syphilitic origin.

DISCUSSION.

Dr. FERRIER said he had listened with great attention to Dr. Wilson's important communication, and felt much obliged to the author for reminding him of the interesting case which he had worked out so carefully. Everyone was acquainted with cases of muscular atrophy associated with tabes. Dr. Whiting, who was House Physician at Queen Square, published a paper ten years ago in which he analysed about 200 cases of tabes. In these he found not only a large percentage of paralysis, but of atrophic paralysis—he believed it was about 8 per cent. Many pathologists had described changes in the anterior cornual cells in tabetic amyotrophy, though not perhaps so accurately or with so much detail as Dr. Wilson. But he (Dr. Ferrier) did not think that all cases of atrophic paralysis occurring in tabes were of the central type. He thought the clinical history of many of them would point rather to a peripheral origin, especially the cases associated with drop-feet. And many pathologists had described neuritic changes in the muscular atrophy of tabes. Still, in many cases the atrophy was undoubtedly of central origin, segmental in character, and slowly progressive. But what he thought most interesting was the method of production of this degeneration. There were various theories with regard to it. One wondered whether it was often only a case of juxtaposition, not causally related to the tabetic degeneration. The common view was that it was a nutritional change conditioned by the degeneration of the

collaterals of the posterior roots, extending into the anterior horns and causing the cells of the anterior cornua to undergo corresponding atrophy. Dr. Wilson's view that tabetic amyotrophy was central in origin and specific in character was an important one, and worthy of further investigation.

Dr. JAMES TAYLOR said that everyone who saw many cases of tabes must have been struck by the muscular atrophy which occurred in some of them, in some an atrophy very suggestive of the Aran-Duchenne type. And it was particularly interesting that Dr. Wilson should have brought forward the idea that it was possible in some cases that the process was actually central; at all events, the pathological findings and microscopical appearances were identical with those in progressive muscular atrophy. But he (Dr. Taylor) thought that in some of the other cases the atrophy might be peripheral in type, and he had been considerably interested in a group of cases in which that muscular atrophy affected the ocular muscles. Nothing impressed one in reference to tabes more than the occasional occurrence of ocular palsies, especially paralysis of the third nerve, which was an extremely common complication. And one was struck by the rapidity with which that cleared up under the influence of antisyphilitic remedies. Those cases he had always been inclined to regard as probably syphilitic in origin, very likely the result of a gummatous neuritis affecting the third nerve. But besides those, there were others, in which the process was much more widespread—more random—and did not pass off; that he had always been accustomed to look upon as of the nature of a progressive muscular atrophy occurring in tabes and complicating it. So he thought it possible—although it was to some extent theoretical—that that view might be correct, not only as regards muscular atrophy affecting ocular muscles, but also respecting the muscular atrophy affecting muscles supplied from the spinal cord. With reference to the second case which Dr. Wilson described, in which there was hemiplegia complicating the tabes, one knew that in many cases of hemiplegia, especially, as Dr. Hughlings Jackson had been constantly pointing out, cases of *left* hemiplegia, much muscular atrophy might be present, in the arm especially, frequently associated with some arthritic changes. And the changes which Dr. Wilson described in one of his cases seemed to him (Dr. Taylor) to be much like those which would be found in such a case of hemiplegia associated with arthritis of the shoulder-joint, with consequent brachial neuritis; he meant the changes in the muscles and in the peripheral nerves themselves—he did not speak so definitely about the changes in the spinal cord. He threw out the suggestion that hemiplegia, with some arthritic and neuritic complications, might have conduced, at all events in some degree, to the presence of the muscular atrophy in that particular case of tabes. The subject was an extremely interesting one, and the Section was much indebted to Dr. Wilson for having brought it forward so lucidly.

Dr. FARQUHAR BUZZARD said he understood Dr. Taylor attributed to Dr. Wilson the statement that pathologically the cell changes in the two cases were identical with those in cases of the Duchenne type of muscular atrophy.

He (Dr. Buzzard) did not know whether that was what Dr. Wilson said, and he would like to inquire, because he had examined a considerable number of cases of progressive muscular atrophy, but had never seen cell changes comparable with those described by Dr. Wilson, with marked vacuolation and excentric nuclei. He was surprised to hear Dr. Wilson describing those changes as primary degeneration, that is, if one looked upon progressive muscular atrophy as the type of a primary degeneration affecting the anterior horn-cells. Some of the changes which Dr. Wilson described were what one saw well in alcoholic neuritis, and it was difficult to say whether they were primary or secondary from their appearance alone.

Dr. BATTEN said that one of the points referred to by Dr. Wilson was in favour of the peripheral rather than the central origin of the condition, namely, that the muscle spindles had a thickened sheath.

Dr. GORDON HOLMES said he owed an apology to Dr. Wilson for having given him incomplete information concerning the materials he handed to him. One of the most interesting points in the second case was that, in addition to the wasting of small muscles of one hand, there was complete atrophy of the masseter and temporal muscles on one side. That was an important point in attempting to interpret the nature of the muscular atrophy, because in that case he knew before performing the autopsy that those muscles were completely atrophied, and he searched as carefully as possible for any lesion in the course of the fifth nerve, and could not find any. The motor nucleus of the fifth had not been completely examined yet, but there was evidently a considerable loss of cells in it and degeneration of its fibres. This was much in favour of Dr. Wilson's view that the atrophy was due to central change, so also was the fact that the atrophied muscles were also represented in the cord in functional groups—for instance, the small hand muscles were represented by nuclei in the first dorsal region. On the other hand, they were not represented in any single nerve, certainly not so completely as in the cord. With regard to the question raised by Dr. Taylor as to hemiplegia, he stated it was even at first very slight; there was only a history of it, and when the patient came under observation, three or four years before death, it had disappeared. He discovered that the hemiplegia was due to a small lesion in the middle of one pyramid, secondary to degeneration of the basilar artery. He did not think the hemiplegia could have had any influence in the development of the process, especially as no secondary degeneration could be detected by the ordinary methods in the spinal cord.

The PRESIDENT asked whether Dr. Wilson thought the changes in the anterior horn-cells might be secondary to the changes in the cells of the posterior horn if they were affected. One knew that the motor cells were very dependent on the sensory cells. And von Monakow and Schäfer's work showed that the pyramidal tracts ended in the posterior horns and not in the anterior horns. He asked whether that degeneration might be the result of disuse secondary to atrophy of the cells of the posterior horns.

Dr. WILSON, in reply, said he had purposely avoided referring to the clinical side of tabetic amyotrophy, and had confined himself to the pathological findings in two known cases of the disease. From the clinical side, however, it could scarcely be said that the atrophy was of peripheral type. Its progressive and diffuse nature did not correspond with what was known of peripheral nerve palsies. While the hemiplegia complicated the clinical picture of the second case, it was transient, yet the atrophy was progressive. He thought it comparable to the progressive amyotrophy of plumbism from chronic toxæmia. He wished to make it clear that many causes might be considered as being at work in the production of the cellular changes, and he attributed the chronic type of change to the chronic action of the syphilitic toxin.

Neurological Section.

December 3, 1908.

Dr. CHARLES E. BEEVOR, President of the Section, in the Chair.

Lesion (? New Growth) of Corpora Quadrigemina and Right Optic Thalamus.

By JUDSON BURY, M.D., and C. E. BEEVOR, M.D.

T. B., MALE, aged 24. Family and past history unimportant. He denies having had venereal disease. Three years ago there was blurring of vision and diplopia for three months, and in September, 1907, drooping of the right upper lid was noticed. He has become progressively worse since then; there was no diplopia then, and there has been none since. In February, 1908, he had "influenza," and was in bed for a week, numbness in the left hand being noticed when he got up. "Rheumatism" followed a week later; almost all the joints were swollen and painful, and he was in bed for six weeks. On getting up he noticed that he was weak in the left leg (not in the right). The weakness in the left leg has steadily progressed since then. In July, 1908, weakness and athetoid movements in the left arm developed, of slow onset, but steadily progressive, and the following month blurring of print in reading occurred, but no diplopia. He found he could not read with the right eye alone, and he also noticed that this eye "swung out." For six months his speech has been slow and drawling, but there has been no headache and no vomiting.

On admission.—Special senses: Sight, vision, right $\frac{6}{18}$, left $\frac{6}{6}$; fields normal (rough test); optic discs normal. Hearing: Right, good bone and air conduction; left, fair bone and air conduction, but worse than on right. Taste and smell were normal. Cranial nerves (iii, iv, and vi): Pupils unequal, R. > L.; no reaction to light or accommodation. Right eye: Incomplete ptosis; all ocular movements abolished

except outward, which is good. Left eye: Great limitation of upward movement; slight limitation of outward, with downward rotation of eye at extremity of movement. Nystagmus on extreme movements of both eyes to right and of left eye only to left, rotatory clockwise. (v) Normal. (vii) Slight supranuclear weakness of left face with greater weakness on volitional than on emotional movement. (ix, x, xii) Normal. Motor: Left arm, poor in power, marked athetosis; occasional fine tremors in hand; ataxia in finger-nose test with closed eyes. Left leg poor in power, no athetosis, no tremor. In walking left leg ataxic, and cannot stand on left leg alone with eyes open. Sensory: Left hemi-anæsthesia and analgesia, variable in degree, but most marked in hand and forearm; cold sensation recognized over left side, but cannot appreciate heat as such; sense of position abolished in all joints of left arm and complete astereognosis; sense of position poor in left leg. Reflexes: *Deep*, all present in arm and R. < L.; knee-jerk and ankle-clonus present R. < L. *Superficial*, abdominal present R. > L.; plantar, flexor on right, extensor on left.

Two Cases of Familial Cerebellar Degeneration.

By H. G. TURNEY, M.D.

J. S., AGED 37. With the exception of the half-brother presently to be described, the family history is negative. The patient has always been a moderate drinker, denies syphilis or other venereal disease, and has had excellent health all his life. He is married, has children, and the sexual functions have been and are normal. He dates his nervous troubles from an attack of influenza which occurred ten years ago, but the connexion is not very clearly made out. The first symptom noticed was the increasing unsteadiness of his gait. This was followed by tremors of the hands and trunk and, about four years after the onset, by an affection of his speech. All these symptoms have steadily increased, but have left his general health unimpaired; his mind and memory are as clear as ever, and he does not know what a headache is. On examination, he is strongly built and of normal development. Lower limbs: muscles somewhat hypotonic; knee- and Achilles-jerks present; plantar response flexor on left side, doubtfully extensor on right. Romberg negative. Gait typically cerebellar. Motor and sensory functions normal. There are constant tremors of the trunk; from the involvement of the respiratory muscles, even as he sits quietly, his breathing sounds

like that of a person shivering. The tremors, both of the trunk and of the upper limbs, are made worse by attention or attempts at control. The oscillations are fairly regular and simple, though there is a tendency to overshoot the mark if, for example, he tries to pick up a cup. The movements cease only when the patient lies with all the limbs supported. The speech is guttural and the articulation blurred; it resembles that of intoxication. There is no nystagmus, facial or oculo-motor paralysis, and the visual fields are normal.

S., aged 27, half-brother of the above on the mother's side. Symptoms began about ten years ago with unsteadiness of gait, which was followed in the course of the next few years by tremors of the rest of the body and an affection of speech. No cause is assigned by the patient, and the previous history is quite negative. The present condition is very similar to that of his brother, but, if anything, rather more advanced, and there are the following features in addition: the knee-jerks are absent; there is paresis of the right side of the face on emotional expression, which disappears on voluntary movements.

Two Cases of Peroneal Atrophy with a very late Development of Symptoms.

By JAMES COLLIER, M.D.

THESE patients are sisters. The mother was similarly affected; she died when aged 59, and was capable with her hands and feet till her death. There were thirteen children, of whom ten survived; four of these—one son and three daughters—were affected. The son was affected in early childhood; he is now aged 52, and is able to get about and earns a good wage as a tailor. One daughter was affected in the fourth decade of life similarly to the two patients here presented.

Case I.—Mrs. E., aged 40. She thinks that her feet were always of a curious shape, but no weakness was noticed till three years ago, when she commenced to wear her boots down on the outer sides. The weakness has increased very much in the last two years. Six years ago it was noticed that her hands were unduly thin—the weakness of the hands has become conspicuous during the last two years. There is marked weakness and wasting of the intrinsic muscles of both hands and of the flexor group of both forearms; there is backward dislocation of the upper end of the left radius (presumably spontaneous). Double pes cavus;

weakness of the extensors of the right toes; all movements of feet and toes slightly weak; no noticeable wasting between knee and ankle; knee-jerks present; ankle-jerks absent; no change of sensibility. She has one daughter, an epileptic, not affected.

Case II.—Mrs. de L., aged 48. Her feet have been a strange shape ever since she can remember; she suffered from weak ankles after scarlet fever in early childhood. When aged 42 she first noticed that the weakness of the ankles was increasing; when aged 45 weakness and wasting of the hands appeared for the first time. There is marked weakness and wasting of the intrinsic muscles of the hands and of the flexors in the forearms upon both sides. Double pes cavus; weakness of movement in toes and ankles; conspicuous wasting of the peronei on the right side; knee-jerks present; ankle-jerks absent; no change in sensibility. Children unaffected.

DISCUSSION.

Dr. L. GUTHRIE asked whether Dr. Collier had traced any family history of the condition in bygone generations of the family. Some years ago, at a Bristol meeting, two brothers were shown, typical examples of peroneal atrophy. They were yeomen, and the family had lived in the country for many generations, and the disease could be traced back as far as the late eighteenth century, at which date one of their ancestors was said to have died of "consumption of the muscles." With a little trouble, perhaps, such cases could be traced back, and the result would be interesting.

Dr. JAMES COLLIER, in reply, said the furthest history which could be obtained was that of the mother, who was affected with the condition. In other cases he had not gone back far enough to elucidate the point mentioned by Dr. Guthrie. Among the poor, the history of ancestors was often soon forgotten, and for that reason was difficult to trace.

Two Cases of *Myotonia atrophica*, showing a peculiar Distribution of Muscular Atrophy.

By F. E. BATTEN, M.D., and H. P. GIBB, M.B.

Case I.—Male, aged 37. For eight years the thighs have been wasting, and during the last five years there has been a tendency for the knees to give way in walking and a stiffness on first starting out to walk. Difficulty in relaxing the grasp, some stiffness of the jaws in eating, and stiffness of the tongue have existed for many years. The right and left temporals are wasted, but not the masseters. He has a myopathic facies,

and there is weakness of the orbicularis palpebrarum. The sterno-mastoids on both sides are wasted, and there is slight wasting of the ulnar border of both forearms. The vastus internus and externus are also wasted, but the rectus femoris is not. There are no pseudo-hypertrophy and no knee-jerks, but the ankle-jerks are brisk. Myotonic phenomena are shown in grasping right and left, and myotonic electrical reactions in flexors of forearm.

Family history: One brother had some difficulty in relaxing the grasp and walked with a high-stepping gait; he is said to have died of locomotor ataxia. One sister shows myotonia with myopathic facies and wasting of sterno-mastoids.

Case II.—Male, aged 56. For many years he had a difficulty in relaxing grasp and some stiffness in first starting out to walk in the morning. He has a myopathic facies and there is a weakness of the orbicularis palpebrarum as well as partial abductor palsy of the right vocal cord. The sterno-mastoids were wasted, and the upper part of the trapezii slightly so. There was a general wasting of the forearms and wasting of the vastus internus on both sides, also pseudo-hypertrophy of the calves and weakness of the flexors and extensors of the ankles. The knee-jerks were sluggish on the left and absent on the right. No ankle-jerks. He walks with a steppage gait and shows the myotonic phenomenon in grasping. No history of any similar condition in any relatives can be obtained.

An Unusual Case of Muscular Atrophy.

By E. FARQUHAR BUZZARD, M.D.

F. W., AGED 26, a carman. Family history: Several members of the family have died of "consumption." There are no instances of paralysis. Previous history: Has always had good health until three and a half years ago, but has been a heavy whisky drinker. Three and a half years ago he noticed some weakness of the right leg, as well as "pins and needles" in the same limb. Three years ago he had an attack of acute rheumatism, which was followed by increased weakness of both legs; this has been progressive ever since. At least three years ago it was noticed that the patient did not close his eyes completely during sleep, and the patient says he has never been able to whistle in the ordinary way. In October of this year he suffered from shooting pains and tenderness in both calves.

Present condition: Paresis of the orbiculares palpebrarum et oris. Atrophic paresis of the sterno-mastoids, the extensors and flexors of the wrist, and of all the dorsiflexors of the ankles. No sensory disturbances except tenderness of the calves. All reflexes obtained except the ankle-jerks. Sphincters normal. The condition of the patient is much the same now as it was two years ago, when he first came under observation, except for increased palsy of the dorsiflexors of the ankles.

DISCUSSION.

Dr. BUZZARD said the fact that his case showed some myotonia brought it alongside the cases shown by Dr. Batten and Dr. Gibb. The only points of difference he noticed were in some aspects of the muscular atrophy. In his case the chief muscular atrophy was in the peroneal and anterior tibial groups, whereas in one of the other cases the chief atrophy was in the thigh, less below the knee. In other respects the areas affected seemed much the same, and they appeared to belong to the same category, with which he had not previously been familiar.

The PRESIDENT (Dr. Beevor) said he did not remember seeing quite the same type of case previously; the combination seemed to be very extraordinary. He asked the authors whether they thought there was any myasthenic condition, and whether repeated efforts tired the patients, like cases of myasthenia gravis. He also asked whether the difficulty of relaxation completely stopped the person from moving, as in Thomsen's disease.

Dr. GORDON HOLMES asked the authors of what nature they considered the muscular atrophy to be; was it a primary muscle affection? Secondly, what was the pathology of the disease? In relation to the cases exhibited it was an interesting fact that in Thomsen's disease the essential pathological condition was enlargement of the muscle fibres with central nuclei; on the other hand, it was well known that the only muscle change which histologically separated primary myopathy from amyotrophies due to peripheral neuronie lesions was the presence of similar large or hypertrophied fibres. He asked in what way the hypertrophied fibres of myopathic muscles differed from those which characterize Thomsen's disease.

Dr. BATTEN, in reply, said he was almost unable to answer Dr. Gordon Holmes's second question, as he had had no personal experience to enable him to do so. It was only from reading the literature that he had been able to gather anything about the pathological condition. He hoped shortly, with Dr. Gibb, to put a record of such cases together, and to say something more on the question. He took the muscular condition to be a true myopathy, not a myelopathy. It would be interesting to know what were the electrical reactions in Dr. Buzzard's case.

Dr. BUZZARD, in reply, said that he had not examined the muscular reactions in his case. He remembered reading a paper by two French neurologists on the question of the association of myopathy and myotonia; it was an effort

to bring those two conditions together, and although at that time he thought the argument was rather far-fetched, the present cases seemed to show that there was something in it.

Two Cases of a Family Disease, the Symptoms of which closely resemble Disseminated Sclerosis.

By F. E. BATTEN, M.D.

E. S., AGED 27, was first seen in August, 1908. She is the first of a family of six children, three boys and three girls; both her sisters are healthy, but one of her brothers suffers from a similar complaint. Two years ago she first noticed trembling of the hands, which was worse when she attempted to grasp anything. Four months ago she noticed that her legs trembled and would give way suddenly. She also noticed that she was unsteady and would walk from side to side. Lately she has noticed a thickness in her speech. Never any trouble with the sphincters. Never diplopia. Some numbness under both knees lately. Present condition: Memory, attention, good; special senses normal. Articulation slow and hesitating. No nystagmus. No optic atrophy. All the movements of the arms are good, but there is some intention tremor of both hands and some ataxia in the left. All the movements in the legs are good, but there is ataxia when walking. There is slight tremor of the head and trunk. The knee-jerks are active. There is no ankle-clonus. The plantars give a flexor response. The abdominal reflexes are present. There is no sensory loss. There is a very slight lateral curvature. There is no pes cavus.

T. S., aged 25, brother of the above, second in family. Four years ago he had severe and sharp pain in the back extending down the thighs. The gait became unsteady and has gradually got worse. About the same time he noticed shaking of the hands, shaking of the body, alteration in speech, and occasional diplopia. There has never been any bladder trouble and no sensory disturbance, either subjective or objective. He was in the National Hospital four years ago, and since then has been in Guy's, St. Thomas's, and University College Hospitals. No very marked change has taken place in his condition. Present condition: Articulation slow and hesitating. Memory and attention good. There is no nystagmus, but sometimes diplopia. Slight tremor of head. The power of the arms is good, but there is intention tremor on both sides and some inco-ordination on the right. The power of the legs is good;

there is intention tremor on both sides and the gait is ataxic. There is slight Rombergism. The knee- and ankle-jerks are active. There is ankle-clonus on both sides. The abdominal reflexes are present. The plantar reflex is flexor on the left and indefinite on the right. There is no alteration of sensation.

DISCUSSION.

Dr. JAMES COLLIER said that disseminated sclerosis was such a common disease that it would be strange if one did not sometimes meet with more than one case in a family. He had known several instances of two members of a family suffering from disseminated sclerosis. On one such case he did an autopsy at the National Hospital some years ago, and the pathological condition was typical disseminated sclerosis, the plaques of sclerosis being in the ponto-cerebellar region. He had seen several cases the clinical course of which had advanced to a condition to place the diagnosis beyond doubt. If the lesions of disseminated sclerosis were in the outlying portions of the pons and cerebellar peduncles, the symptoms would be similar to those present in these cases. Until there was some striking clinical symptom which did not occur in disseminated sclerosis, or until there was some pathological verification, he would regard such cases as disseminated sclerosis, even though a whole family were affected. Dr. Batten said there was no sign of organic disease, but he (Dr. Collier) regarded the peculiar speech and the intention tremor as conspicuous signs of organic disease.

Dr. GORDON HOLMES said that both the cases seemed to be similar to Dr. Turney's first case, but he did not suppose Dr. Collier would label that one disseminated sclerosis. In Dr. Turney's younger patient the symptoms were very similar, but the knee-jerks were absent, and in the one foot he examined there was typical extensor response, so that he did not see how it differed from Friedreich's disease. If that was so, Dr. Batten's cases were connected by that transition case with Friedreich's disease. He asked whether, in the case of disseminated sclerosis examined post-mortem by Dr. Collier, the patient had ever had any extensor response.

Dr. STANLEY BARNES expressed surprise that Dr. Collier should regard the cases as disseminated sclerosis. Although it was conceivable that two cases should occur in members of one family, yet the occurrence of such a coincidence should make one hesitate to diagnose disseminated sclerosis unless the cases were quite typical. Neither of these patients showed an extensor response, precipitate micturition, or any eye defect, such as nystagmus, transient or permanent diplopia, or optic atrophy, and it was very rare for a patient to show such marked cerebellar symptoms in disseminated sclerosis without having exhibited one or more of these signs. That such a rare type of the disease should occur in two separate individuals in one family was almost proof that the diagnosis was incorrect, and he suggested that in these cases one had to deal rather with a condition of progressive degeneration such as cerebellar

ataxia than with one in which plaques were scattered through the ponto-cerebellar region in the rather haphazard manner to which we are accustomed in disseminated sclerosis.

Dr. WILFRED HARRIS said the two cases resembled very closely two sisters whom he showed before the Section a year ago, whose condition at the time was accepted as cerebellar ataxy. In them also there was no extensor response, nystagmus, nor diplopia, and they were slowly progressive, with articulatory trouble and ataxia of arms and legs. The fact that these cases occurred in the same family was against their being disseminated sclerosis, as also was the fact that there was such weakness without true ankle-clonus, extensor response, or sphincter trouble. Dr. Batten did not seem quite convinced that there was true ankle-clonus, and it seemed to him (Dr. Harris) more like pseudo-ankle-clonus.

The PRESIDENT (Dr. Beevor) said he thought it unlikely that disseminated sclerosis would have gone on so long without an extensor response or loss of the abdominal reflexes. It seemed to be doubtful whether the plantar reflex was extensor or not. If it was flexor, that was against it being disseminated sclerosis. It was unusual for disseminated sclerosis to occur in more than one member of a family, and his opinion was that it was more like a case of cerebellar ataxy.

Dr. BATTEN, in reply, said he had been much interested in Dr. Collier's criticism, though he did not agree with him. He fully recognized that two cases of disseminated sclerosis might occur in a family, and at the present time he had under his care two members of a family suffering with that disease, but in both those cases marked physical signs were present. He did not think that the diagnosis of disseminated sclerosis was justifiable where one could not elicit any sign of organic disease. He would put the present cases into the Friedreich or cerebellar ataxy group, as those two merged very much into one another.

Case of Bilateral Dislocation of Shoulder with Marked Muscular Wasting.

By P. W. G. SARGENT, F.R.C.S.

E. R., FEMALE, epileptic, aged 17, has suffered from epileptic fits since birth. There is no history whatever of injury to shoulders. Weakness of right arm was noticed six months ago. On examination, incomplete forward dislocation of both humeri, with limitation of movement. Marked wasting of right deltoid and spinati. Electrical reactions of these muscles show very marked diminution to faradism and diminution to galvanism, but without polar changes. No sensory changes; reflexes normal. Skiagrams of shoulder shows no disease of bone. All features present in left shoulder, but much less marked.

Gummatous Meningitis involving all the Spinal Roots on the Right Side of the Cord from the Tenth Dorsal to the First Sacral inclusive.

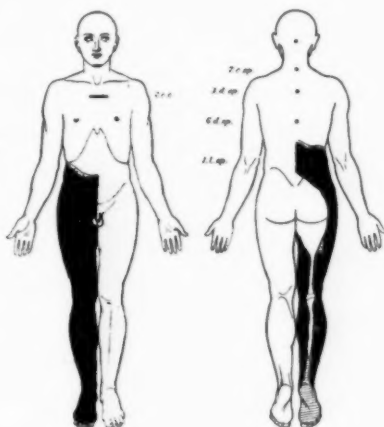
By T. GRAINGER STEWART, M.B.

J. S., MALE, aged 32, was admitted into the National Hospital in September, 1908, complaining of pain, numbness, and weakness in the right leg. Previous health excellent, except that eight years previously he had contracted syphilis followed by well-marked secondary symptoms. Treatment was carried out irregularly for three months. Present illness: In November, 1907, he began to suffer from pain in the front of the right thigh; this continued for some months, and in February, 1908, he complained of pain in the right leg, chiefly on the inner and outer aspects of the calf. The pain increased and was then followed by numbness and weakness of the right lower extremity. When seen in June, 1908, his pupils were unequal and both reacted sluggishly to light. He had weakness of the muscles and sensory loss in the areas supplied by the first, second, third, fourth, and fifth lumbar roots of the right side. There was no sphincter weakness nor was there any evidence of an intramedullary lesion. The sensory loss was relatively much more severe than the motor paresis. A diagnosis of syphilitic gummatous meningitis involving the lumbar roots of the right side of the cord was made and energetic antisyphilitic treatment was carried out. This condition, however, became worse, the pain disappeared, but the sensory loss and the motor weakness increased.

On examination (September, 1908):—Cranial nerves: Pupils unequal, reacting sluggishly to light, otherwise normal. Motor system: The muscles of the lower abdomen on the right side were in a state of mild flaccid paralysis. The right lower limb was weak; at the hip-joint flexion, abduction, and extension were considerably impaired, as was external and internal rotation; adduction was less affected. At the knee-joint the extensors were weak and the flexors affected to a less degree. At the ankle-joint there was no power of dorsiflexion or eversion, inversion was feeble, but extension was good. At the toes the power of flexion was fair, but extension was impossible save to a slight extent in the great toe. The motor system elsewhere was normal.

Electrical examination showed reaction of degeneration in the peronei and tibialis anticus on the right side and diminished faradic excitability

in the other muscles affected. Sensory system: There was no subjective pain but some numbness on the sole of the right foot. Objectively there was complete anaesthesia and analgesia, loss of appreciation of all degrees of temperature, and of vibration in the areas of distribution of the posterior roots from the tenth dorsal to the fifth lumbar inclusive. At the upper limit of the tenth dorsal area there was a zone in which the sensory loss was incomplete. Over the first sacral root area there was a diminution to all forms of sensation. Painful pressure over the tenth, eleventh, and twelfth dorsal and first, second, third, fourth, and fifth lumbar areas was felt only as "something touching him." Sensation elsewhere was normal. Reflexes: The right inferior abdominal



Black indicates areas where sensory loss was complete.

Dots indicate areas where sensory loss was not quite complete.

Lines indicate area where loss of all forms of sensibility was only relative.

cremasteric and plantar reflexes were absent, as were the right knee- and ankle-jerks. The reflexes of the left lower extremity and other parts of the body were normal. There was no sphincter affection.

As the patient's condition progressed steadily despite antisyphilitic treatment, an operation was undertaken to try and free some of the affected roots and possibly to render medicinal treatment more effective. Mr. Armour removed the laminæ from the tenth dorsal to the first lumbar. The dura was apparently normal on the outer aspect; on incising the theca the left side of the cord was exposed and showed no

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evidence of meningitis; on the right side, however, the dura was more or less firmly bound down to the cord by a gummatous meningitis which involved the tenth dorsal root at its exit through the dura and extended downwards as low as the level of the exit of the first sacral root from the cord. The roots were partly freed by dissection and the dura was incised in the intervals parallel to the roots. The wound was washed out with strong perchloride solution, the dura was not sewn, and the external wound was closed. The wound healed perfectly, and anti-syphilitic treatment was again resumed. Some improvement in motor power has taken place, but the the sensory loss remains much the same.

The PRESIDENT (Dr. Beevor) said one rarely found a lesion which picked out certain parts of the cord, from the tenth dorsal to the first sacral, and left the rest of the sacral practically untouched.

Case of Heart-block associated with Remittent Atrophic Paralysis of various Muscular Groups.

By E. FARQUHAR BUZZARD, M.D.

C. W., COMMERCIAL agent, aged 47. Family history: His mother, a brother, and two sisters died of morbus cordis after 40 years of age, with no history of rheumatism. Previous history: With the exception of one slight attack of influenza he has always enjoyed perfect health; no history of syphilis. November, 1905: began to experience increasing difficulty in ascending steps and in raising himself from a seat to the erect position. This was associated with severe cramps in the legs after rest. Christmas, 1905: marked weakness in both shoulders, with gnawing pains in those parts; within a month he was completely incapacitated from work, but during the spring he improved, and by August, 1906, he was able to go to the seaside feeling well, free from pain, and able to walk six miles a day. He then became gradually worse again until May, 1907, when some improvement set in once more. February, 1908: the weakness again became much more marked, and in addition the patient suffered much from coldness and blanching of the hands and feet, as well as from some dyspnoea after exertion. July, 1908: improvement commenced, and has continued ever since.

Condition at end of September, 1908: Well nourished; pale, except for dilated venules on cheeks; hands and feet cold and clammy; capillary circulation very sluggish; radial pulse and apical impulse 35 to 40 per

minute; marked jugular pulsation, especially on the right side, at about twice the rate of the apical beat and radial pulse; apex beat in sixth interspace, $1\frac{1}{2}$ in. outside nipple line, heaving in character, accompanied on auscultation by a faint blowing bruit. Probably some paresis of the orbiculares palpebrarum et oris; cranial nerves otherwise normal, except spinal accessory. Atrophic paresis of the sterno-mastoids, trapezii, and other neck muscles; also of spinati, pectorales, deltoids, and all the upper and forearm groups in varying degrees. Very slight weakness of the recti abdominis, but very little or no impairment of power in the lower extremities, where there is no evidence of atrophy; gait normal; no ataxy; no spasticity. Sensibility, cutaneous and deep, normal. All deep reflexes rather brisker than normal, both in arms and legs. The superficial reflexes are present and healthy in type; no sphincter trouble. The electrical examination of the affected muscles shows polar change in all, and slight R.D. in a few.

During the two months he has been in hospital there has been marked recovery in power as well as considerable improvement in the circulatory conditions, although the pulse has never exceeded 45 per minute.

DISCUSSION.

Dr. FARQUHAR BUZZARD desired to ask what, if any, relation there was between the cardio-vascular condition in that patient and the muscular atrophy, which latter did not conform to any known type. He did not think there was any evidence of the paralysis being due to peripheral neuritis, and it was not the common progressive muscular atrophy of spinal origin, because it had had very remarkable remissions, and had not followed that disease in distribution. Could the cardio-vascular condition cause such an atrophy in a case where something was known to be wrong with the circulation, and was the heart-block due to central or to local influences? Secondly, did the state of the man's circulation give rise to changes in the anterior horn-cells, and thus cause the muscular atrophy? The man had improved in an extraordinary way under treatment, chiefly rest and massage. When he came in he complained very much of cyanosis and coldness of hands and feet, but now that his circulation had improved there were no such complaints. Coincidentally there was improvement in the muscular condition.

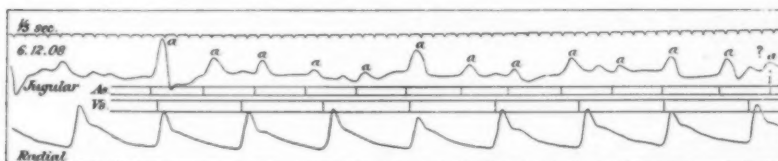
Dr. L. GUTHRIE asked whether Dr. Buzzard could throw any light on the association of muscular atrophy with the heart-block. He mentioned that there were cramps in the legs, and apparently the circulation was bad. It had therefore occurred to him that there might be a condition of obliterative arteritis in different parts. Such a condition might conceivably cause some wasting of muscles, and as circulation became restored those muscles might

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recover. He did not think the atrophy would fit in with a spinal or neuritic condition.

Dr. BUZZARD, in reply, said the theory which Dr. Guthrie put forward did occur to him, but he decided against it chiefly because of the symmetry of the symptoms. It would be curious if, with so many lesions associated with muscular arteries, there should be no symptoms arising from arterial obliteration in any other organ, or even on the sensory side. He did not agree with Dr. Guthrie that they could not be associated with a spinal condition. He contended that the muscles wasting and becoming weak and then recovering fitted in very well with the cell changes, which might presumably take place under the influence of local anæmia. He had been particularly interested in what Dr. Mackenzie said about temporary remissions in the condition of the heart in such cases as these.

The following is a synchronous tracing of the radial and jugular pulse in Dr. Buzzard's case, taken by Dr. James Mackenzie a few days after the meeting, together with his observations upon it.



"The auricle and ventricle pursue independent rhythms—about five auricular to three ventricular beats—implying complete dissociation. This practically puts out of court the possibility of vagus stimulation, because the vagus cannot produce complete dissociation for so long a period. One is forced, therefore, to the conclusion that there is a lesion of the auriculo-ventricular bundle, and if it is connected with the paralysis it must be through some general cause, such as syphilis. I have never seen so big waves due to the auricle; from the intercalated diagrams representing the auricles and ventricles you will see that the big beats fall during some part of the ventricular systole and the small beats during diastole. . . . The first sound of the heart varies in a very interesting manner. It is at times scarcely perceptible, then it becomes very sharp and distinct. This latter always occurs with the big wave in the jugular—that is to say, when auricles and ventricles contract together."

Neurological Section.

January 21, 1909.

Dr. J. A. ORMEROD, Vice-President of the Section, in the Chair.

Death of Dr. Beevor.

THE CHAIRMAN (Dr. Ormerod) regretted that there must be a very melancholy prelude to their proceedings that evening. They could not begin their work without a reference to the great loss they had sustained in the death of their President, Dr. Charles Beevor. That death was appallingly sudden. He presided at their last meeting (December 3) and himself contributed a clinical case to the series shown that night. The next evening (December 4) he represented them, as President of the Section, at the anniversary dinner of the Royal Society of Medicine. That very night he was seized with sudden and severe cardiac pain, and died before medical help could reach him, from failure of the heart, due to atheromatous disease of the coronary arteries. For a long time past his friends had noticed that he looked old, but they had no suspicion of what was coming, and even if he had himself he did not let it overshadow his life or dishearten him for work.

The following resolution, which had been passed by the Council, was unanimously endorsed: "The Council of the Neurological Section of the Royal Society of Medicine desires to express the greatest regret at the death of Dr. Charles Beevor, its President, and to convey to Mrs. Beevor and her family its deepest sympathy with them in their bereavement. The Council feels that by the death of Dr. Beevor, who has been President of the Neurological Society and the first President of the Neurological Section of the Royal Society of Medicine, the science of medicine, and of neurology in particular, has sustained an irreparable loss."

Apoplectiform Cerebral Hæmorrhage. Operation. Evacuation of Blood. Slow Improvement.

By A. E. RUSSELL, M.D., and PERCY SARGENT, F.R.C.S.

THE case which we are about to relate we regard as of importance, as bearing upon the question, already raised by Harvey Cushing in America, of the advisability and possible scope of operations for the relief of cerebral compression due to pathological hæmorrhage.

Mrs. C., a lady aged 54, was seen by one of us on March 12, 1908, in consultation with Dr. H. J. Wheeler, of High Wycombe. She had previously been a particularly healthy and vigorous woman. On March 2 she was seized with apoplexy. The onset was very sudden, and unconsciousness was profound. No improvement had occurred. On examination ten days after the onset the patient was unconscious with complete right-sided hemiplegia. The pupils were contracted and equal, and there was slight conjugate deviation of the head and eyes to the right. Breathing was stertorous in character. The general appearance was strikingly good considering the duration of the symptoms. When disturbed she would move the left arm, to some extent in a purposive manner. The pulse-rate had varied from sixty to seventy per minute. The blood-pressure, estimated by Martin's sphygmomanometer, stood between 190 mm. and 200 mm. Hg. The heart was not obviously enlarged, no murmurs were present, but the aortic second sound was markedly accentuated. The peripheral arteries showed no signs of disease. Urine: specific gravity 1012, with a slight trace of albumin. The bowels had been extremely constipated. Optic discs: the outlines were hazy, but white rather than pink in colour. The veins were not obviously swollen, and no hæmorrhages were seen. Lumbar puncture was performed, but no fluid was obtained. The long duration of the coma, the almost complete absence of any signs of improvement, and the healthy state both of the cardio-vascular system and of the patient generally, coupled with the high blood-pressure, suggested the possibility of affording relief by means of a decompressive operation.

We therefore saw the patient together on the following day. Her condition was unaltered, and, taking all the circumstances into consideration, we were of opinion that operation afforded the best means of saving her life.

Operation : Chloroform and oxygen were administered with a Vernon Harcourt inhaler by Dr. E. W. Hedley. A large osteoplastic flap was turned down over the left parietal region. The dura was fairly tense. On incision there was only slight bulging of brain tissue, but sufficient to indicate a moderate increase of intracranial pressure. The surface of brain was slaty in colour, and the veins were remarkably full. The convolutions were flattened and pulsation poor. A trocar plunged downwards, inwards and forward through the post-central gyrus encountered diminished resistance at a depth of $2\frac{1}{2}$ in. Along this a few drops of dark, thick, treacly blood exuded, exactly like the fluid contents of an old pelvic hæmatocele. An incision was made through the cortex, and more blood of the same nature escaped. The finger was then gently inserted, and found a cavity which felt about the size of a walnut, from which some 3 dr. of altered blood were allowed to escape. The dura was replaced, and the wound closed without drainage.

During the operation observations on the blood-pressure were taken at intervals, and were as follows :—

Before administration of anæsthetic	190 mm. Hg.
Under anæsthetic before commencement of operation	174 "
Osteoplastic flap ready to turn down	148 "
Flap turned down	116 "
Dura opened	126 "
At completion of operation	128 "

The pulse-rate at the completion of the operation was 88, regular, and of distinctly lower tension and better volume than before. There were no symptoms of shock whatever.

Subsequent course of case : For two or three days there was a considerable discharge of blood-stained cerebro-spinal fluid ; the wound healed by first intention, and the stitches were removed on the tenth day. On the fourth day after operation she recognized her daughter. On the seventeenth day after operation we again saw the patient together. The upper border of the bone-flap was slightly displaced outwards, and there was a distinct bulging at the site of the trephine hole. She was conscious, but completely aphasic, only saying "Yes" on one occasion. The paralysed right arm was œdematous. Blood-pressure in the right arm was 170 mm. Hg.; in the left arm 140 mm. Hg. A month after the operation she was removed to London, and since then has been under the care of Dr. M. M. Townsend at Hammersmith. Shortly after this, on April 20, we again saw the patient. The general appearance was that of good health. Appetite good. No return of

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power or of speech. Blood-pressure, 240 mm. Hg. in the paralysed and œdematous right arm; 196 mm. Hg. in the left. The bone-flap was now even more prominent, but there was no hernia cerebri. On June 30 the condition was as follows: Blood-pressure, 190 mm. Hg. on the healthy side. The right arm was too œdematous for accurate observation. No return of power in arm or leg; there were hardly any signs of facial weakness. The bone-flap was perhaps less prominent. The patient was clearly more intelligent, but apparently understood very little of what was said to her. She could say several words, and could now read simple words slowly and with difficulty. She was rather emotional. On October 12 patient was sitting up in a chair looking very well. No return of power. Evidently understood something of both spoken and written words. Tendency to repetition of certain words such as "Oh, yes"—"better." On December 16, nine months after the operation, there was slight return of power in the right leg, which could be raised from the bed. Patient could understand printed words and could read them, though with imperfect enunciation.

COMMENTS.

The only cases of operation for apoplectic cerebral hæmorrhage that we are aware of were reported by Harvey Cushing.¹ In one case operation was performed forty-three hours after onset of symptoms in a negro aged 40. The blood-pressure at the operation was 360 mm. Hg., and dropped to 230 mm. Hg. after evacuation of 3 dr. of old discoloured semi-fluid blood-clot. The major symptoms were immediately relieved by the operation, but death occurred on the third day. Cushing refers briefly to an earlier case of Professor Osler's, in which an osteoplastic flap was elevated, but in which apparently the site of hæmorrhage was not sought for. The patient was almost *in extremis*, but the symptomatic relief was striking, blood-pressure falling to an approximately normal level, at which it remained until death from pulmonary complications thirty-six hours later. At the autopsy a large clot the size of a hen's egg was found closely underlying the cortex and in exact correspondence with the opening in the skull. No further details are given concerning this case.

In commenting on our case we feel that certain questions for discussion naturally present themselves. Was the patient's life saved

¹ "The Blood-Pressure Reaction of Acute Cerebral Compression," *Amer. Journ. Med. Sci.*, 1903, cxxv, p. 1017.

by the operation? It must be remembered that coma had been present for ten days, and there had been no signs of improvement. Recovery, though not impossible, is probably uncommon after so long a period. Our own opinion is that the operation did save her life, but we should like to know the experience of members of the Society as to the frequency of spontaneous recovery after such prolonged coma. On the assumption that the patient might have survived without operation we have to consider:—

(a) Whether the operation hastened recovery from the apoplectic stage.

(b) Whether the ultimate improvement is greater than would have occurred without the operation.

Despite the relief of intracranial tension no striking alteration occurred in the patient's condition, but rather a very slow improvement.

We can hardly doubt that had the compression been allowed to continue longer those tracts that had escaped actual destruction by the hæmorrhage would have required a longer time before regaining their normal conductivity. The possibilities of improvement in these cases must, of course, depend upon how much of the motor tract has escaped actual destruction; this is impossible of determination, and it may be that in our case the limited improvement is the greatest attainable.

GENERAL CONSIDERATIONS.

Granted that operation is justifiable in a few selected cases of apoplectic hæmorrhage, the factors that should govern us in arriving at such a decision must be considered.

(1) *Correctness of Diagnosis.*—It is unnecessary to enter into details, but cerebral thrombosis, embolism, &c., should be carefully excluded. In certain cases this is a matter of the greatest difficulty, and we should not go so far as to suggest exploratory operation in a doubtful case.

(2) *General Condition of the Patient.*—The factors of age, physique, cardio-vascular and renal condition would in many cases at once negative operation.

(3) *Type of Case suitable for Operation.*—Clearly the milder types of case with early signs of improvement do not require surgical intervention. Cases with marked bilateral symptoms indicative of pontine or ventricular hæmorrhage also scarcely come within the scope of surgery. The strictly unilateral symptoms indicative of a localized hæmorrhage in the white matter, not flooding the ventricles, with deep coma and high

blood-pressure, appeal to us as most favourable for such treatment. Prolonged coma, with little or no improvement, as in our case, would also seem to be evidence of such increased general intracranial pressure as to demand relief. Extreme urgency of symptoms and even the apparent imminence of death may be an indication rather than a contra-indication.

TECHNIQUE OF OPERATION.

We do not wish to enter into such details of surgical technique as are common to all cerebral operations, but rather to raise certain special points suggested by a survey of our case.

(1) *Operative Objective.*—If it be granted that in certain selected cases we are justified in attempting to relieve the cerebral compression due to pathological hæmorrhage by operation, are we to perform only a decompressive operation, or are we to make an attempt to evacuate the blood and clot at the same time? It would seem reasonable to suppose that the more rapidly relief from the local compression of those motor fibres that have escaped destruction can be attained the better will be the chances of recovery, and on this ground, therefore, we should be in favour of attacking the site of the hæmorrhage, and not merely performing a decompressive operation. This, however, necessitates an incision through healthy cortex, and also the possibility that the intracerebral manipulation may increase the destruction of the corona radiata fibres. How far this is likely to affect the case is difficult to say. At the time of operation it is not possible to differentiate between the palsy due to destruction and that due to compression of the motor fibres. Whether the damage inflicted by incision of the brain is disproportionate to the advantages derived from the more certain and more rapid recovery of those fibres which are only physiologically interrupted is another difficult question.

(2) *Route of Approach.*—In our case the cortical incision was made through the post-central gyrus, and the cavity of the hæmorrhage was found with great ease. We have been unable to test the patient's stereognostic sense, and even had that been practicable it would have been impossible to say whether the operation had affected the sensory loss at all. We do not think that a clean-cut incision through this part of the cortex, followed by the gentlest manipulation through it, could materially affect the case.

(3) *Method of Dealing with the Clot.*—The next question which arises is whether any attempt should be made to clear out the clot as

well as the fluid part of the hæmorrhage. To this question we give a negative answer. We do not think it would be possible to differentiate by the finger between clot and cerebral tissue, and certainly any attempt at removal of clot would seriously augment the chances of increasing the damage, to say nothing of the risk of restarting the hæmorrhage. Further, we are unaware of the rapidity with which the effused blood coagulates. Recent soft clot would probably be extruded with comparative ease, but with operation in a later stage, as in our case, any firm clot adherent to the wall of the cavity might be impossible of differentiation or of removal, the serum alone being accessible.

(4) *Irrigation*.—On the question of irrigation of the cavity we have an open mind. We did not irrigate in our case, but we think that such a procedure might have enabled us to remove more of the treacly blood than we did without any increased damage to the surrounding brain tissue.

(5) *Drainage*.—On general grounds drainage is objectionable as inviting septic infection from the exterior, nor can any form of drain be brought from the cavity of the hæmorrhage through the cortex to the surface without increasing the damage to normal brain tissue. Apart from these two serious objections we think that drainage might be advantageous. In our case there was evidence of some increase in the intracranial tension after operation. This might have been obviated by drainage, and possibly the ultimate result might have been better.

DISCUSSION.

Dr. WILFRED HARRIS said he thought the authors were to be congratulated on their good result in this case, but it was open to question whether the patient would not have recovered without operation. The amount of bulging of the brain after the opening of the dura having been small he regarded as evidence that the patient would probably have recovered without the incision. Five years ago he had to deal with a case of the same nature, that of a medical student, aged 25, who had had a slight stroke. He had been taken ill one night, had vomiting, and probably he had been unconscious. He was sent to Dr. Harris because the vision was wrong; he had incomplete hemianopia on the left side, otherwise he appeared well; no hemiplegia or anæsthesia detectable. The hemianopia slowly improved for a fortnight, and then he began to suffer from severe headache, which became rapidly worse, so that when admitted into the hospital he was almost delirious with pain. His discs began to swell, and flaming optic neuritis developed, and coma was threatening. He was not suspected to have cerebral hæmorrhage, but on the chance of finding the cause of the increased intracranial pressure, Mr. Pepper trephined

him, opened the dura, the brain ruptured, and there came out $1\frac{1}{2}$ oz. of blood-clot. The result was extraordinarily good, and certainly the patient would have died but for the operation. He had remained well ever since, and has married recently.

Mr. C. A. BALLANCE congratulated Dr. Russell and Mr. Sargent on the case, and he hoped they would be able to bring before the Section at some future time records of a series of such cases. What appealed to the surgeon was the cutting down directly on the bleeding spot. It was true that the bleeding was arrested at the time of operation in Mr. Sargent's case, but it might be possible in the future to cut down on the bleeding spot at an earlier stage. He remembered a paper which was published by Mr. Spencer and Sir Victor Horsley in the *British Medical Journal* in 1889, in which a series of experiments were described in which the common carotid artery was tied or compressed. It was shown then that when the common carotid artery was compressed, hæmorrhage from the middle cerebral was immediately arrested. Some of those experiments he (Mr. Ballance) witnessed, and it was very striking. One of the suggestions of Spencer and Horsley was that in ingravescent apoplexy compression or temporary ligation of the common carotid should be carried out, and he wondered whether that plan had ever been adopted in cases of apoplexy. He thought that possibly a combination of the method recommended by Horsley and Spencer twenty years ago, with direct incision on to the bleeding spot in certain typical cases of apoplexy, might be of great service. The arrest of the hæmorrhage by the temporary ligature and the exposure of the bleeding point would almost certainly lead to the permanent arrest of the hæmorrhage, because it did so in other parts of the body. With regard to the method of approach, Cushing thought that the osteoplastic flap should not be used in these cases. When Mr. Ballance was in Baltimore he discussed the operation with Cushing. Cushing recommended strongly the subtemporal mode of approach, and Mr. Ballance agreed that Cushing's view was probably correct. He had used it in analogous cases of fracture of the base, where the patient was desperately bad, and was really suffering from intradural hæmorrhage. In such cases, and in those of hæmorrhage from the lenticular striate artery, the subtemporal mode of approach seemed to him the best. The drainage of these cases could easily be carried out into the space beneath the temporal muscle, and the scalp wound completely closed, so that there would be no risk of any septic untoward occurrence afterwards.

Dr. F. E. BATTEN said it was with a feeling of disappointment that he heard the history of the case, for he had hoped that the word "recovery" would have indicated that the patient had been greatly improved by the operation. The patient had, however, not only complete hemiplegia, but had also persistent aphasia in spite of the fact that the original lesion was probably capsular. He thought the advantage of lowering the blood-pressure in certain cases of cerebral hæmorrhage was certainly great, and it was lowered by the operation; but surely that could have been performed in a more simple manner.

The operation necessitated the passage of the finger through the cortex in order to evacuate the cavity in which the blood was situated. He (Dr. Batten) had had the opportunity of examining more than one brain in which the finger or a trocar had been passed in through the cortex, and the pathologist knew that the destruction of brain tissue extended far outside the track of the trocar or finger. Even if the operation did not increase the hemiplegia, it was almost certain that the damage to the cortex would render the aphasia more permanent. Of course, such a case was a triumph for the surgeon who performed it.

Dr. A. E. RUSSELL, in reply, said that it was impossible to say that the patient could not have recovered without operation. She had been comatose a clear ten days, and he took it that the longer the condition lasted the more likely was the patient to die. If after ten days' coma there was not much likelihood of recovery, the operation was justified. He agreed that the moderate degree of bulging of brain at the operation showed the possibility of spontaneous recovery, but they could not tell before the operation how much bulging there would be. The case of the student narrated by Dr. Harris was very interesting, and, as far as it went, it was in favour of operation. In regard to Mr. Ballance's suggestion of early operation by combined digital pressure of the common carotid, and trephining at the same time, it was difficult to say in the early stages whether a case would be severe. If this could be determined it would be the ideal operation. He had just heard from Dr. James Taylor that cases of coma from cerebral hæmorrhage could go on for six weeks. If there was a probability of recovery after much prolonged coma, it might be wise not to operate at the end of so long a period. The criticism of Dr. Batten as to injury of surrounding brain tissue by the finger was valuable, and might militate against digital exploration of the cerebral cavity, but he took it that mere insertion of the knife or of the trocar need not produce injury out of proportion to the possible benefits from the operation, and it had to be remembered that the types of case recommended for operation were of great gravity and not likely to recover in any case without severe disablement.

Mr. SARGENT, in reply, said that he had little to add to Dr. Russell's remarks. With regard to the method of approach, his experience of the subtemporal route had not been encouraging, as the opening which it afforded was not sufficiently large for the relief of such high degrees of intracranial pressure as were met with in cases of tumour. In such cases, however, as the one now under consideration, the intracranial pressure was of a lesser degree, and therefore the subtemporal route would, as suggested by Mr. Ballance, probably be the best. In answer to Dr. Batten's criticism, he presumed that in the cases which had lived for six weeks or more in a comatose condition, the term most properly applicable would be survival rather than recovery, and perhaps that term would be preferred by Dr. Batten for this case also.

A Case of Thrombosis of the Left Posterior Inferior Cerebellar Artery.

By S. A. KINNIER WILSON, M.B.

OF various medullary syndromes that are found to present a more or less constant clinical picture, that occasioned by a lesion of the posterior inferior cerebellar artery is certainly one of the most striking. With the exception, however, of a case recorded by Dr. Ormerod—where the complexity of the symptoms indicates that the lesion was not confined to the artery in question—and others reported in America by Hun, Thomas, and Spiller, there does not appear to be any adequately analytical examination of the clinical features of this condition in the English literature. Though its occurrence is a rarity, a sufficient number of instances, corroborated by subsequent pathological investigation, have been published to justify its description as a definite syndrome resulting from a definite vascular lesion, a syndrome which is commonly so well pronounced and so sharply defined as to render its recognition easy. For here we have, in a word, hemiataxia, unilateral disturbance in the distribution of the eighth, ninth, and tenth cranial nerves and their centres, disturbance of the cervical sympathetic on the same side, and a crossed dissociated anaesthesia, the latter a dominant feature in the picture, and one which must be examined with that minuteness which recent developments in the field of sensation demand.

The posterior inferior cerebellar artery (fig. 1) arises from the vertebral artery on the ventral or anterior aspect of the medulla oblongata, and winds round it obliquely between the roots of the hypoglossal nerve and then between the roots of the spinal accessory and vagus, into the vallicula of the cerebellum [Cunningham]. The trunk gives off small branches to the medulla, which are end arteries, but the terminal branches anastomose freely on the cerebellum with the similar artery of the other side and with the superior cerebellar. The distribution of the vessel is variable, and in some cases of the syndrome it may be difficult to exclude involvement or partial involvement of the vertebral artery. As the artery courses round the medulla, it sends in short terminal vessels to the lateral aspect of that structure,

and these are the vessels which suffer most and recover least in a lesion of the main trunk, since they have no anastomosing connexion with other arteries. The constancy with which the residual destruction is confined to a limited area in the lateral aspect of the medulla is remarkable. Speaking roughly, this area extends across the medulla dorsally from the inferior olive to the restiform body. The nuclei on the ventricular floor have a double arterial supply, from the anterior spinal artery and from radicular arteries, derived from the vertebral and passing along the nerve-trunks to their origin; hence they do not suffer to the same extent. The anterior spinal artery, further, supplies the pyramids and the median part of the bulb, areas which

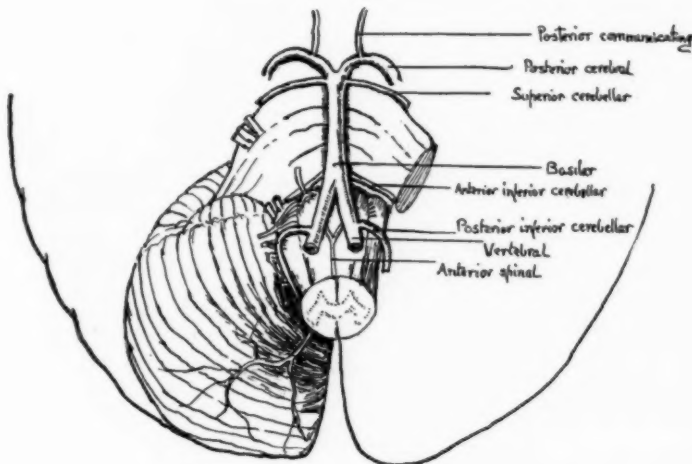


FIG. 1.

Diagram of the arteries of the pons and medulla (modified from Cunningham).

escape in a pure posterior inferior cerebellar lesion. These points are sufficiently indicated in the accompanying diagram from Van Gehuchten (fig. 2).

The vertical extent of the necrosed region is usually from the level where the restiform body begins to pass into the cerebellum above to the level of the middle part of the hypoglossal nucleus below. The sixth and seventh nerves are rarely involved; the affection of the eighth and of Deiters's nucleus is usually transient; the eleventh and twelfth

are also unaffected, and in a pure case there is a conspicuous absence of pyramidal symptoms. It is very instructive to note how after the initial widespread disturbance of function the clinical symptoms resolve themselves into a more or less permanent grouping, indicative of the limitation of the change to an area not reached by anastomosing vessels.

It may be said that the structures which are most commonly involved, partially or wholly, are the reticular formation and its nuclei, the descending root of the fifth and the cells in association, the nucleus ambiguus and vago-glossopharyngeal nucleus, the glossopharyngeal and vagus nerves, the ventral spino-cerebellar tract, and

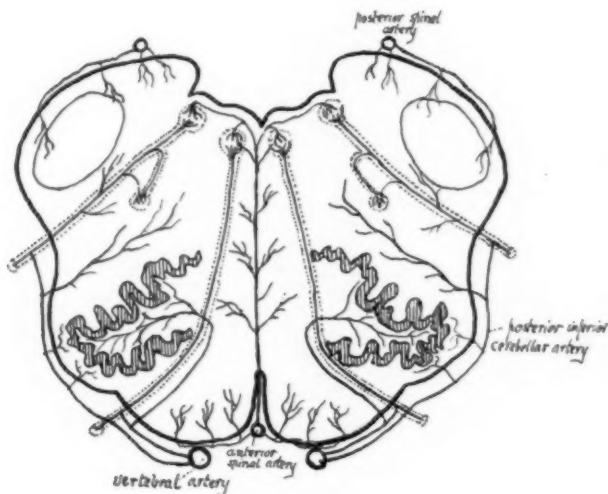


FIG. 2.

Schematic representation of the arterial supply of the medulla (after Van Gehuchten).

the dorsal spino-cerebellar tract and other fibres passing into the inferior cerebellar peduncle; I shall have occasion to mention other structures at a later stage. The accompanying composite diagram has been prepared to give a general idea of the lesion in its greatest extent, as determined by the results of various autopsies (fig. 3).

In the following paragraphs is given in detail the clinical history of a striking and characteristic case, which has been followed and examined frequently for almost five months, which during that time

has passed from an initial stage of widespread disturbance to a second one of more or less unchanging symptoms, and which finally, in the course of the last three weeks, has commenced to show unmistakable signs of recovery. I cannot refrain from stating that I have had the advantage of examining a highly intelligent patient, whose introspection has been astonishingly good, and who, moreover, has discovered by experiment on himself one or two facts which might otherwise have been missed, the significance of which will be at once recognized when they are mentioned subsequently. To Dr. Grainger Stewart, under whose care the patient is attending the National Hospital, Queen Square, I wish here to express my indebtedness.

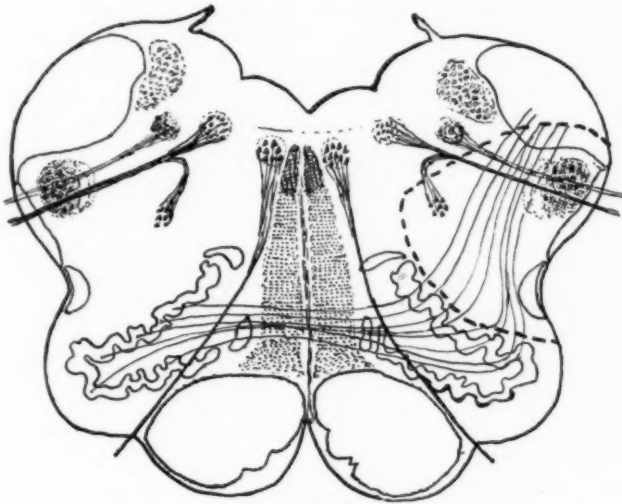


FIG. 3.

Composite diagram to indicate the structures of the medulla commonly involved in the lesion—indicated by the dotted line—as found by pathological examination.

W. M., aged 42, a monumental mason by trade, has been married nineteen years and has eight healthy children. His wife has had no miscarriages. He is a well-developed, active and intelligent man, who denies venereal risk.

History as given by patient on September 17, 1908:—

On Friday, August 21, 1908, he was perfectly well. On Saturday, August 22, about twelve noon, he suddenly felt faint and giddy, and a

flush came over the right side of his face and neck. At the same time the stone at which he was working appeared to be swinging quickly from side to side. He stopped and walked home, a five minutes' walk. During this short walk he kept falling over to the left, and knocked against objects on his left. He had a feeling of being actually pulled to the left. He does not remember having any more definite sensations of rotation. As he walked home he found that the right side of his face was sweating profusely, while the left was quite dry. He lay down for about an hour, feeling inclined to lean over to the left, and noticing that objects in front of him were blurred, but not of double outline. He then went back to work for two hours, during which time he felt himself constantly edging to the left, while the stone again appeared to be swinging from side to side. At 6 p.m. he got on his bicycle and at once found that he was riding into the kerb on the left side of the road. He could not keep the handle-bar straight. He ran into a van on the left, but fortunately was not hurt. On reaching home he ate a hearty tea, which, however, he had considerable difficulty in swallowing—"I had to swallow each mouthful twice before it would go down"—and then went out with a stick. He felt very helpless on his left leg, which kept "giving way," so that he soon returned. He noticed that the conjunctiva on the outer side of the left eye was a little blood-shot. His wife, further, noticed that the left upper eyelid was drooping and that the left pupil was smaller than the right.

On Sunday, August 23, he was seen by Dr. Featherstone, of Tooting, through whose kindness the patient was sent for examination, and who wrote as follows on September 15, 1908: "Mr. M. came under my attention on August 23, suffering from a suddenly developed ataxia. He was absolutely unable to stand erect, falling always to the left. His left eye had a tendency to diverge to the left, and there was diplopia and much unsteadiness (nystagmus). The left pupil was contracted and the right dilated, but both reacted to light. The whole left eye had an appearance as if sunken in the orbit, probably somewhat exaggerated by a swelling of the facial and temporal regions on the left side. The left conjunctiva showed a slight suffusion of blood. The left fifth cranial nerve, especially the ophthalmic and superior maxillary divisions, was evidently markedly affected from the alteration in sensation—the areas felt hot and painful and were tender to the touch. Alteration in taste was marked on the left side only; things felt hot and air felt hot in nose and mouth; there was no

affection of the facial muscles, and hearing was perfect; muscular grip was good; reflexes were slightly diminished; no ankle-clonus or Babinski; pulse 48, slow, full volume, and soft; respiration slow, no stertor; temperature 96°. He had periodic attacks of violent vomiting, which passed away in a couple of days. The right side of his face was bathed in perspiration, the left side was perfectly dry."

The rest of the history as given here was supplied by the patient. On the same day, Sunday, August 23, he experienced a humming, buzzing noise in the left ear, which continued all that day and the next; there was no deafness. The difficulty in swallowing persisted and remained a feature of the case for at least two months. When lying in bed on his back, the wall being on his left side, he noticed that it seemed to be leaning over him as though it were going to fall on him; the bed seemed raised about 2 ft. on the left side, and he felt he must therefore roll out to the right, but when he sat up he always fell to the left. There was tingling and a feeling of numbness over the whole of the left half of the face, which has continued. When he was drinking warm cocoa it felt natural on the right side of the mouth, but "ice cold" on the left. As he breathed the air seemed cool on the right side and warm on the left. Cold fluids, on the other hand, such as milk and soda, felt natural on the right side, but gave rise to a "burning, prickling" feeling on the left. Pressing the tongue against the left lips produced a hot, burning feeling, with prickling; against the right the sensation was normal. He found that the left face was intensely tender when it was being shaved; the sensation was one of "pricking heat." The inco-ordination of the left arm and leg was very marked; he could not put the left leg where he wished—he had to "throw" it; he could not control the left arm. On looking to the left he had diplopia, the upper image being the left one, which seemed about 4 in. to the left of the other; on looking further to the left, or up and to the left, the images went farther apart. The left nostril bled twice spontaneously during the first few days. There was intense headache on the left side, front and back, and a most painful smarting of the left eye; the headache appeared somewhat relieved by the nose bleeding. His voice was thin and inclined to be a little high-pitched, instead of showing its normal rich timbre; he is a bass singer in a church choir, but on attempting he was quite unable to produce any volume of sound.

During the next week or two all the more acute symptoms passed off. Thus the vomiting and malaise disappeared; the sensation of

falling and the forced movements became less intense; the diplopia was rather less marked; the asynergy of the left limbs less obvious. On the other hand, he noticed on attempting to stand that the right leg had somehow "changed"; he was a little uncertain on it, and this coincided with the spread of the "subjective" sensations. The whole of the right leg to the groin became numb and dead; "it felt like leather," but on touching, and still more on rubbing it, a definite sensation of "hot pins and needles" was experienced. The change was at first confined solely to the leg. He noticed that whatever was the actual temperature of the object pressed against the leg it produced the same warm sensation; "a cold earthenware vessel put against the right leg felt as warm as toast." On increasing the pressure the sensation became one of painful heat, which radiated slightly; this was always easily obtained by stroking or rubbing the right leg. It felt to the patient exactly as did the sensation from similar stimuli applied to the left half of the face. He was never able to convince himself that the actual temperature of the leg was above that of the left.

When I first saw the patient, on September 16, he presented the following points: The left side of the face was dry, the right was bathed in perspiration; the left eye was sunken, with a small pupil and some ptosis; these are sufficiently indicated in the photographs (figs. 4 and 5). Both pupils reacted to light, but the left scarcely dilated to shade at all; the optic discs were normal; there was rather quick, slightly irregular but well-sustained nystagmus on lateral deviation to the left, not in any other direction; the visual axes were parallel. Some slight inco-ordination of the left arm and leg persisted, but it was very slight. Hearing was normal; tinnitus was absent; there was no "subjective" sensation of rotation, but on walking the patient tended to deviate to the left, and I thought showed a tendency to bring his left shoulder forward. Occasionally he rather lurched to the left, and when that occurred he said he felt "pulled" to the left. As he sat he maintained his equilibrium comfortably. There was no facial asymmetry or weakness; the motor fifth was normal on the two sides. The palate was definitely weak on the left side; the left vocal cord was paralysed; the tongue was protruded straight. There was no sign of motor weakness in the arms; no tremor was present in either; the grasps were good and equal. The right leg, however, seemed slightly less strong than the left, but the deep reflexes were present and equal on the two sides; no ankle-clonus was found, and

the plantar response was double flexor. The abdominal reflexes were brisk and equal.

The alterations in the domain of sensibility were of peculiar interest.

(A) Reference has been made to the "subjective" sensations of heat and tingling over the left face and right leg. These had now spread, so that the right leg, right lower trunk, and right arm almost to the shoulder exhibited the phenomenon when pressed slightly, or

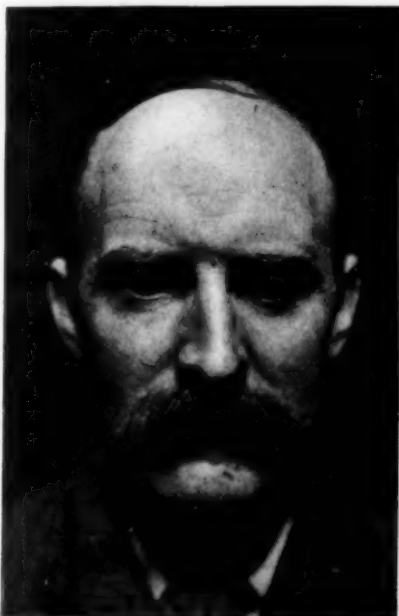


FIG. 4.

Lesion of left sympathetic—ptosis, exophthalmos, and small pupil.

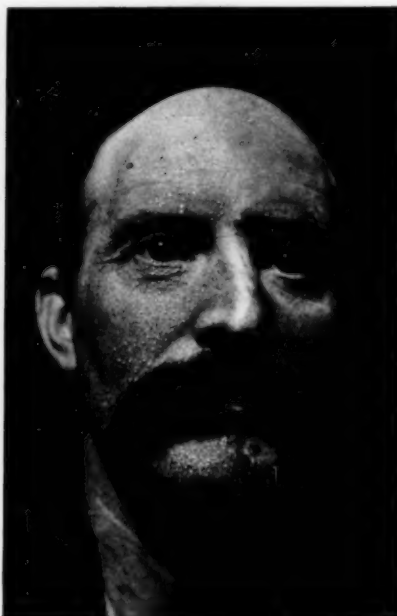


FIG. 5.

Sweat-beads on right side of face only.

stroked, or rubbed. The sensation thus obtained on the left face and right limbs was one of "tingling warmth," of "hot pins and needles," apparently dependent for its intensity on the amount of pressure used; light pressure produced a pleasant feeling of warmth, stroking or rubbing intensified this and introduced the tingling, while squeezing the limb caused both to be accentuated. Exactly the same condition obtained on the left half of the face, where there was no

radiation of the sensation thus obtained (fig. 6). The patient remarked spontaneously that "he liked when the wind blew; it made his right side and his left face so warm."

(B) The objective sensory changes were of the following description (September 16-19):—

(a) *Touch*.—This was tested with cotton-wool. It was found to be entirely normal over the whole body, in particular the right side and

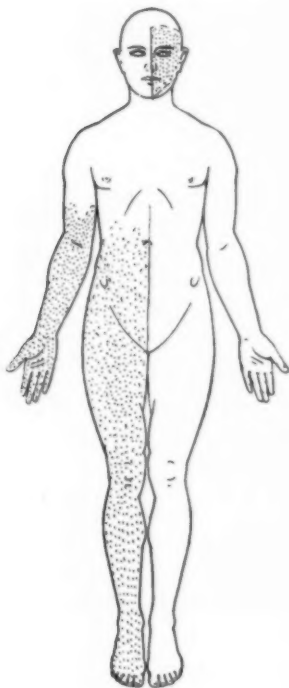


FIG. 6.

"Subjective" sensations.
(September 19, 1908.)



FIG. 7.

Loss to cutaneous painful stimuli.
(September 19, 1908.)

right limbs. Over the left trigeminal area the patient felt the lightest touches, but he said they were not so "tickly" as on the right side of the face.

The localization of touch was everywhere perfect. There was no astereognosis. If the touch passed into pressure, however light, it gave

rise on the right limbs to the "tingling warmth" already referred to, as also on the left face.

The left corneal reflex, tested by touching the corneo-scleral junction with cotton-wool, was not so brisk on the left as on the right. Cotton-wool in the left nostril was not so "tickly" as on the right, but it was felt, and similar touches over the mucous membrane of the mouth and throat on both sides were felt and localized.

(b) *Deep Sensibility*, tested with the rounded end of a pencil, was practically identical on the two sides of the body. (For algometer results see below.) The localization was perfect.

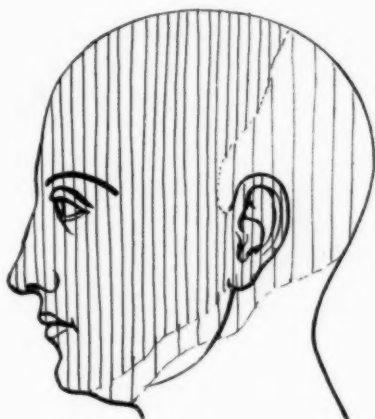


FIG. 8.

Loss to painful cutaneous stimuli over left face. It extends a little beyond the ordinary "peripheral" trigeminal area. (September 19, 1908.)

(c) *Muscular Sense*.—Series of passive movements of flexion and extension were carried out on both sides of the body, and the patient was always correct in appreciating the movement. He was able to imitate with accuracy on either side any movement passively made on the other.

(d) *Cutaneous Pain*.—There was absolute loss to pain over the whole of the left face, and over the right half of the body as far as the clavicle (fig. 7). Over these areas pain was felt as touch or pressure merely. The localization of the stimulus was perfect. In the left side of the mouth, tongue, palate, fauces, uvula and pharynx, sensibility to pain

was lost. The left facial area where sensibility to pain was abolished extended over the pinna, front and back, but corresponded otherwise to the trigeminal distribution (fig. 8). It is interesting to note that a distribution such as this is never obtained by a lesion of the trunk of the trigeminal nerve or by destruction of the Gasserian ganglion. Pressure on the globe of the eye on the left side was not painful.

(e) *Temperature*.—There was loss to all degrees of heat and of cold over the left face, from very cold water (not iced) to water at about 50° C. The application of the tube simply gave rise to a sense of pressure or touch, without any "tingling warmth." If the cold tube, however, lay on the skin a moment or two it sometimes gave rise to a feeling of warmth, never of tingling. On the whole of the right side, including the face, all degrees of heat were felt as "tepid," or "slightly warm," and they did not give rise to any additional sensation of "tingling" (fig. 9). If the tube were pressed against the skin, however, this tingling or smarting was felt. On the whole of the right side, including the face, there was complete loss of sensibility to all degrees of cold (fig. 10). But the mere application of the cold tube at once produced the feeling of warmth or of smarting heat, apart altogether from the question of pressure. If the cold tube was pressed hard against the skin the sensation was intensified. Thus on the right side the cold tube never gave rise to a sensation of coldness, but always to one of warmth, passing into smarting, whereas the hot tube always gave rise to a sensation of slight warmth, which did not become tingling unless the tube was pressed against the skin. The localization of the stimuli was always correct.

(f) *Tactile Discrimination* was not tested at this stage.

During the next month or two the patient's condition remained much the same. He was able to be up and about, and to attend to business. The "subjective" sensations were as before. He still showed a certain tendency in his gait to deviate to the left, and remarked that any sudden change of position, such as lying down in bed too quickly, produced a momentary disturbance of equilibrium, a momentary "all-gone-ness" which was particularly disagreeable, and made him put out his arms involuntarily, as if to save himself, he did not quite know from what. Further, after walking perhaps a hundred yards, especially in cold weather, he felt some difficulty in breathing, accompanied by a degree of tachycardia which necessitated his resting for a minute or two. This disturbance of heart and respiration remains. His wife told me that at night his respiration became usually quiet—indeed, inaudible

—till there came a few deep long-drawn inspirations, sometimes with sighing, followed again by a quiet interval. This phenomenon continues, but it does not seem to occur during the day, and the patient is unaware of it. I am assured that this condition was never observed previous to the illness.

On objective examination (November 16-19), a slight change was found in the field of sensibility. The loss to painful stimuli had appar-

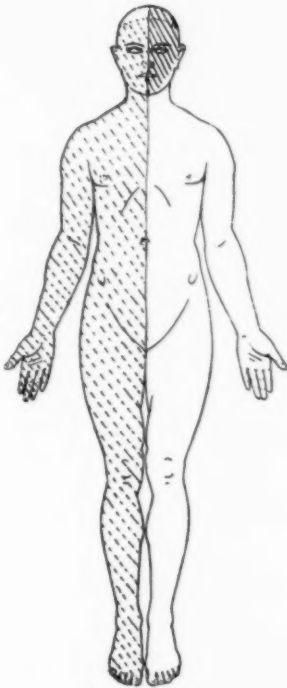


FIG. 9.

Loss of appreciation of all degrees of heat over left face; on right body all felt as "tepid." (September 19, 1908.)

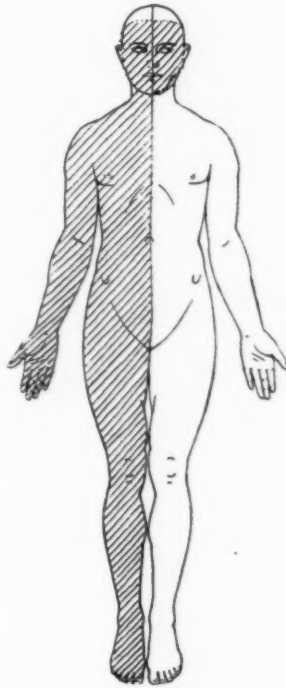


FIG. 10.

Loss of sensibility to all degrees of cold. (September 19, 1908.)

ently extended up to the margin of the trigeminal area on the right side. (It is conceivable that this limit was the correct one, and that it may have been missed at the first examination.) It was difficult to determine whether the line corresponded to a true trigeminal limit, or

whether it was not rather of a segmental type, as opposed to a peripheral. The alteration is indicated in fig. 11. Taste was carefully tested all over the tongue. There was complete loss over the left half of that organ, anteriorly and posteriorly (salt, sugar, quinine). There was no loss or diminution to touch inside the mouth on the left side, and the palatal and pharyngeal reflexes were obtained readily enough on either side.

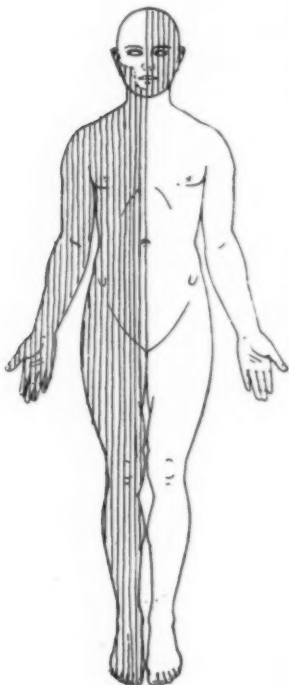


FIG. 11.

Pain of cutaneous origin.
(November 19, 1908.)

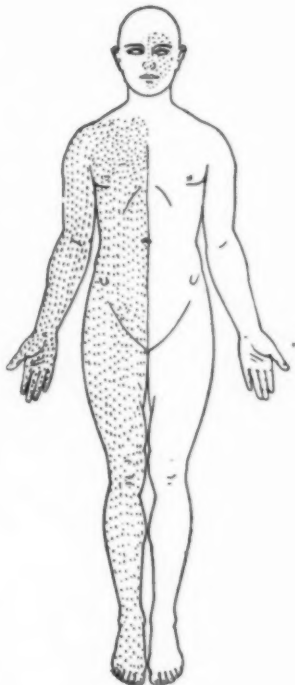


FIG. 12.

"Subjective" sensations.
(January 17, 1909.)

Pinching the tongue in a pair of dissecting forceps seemed to produce pain as readily on the left side as on the other.

From this point a stage of definite improvement is entered on. The second week of January, 1909, was spent in re-examination of the patient, and the following are the chief points to be recorded. There

is still a liability to a sudden feeling of "helplessness" if the patient bends down quickly or turns round quickly, or lies down in bed too quickly. It is described as a feeling of general giddiness which comes and departs again with almost equal rapidity. He is able to get a better volume of sound now when singing; his voice, which was somewhat higher pitched than before the illness, is resuming its natural robustness. After four months he is now able to get down to "bottom E" again.

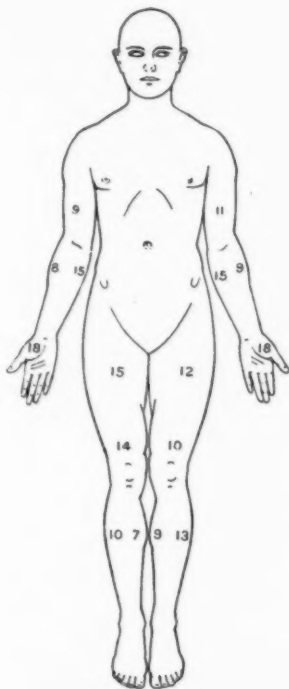


FIG. 13.

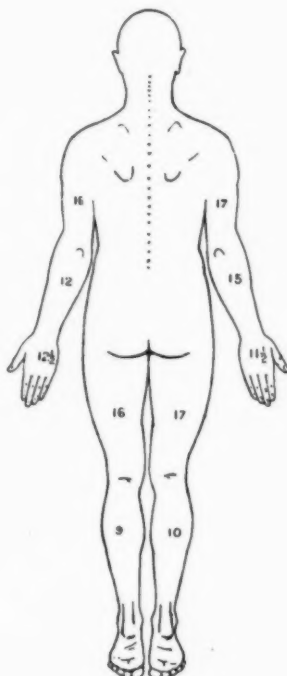


FIG. 14.

Deep pressure pain. (January 12, 1909.) The figures indicate kilograms on Cattell's algometer. (It is to be remembered that the skin of patient's hands is hard and thickened from his occupation.)

There is no difficulty in swallowing. The attacks of slight shortness of breath continue, and the patient says they are associated with a curious feeling as though he must sneeze. He does not actually do so, but the association of the sensation with the affection of respiration recurs

almost daily with any exertion. There is no longer any anomaly of sweating on the face, nor has there been any difference in this respect between the two sides for about six weeks or two months, but there is still a mild degree of narrowing of the left palpebral fissure, and the left pupil is still slightly smaller than its fellow. All diplopia has gone. The optic discs are normal. The nystagmus to the left is less noticeable, but can still be elicited. The motor fifth is entirely normal on the two sides. The palate is almost symmetrical in its movements now, but the vocal cord does not yet move freely. There is no inco-ordination on either side. The patient is still uncertain on his right leg, and there is still the same slight relative weakness of its movements. The deep reflexes are equal on the two sides; there is no ankle-clonus, though

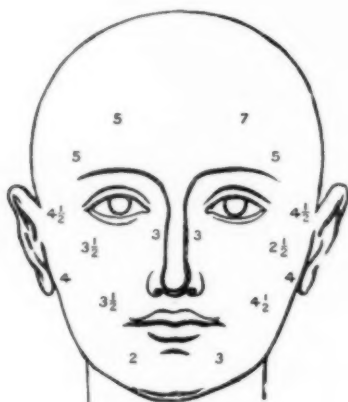


FIG. 15.

Deep pressure pain on face. (January 12, 1909.)

repeated efforts sometimes induce a few clonic jerks at the right ankle; the abdominals are brisk and equal, and there is a double flexor response.

The condition of sensibility is now as follows (January 12 to 17):—

(A) The "subjective" sensations of tingling warmth on the left face and right body are less in evidence. They do not occur spontaneously, but only when the skin is rubbed or pressed. The smarting round the lids of the left eye has not disappeared. Rubbing the left face causes a sensation of warmth which does not radiate; pressing the tongue against the left corner of the lips (upper more than lower) causes

a sensation of pins and needles which does not spread. There is nothing similar to these on the right face, but the area of "tingling warmth" on the right side reached the clavicle about two months ago, where it has remained (fig. 12). The sensation is less obvious now, except over the right thigh, where rubbing or squeezing still causes a bruised or aching feeling, with warmth for a background. There is not, and never has been, any "subjective" sensation of coldness anywhere.

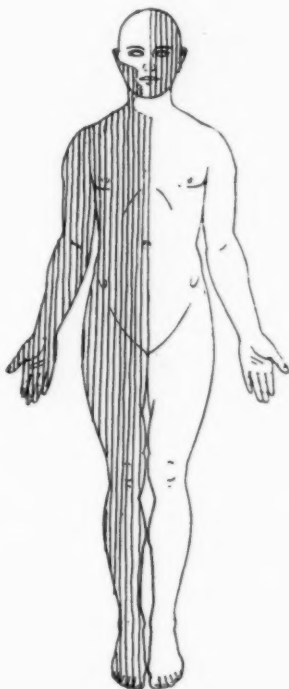


FIG. 16.

Cutaneous pain. (January 14, 1909.)

(B) The objective sensory changes are as follows:—

(a) *Touch*.—This was formerly noted as being less "tickly" on the left face, and that is still noted. There is no loss anywhere, and the localization is always accurate.

(b) *Tactile Discrimination*, by the compass test, is apparently

normal. After many tests no definite and persisting difference between the two sides of the face or body can be made out.

(c) *Deep Sensibility*, tested by Cattell's algometer, is the same on the two sides, as figs. 13 and 14 show. Occasionally one side appeared more sensitive, occasionally the other. Although there is absolute loss to cutaneous pain over the right side of the body, deep pressure pain is not affected, a point to which attention will be drawn later. On the face deep sensibility was carefully tested with the algometer. The results are embodied in fig. 15. Here again there is loss to cutaneous but not to deep pain. No difference can be made out in this respect between the two halves of the tongue. There is no defect of localization.

(d) *Muscular Sense* is everywhere intact.

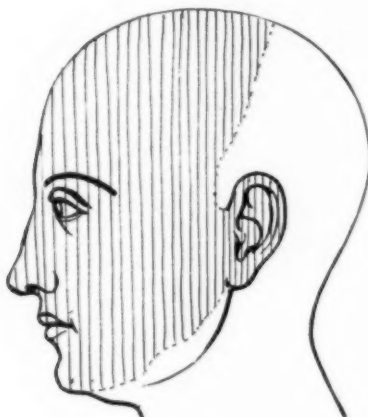


FIG. 17.

Cutaneous pain; a strip of hypalgesia outside trigeminal area.
(November 19, 1908.)

(e) *Cutaneous Pain*.—Improvement has taken place. The upper limit of the loss on the right side is now indefinite over the jaw and neck, receding from the former limit (fig. 16). On the other hand, a narrow section outside the trigeminal area on the left face shows a diminution of sensibility to painful stimuli. At least two months ago there was a suggestion of a change here, but it was very indefinite (fig. 17). The analgesia of the mouth, pharynx, &c., continues. Taste is still entirely absent on the left side.

(f) *Temperature.*—Sensibility to cold has returned over the right face in its middle part chiefly, and also over an area of the left cheek below the eye (fig. 18). Otherwise the condition of insensitiveness to all degrees of cold remains over the right body and left face. There is, further, a strip outside the left trigeminal area where cold is not appreciated normally, but its limits are indefinite. It was noticed at least two months ago. Cold is felt as touch or pressure on the left face, or as a

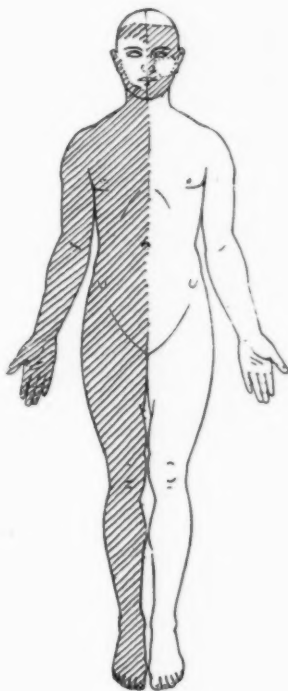


FIG. 18.

Cold. (January 17, 1909.)

pleasant slight warmth. The cold tube does not cause any tingling on the face. On the right body, as before, it occasions a smarting, tingling sensation, with no element of coldness in it whatever. The patient is able to distinguish between cold and lukewarm tubes by the former causing this smarting. As before, all degrees of heat are felt as "tepid,"

or "slightly warm," over the right side, but the area no longer includes the right face, for here in its middle part the patient can feel the hot tube (fig. 19). There is, further, an improvement in the left side of the face, below the eye, where he can distinguish the hot tube. He can distinguish between degrees of heat correctly only in these patches on the face, right and left. As before, the hot tube never gives rise to the additional sensation of tingling unless it is firmly pressed against the skin.

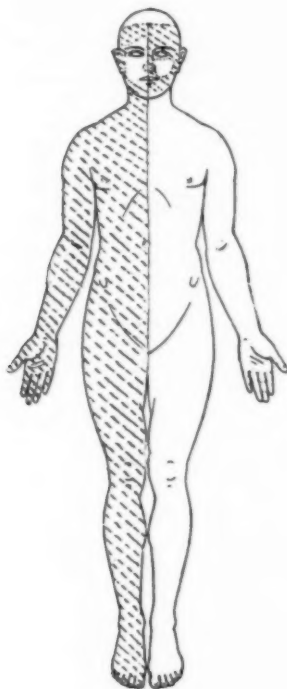


FIG. 19.

Heat. (January 17, 1909.)

When he immerses his hands in cold water the left hand feels the cold, the right feels the water warm, and the sensation of smarting is experienced. "When I put my cold left hand on my right side I get a nice feeling of warmth. When the hands are put into hot water it feels only warm or tepid on the right, but hot, of course, on the left."

One more feature of the case remains to be narrated, and it is one of the most interesting. Mr. M. had often noticed that pricking of the left face, in addition to its being felt as pressure merely, never resulted in any bleeding. He had frequently experimented with himself in this connexion, and says the same thing is true of the right side of the body. About three weeks ago, however, coincident with the diminution of the "subjective" sensations on the left face, he found that pricking was followed by bleeding for the first time since the onset of the disease. I re-examined him from this point of view, and can vouch for the fact that blood is much more readily obtained by pricking on the left hand than on the right, whereas there is no difference now between the two sides of the face. There has never been any appreciable difference between the surface temperatures of the two sides of the body.

A case such as this presents many features deserving of discussion, but as there is no pathological confirmation of the exact extent of the lesion, it is idle to dogmatize on the association of the various symptoms with the various anatomical structures probably involved. It is clear that in its main features, however, the case is to be classed as belonging to the category of those exhibiting a symptom-complex such as has been abundantly proved by pathological evidence to occur in lesions of the posterior inferior cerebellar artery. I shall briefly allude to the probable explanation of the more unequivocal symptoms, as established by the pathological investigations of others, and in closing refer more particularly to the condition of sensibility.

Disturbance of Deiters's nucleus and of the neighbouring vestibular fibres and their connexions is responsible for the acute giddiness, vomiting, forced movements, insecurity of equilibrium and tinnitus of the first stage. There are still indications that the function of these structures is not yet normal—*e.g.*, the momentary "helplessness" produced by too sudden a change of position.

Interruption of the fibres entering the inferior cerebellar peduncle—either fibres of the dorsal spino-cerebellar tract conveying unconscious homolateral impulses underlying co-ordination, or olivo-cerebellar fibres—or possibly disturbance of vestibular connexions, accounts for the hemiataxia.

Disturbance of the glossopharyngeal and vagus centres (nucleus ambiguus, fasciculus solitarius and cells in connexion, &c.) has determined the palsy of palate and larynx on the same side, the analgesia of fauces and pharynx, the difficulty in swallowing, the change in the speaking

and singing voice, the loss of taste over the whole left half of the tongue; probably the irregularity of respiratory rhythm and occasional breathlessness with tachycardia are thus to be accounted for.

That some change of a vasomotor nature has occurred on the left face and right body is certain: the remarkable fact that pricking the skin over these areas did not cause bleeding would seem to indicate overfunction of the vasoconstrictor nerves, or diminution of function on the part of the vasodilators. In the face the area corresponded to that which showed palsy of sweat-fibres. The interest of this phenomenon—in association with absolute loss to cutaneous pain—in an organic lesion is enhanced when we remember the frequency with which it has been observed in hysteria. Further, the return of a certain vasodilation, as indicated by bleeding after pricks on the left face—though the quality of pain is still entirely absent—coincides with a diminution of the “subjective” sensations of tingling when the skin of the left face is rubbed, and suggests that there may be some connexion between the two, especially as these sensations remain undiminished over the right limbs, and here pin-pricks are not followed by bleeding (January 21).

The paralysis of cervical sympathetic fibres in a medullary lesion has been frequently noted in cases similar to the present one. One may fairly say that at least as high as the level of the inferior olives the sweat and sympathetic fibres for the face and eye are uncrossed. The restoration of normal sweating over the left face, with the still persisting eye sympathetic signs, suggests a dissociation of these fibres in the medulla.

The changes in the field of sensibility may be divided into “subjective” and “objective.” Taking the latter first, we may consider the local and the remote effects of the thrombosis.

The local effects are seen in the distribution of the left trigeminal and the left glossopharyngeal nerves. In the distribution of the former we find:—

- (1) Complete loss to painful stimuli of cutaneous origin.
- (2) Complete loss to all degrees of heat and cold.
- (3) Conservation of touch, tactile discrimination, localization, light and deep pressure, and pressure pain.

Thus the loss is not one of protopathic as opposed to epicritic type, for the appreciation of all degrees of temperature is gone; nor, indeed, should we expect to find the persistence of the three sensory systems of the lowest (peripheral) level, since the lesion presumably involves the descending trigeminal root and the cells in association, which belong to

a higher level. The important and significant fact is that we find a dissociation of deep from superficial pain, although the disease is medullary. According to the researches of Head and Thomson, if there is loss to painful stimuli in a lesion above the peripheral level, there is always loss to deep as well as to superficial painful stimuli, but the present case appears at variance with that conclusion. Though we admit the evidence to show that the fibres supplying deep sensibility to the face run in the facial trunk, we have here a medullary lesion in which all forms of pain are not affected together.

The analgesia of the fauces and pharynx on the left, and the complete loss of taste on that side, suggest impairment of function of the glossopharyngeal nerve. As far as I have been able to detect, there seems no loss to touch stimuli in that distribution. The pharyngeal and palatal reflexes are present. The patient felt the pressure of the laryngoscopic mirror on both sides of the back of the mouth. There is thus apparently also a dissociated anæsthesia in the territory of the ninth cranial nerve.

As the motor fifth and the sixth and seventh nerves are normal, we can understand that there is a probability that a part of the descending root and root-cells of the sensory fifth is above the area that has suffered, and therefore that a certain number of afferent fibres from the face have reached the quinto-thalamic path without being touched. We can further state that a lesion presumably in the left half of the medulla has dissociated the impulses underlying the sensations of touch, localization, pressure and painful pressure from those of temperature and of cutaneous pain, over the left face, and those of pain from those of touch and pressure on the fauces, uvula and upper pharynx. Further than this we may not go.

The remote effects of the lesion are seen on the right half of the body. Here we find:—

(1) Complete loss to pain of cutaneous origin over the right trunk and limbs, but not on the face.

(2) Complete loss to all degrees of cold on that side, including the face. With this, loss to stimuli of varying degrees of heat, all of which are felt as tepid merely. This loss similarly includes the right face.

(3) Touch, light and deep pressure, pressure pain, localization, tactile discrimination, muscular sensibility, and sense of shape and size are intact.

We see, again, that in a lesion of the secondary level we may have, and do have, a dissociation of deep from superficial pain; and

whatever be the pathological explanation the fact deserves to be recorded.

There is a difference between the local and the remote effects of the lesion, in so far as all degrees of heat are felt as tepid on the right, whereas on the left face they have no element of "warmness" in them at all. On the remote side the distribution of the changes is not quite the same for all forms, since pain is felt on the right face, but not heat or cold.

This remote dissociation has been frequently observed in similar cases. The suggestion is that in the mesial fillet, the supply of which is the anterior spinal artery, run impulses underlying the appreciation of muscular sensations and certain tactile sensations; while the paths for pain, heat, and cold are in the spino-thalamic section of the lateral reticular formation. The complete preservation of the faculty of localizing any form of stimulus on any part of the body is of great interest in this connexion. It is sufficient at present to record the occurrence of the dissociation in a presumably unilateral medullary lesion.

Of the so-called "subjective" sensations we may enumerate the tingling and warmth on the left face and right side of the trunk and limbs. These do not now occur spontaneously in the strict sense, whatever they did at first, but are easily elicited by the lightest pressure, such as that exercised by a current of air, though perhaps not so readily as formerly. They are positive symptoms, and cannot, therefore, be caused by the lesion; they are permitted by the lesion. Their nature might suggest that the absence of the cold mechanism—for they are experienced only where there is complete loss to cold stimuli—allows other mechanisms to be reached by the cold stimulus. Dr. Hughlings Jackson has recently maintained that, in cases such as this, from the negative lesion there is, directly, inability to appreciate cold substances applied to the cutaneous area, and indirectly from the lesion there is over-hotness referred to the same area. The over-hotness is consequent on removal of control or of inhibition from the physical bases of the sensation heat, by destruction or by some negative lesion of the physical bases of the sensation cold. I have had the opportunity of examining several cases of organic disease of the nervous system, in which there has been referred to some area of the skin a "subjective" sensation of hotness or of coldness, where objectively there has been a loss to stimuli of the opposite kind over the same area, and the present case provides another instance.

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DISCUSSION.

The CHAIRMAN (Dr. Ormerod) said the Section was very much indebted to the author for bringing forward the case, and explaining it in such a lucid way. It was very definite in its nature, and yet such cases were often mistaken. The case of his own, to which Dr. Wilson alluded, was at first thought to be one of syringomyelia, on account of the dissociation of sensation. In another case which he saw recently, and which he believed to be of the same type, the loss of sensation was misunderstood, and it was thought to be "functional." With regard to the last case, it was objected that the loss of sensation transgressed the middle line, and he would be glad to hear if in Dr. Wilson's case, or in any of which Dr. Wilson knew, there had been any indication of loss of sensation crossing over to the non-affected side of the body.

Dr. WILFRED HARRIS said that when he was house physician at the National Hospital in 1896, he had an opportunity of examining a practically identical case to that described by Dr. Wilson, but it showed one or two symptoms to which Dr. Wilson had not referred, but which were described by Spiller and others as characteristic. One was nystagmus to the side of the lesion, and the other, intense and uncontrollable hiccough at the commencement of the symptoms. The case he saw showed both those very well. The history was a rather characteristic one. The man was in a restaurant after dinner, and felt a click, as though something had snapped low down in his back. He could not move the right leg and right arm, and next day he had

severe hiccough, which persisted for four days. When he came to the hospital, some months later, there was definite hemianalgesia of the right side of the face and of the opposite side of the body. In Dr. Wilson's second diagram (fig. 11) the analgesia on the side opposite to the lesion spread above the clavicle to the lower jaw and round the back of the head over the second cervical area. In Dr. Wilson's first diagram (fig. 7) the analgesia ended at the clavicle. He (Dr. Harris) doubted whether the first of those observations was correct; the second diagram seemed to be the true one.

Dr. WILSON, in reply, said there was no hiccough in his case from the beginning, but there was marked nystagmus to the left. There had never been nystagmus in any other direction. In his own case the line of demarcation between the sensory areas on the two sides seemed to be strictly confined to the mid-plane, and in regard to the face the alteration to pain was absolutely bounded by the middle line. He was unable to say whether in any of the recorded cases there was a reference to that point; he had not seen it.

Dr. HEAD read a short paper on a case of fractured spine presenting sensory disturbances of some interest.

Neurological Section.

February 25, 1909.

Dr. J. A. ORMEROD, President of the Section, in the Chair.

The Alcohol Injection Treatment of Neuralgia and Spasm.

By WILFRED HARRIS, M.D.

It is to Schlösser,¹ an ophthalmologist of Munich, that we owe the new treatment of severe long-standing neuralgia and of facial spasm by injections of alcohol into the nerve-trunks themselves, both centrally as well as peripherally. Since then a few others have recorded their experiences with this treatment, notably Ostwalt, and Brissaud and Sicard in Paris, and Kiliani and Patrick in America. I described this treatment for trigeminal neuralgia shortly in my review of neurological literature in the October number of the *Practitioner* of last year, but with the exception of two or three references in the *Lancet* and *British Medical Journal*, during the last two and a half years, to the foreign literature appearing on the subject, I can find no account in any English literature of this valuable treatment. In 1907, at the Innere Medizin Congress at Wiesbaden, the subject of neuralgia was discussed, and Schlösser gave details of 209 cases of various kinds in which he had practised injection by alcohol. Of these, 123 were cases of trigeminal neuralgia, in which various branches of the fifth nerve were involved, in seven cases all three divisions being affected, and in two patients the trifacial pain was bilateral. In practically all of these cases he claims to have cured the patient, for the time being, of the pain, though a recurrence of the neuralgia is to be expected when the nerve has regenerated, and in his 123 cases he obtained an average relief of ten and a half months. When the pain recurs the alcohol injection is repeated, and he finds after an experience of five and a half years of the treatment that the intervals of relief from the pain become longer after repeated injections.

¹ "Versamml. d. deutschen ophthalm. Gesellsch. in Heidelberg," *Klin. Monatsbl. f. Augenheilk.*, Stuttg., 1903, xli, p. 244.

Schlösser found by experiment that the injection of alcohol, in a strength of 70 per cent. to 80 per cent., into a nerve causes degeneration of the nerve by destroying the fibres locally at the point of injection, and he claims that the treatment by alcohol injection amounts to the same thing as a resection of the nerve at that point, though it has several advantages over this cutting operation, chiefly in the injection not being followed by the formation of scar tissue which will irritate the central end of the nerve, and especially in that the injection treatment may be repeated at intervals whenever rendered necessary by the regeneration of the nerve and reappearance of the pain. He admits that excision of the Gasserian ganglion for severe trigeminal neuralgia is theoretically a superior operation, inasmuch as, when properly done, there is no recurrence of the pain, but he points to the severity of the operation, and its mutilating results, with the risk of a certain percentage of deaths even in the most experienced hands. Various drugs, such as osmic acid and chloroform, have previously been used by hypodermic injection for the destruction of peripheral branches of nerves, but the great advance made by Schlösser is the attacking of the three main divisions of the fifth nerve at their points of exit from the skull, and destroying them by deep injections of 80 per cent. alcohol, which he found preferable to other destroying agents. Thus the first or ophthalmic division is attacked by injection at the supra-orbital notch, or foramen, as the case may be, and also by passing the needle along the roof of the orbit to inject around the frontal division of the nerve at the back of the orbit. For neuralgia of the second division of the trigeminal the injection is made in the sphenomaxillary fossa as close as possible to the opening of the foramen rotundum by which the superior maxillary nerve leaves the skull. It is also injected at the infra-orbital foramen where it emerges upon the cheek. The third division, or inferior maxillary nerve, is injected at the foramen ovale, and also at the inferior dental foramen on the inside of the ramus of the lower jaw, and also at the mental foramen in front. Schlösser's method for the deep injections is to use a stout hollow needle, or cannula with stilette, $2\frac{1}{2}$ in. to 3 in. in length and 2 mm. in diameter. This is pushed through the side of the cheek in the necessary direction, without using any anæsthetic of any kind, the patient's sensations being used as the indicator as to when the trunk of the nerve is reached by the point of the needle. When a sharp stab of pain is felt over the distribution of the nerve, the syringe is fitted on to the needle and a few drops of the alcohol are injected, which at once causes a stinging, burning pain over the part of the face supplied by the nerve, which dies away after several

seconds, more and more alcohol being then slowly injected until from one to four cubic centimetres have been used. If the alcohol injection has been successfully placed in and around the nerve, although there is produced a numbing burning pain over the area of the distribution of the nerve, yet there results an immediate relief from the attacks of stabbing pain so characteristic of the disease. On testing the skin and mucous membrane supplied by the division of the nerve which has been injected, it will be found that there is now more or less complete tactile anæsthesia and analgesia; that the patient is now able to talk, laugh, eat, blow the nose, or rub the skin of the face, acts which previously were impossible without starting the painful crises. Schlösser's method has been followed without much alteration by most of those who have adopted this treatment for trigeminal neuralgia; Brissaud and Sicard, of Paris, writing in the *Revue Neurologique* of last November, record their experiences with forty-six cases of trigeminal neuralgia, and describe their technique. Instead of using Schlösser's stout needle, they prefer a fine platinum needle, of only seven-tenths of a millimetre diameter, with a short point. They also anæsthetize the track of the needle with 1 per cent. stovaine solution. They claim that their fine needle minimizes the pain of insertion through the skin and tissues, and also diminishes the risk of producing vascular injury in the zygomatic fossa.

My own personal experiences with this treatment extend only over the past seven months, during which I have used injections of alcohol in thirteen patients, five of which were typical cases of trigeminal neuralgia, or tic douloureux.

In order to acquire the technique necessary for the deep injection of the inferior and superior maxillary nerves at the foramen ovale and foramen rotundum respectively, it is essential to practise first on an articulated skull and then on the body in the post-mortem room. I have studied for this purpose a large number of skulls, both to become familiar with the varying depths and angles at which the foramina are placed, and also with the bony variations that are liable to be met with in this operation. I have, moreover, practised the injection on both sides in fifty bodies in the post-mortem room, using methylene blue for the injection, and, after the brain has been taken out, examining the foramen ovale, Gasserian ganglion, and sphenomaxillary fossa at the base of the skull to trace the course of my injection. Various lines have been given for approaching these foramina, but although I have tried them I prefer the following method, which I have worked out for myself.

For the injection of the inferior maxillary nerve at the foramen

ovale a moderately stout, short-pointed needle, 1.2 mm. in diameter, is pushed through the cheek below the zygoma, as low as possible through the bottom of the sigmoid notch in the lower jaw, the point being directed straight inwards and upwards until it hits the under surface of the sphenoid bone at the base of the skull. The point is then gently lowered by raising the handle, and the edge of the foramen ovale is felt for at a depth of from $1\frac{1}{2}$ in. to $1\frac{3}{4}$ in., according to the size of the individual. My results have shown me that in practically every case the needle can be made to pass through the foramen into the Gasserian ganglion if necessary.

When attempting to inject the inferior maxillary nerve at the foramen ovale, if the needle is passed straight in and too horizontally, the wall of the pharynx will be pierced at a depth of 2 in. to $2\frac{1}{2}$ in. If the point be directed too far backwards the middle meningeal artery may be injured, while if the needle be passed too far through the foramen ovale, to the depth of $2\frac{1}{4}$ in. or more, the cavernous sinus and the internal carotid artery within the skull may be damaged.

For the second division of the fifth nerve, the injection is made below the orbit into the infra-orbital foramen, taking care to use no force when inserting the needle, or the antrum may be pierced. The direction of the needle must be upwards and outwards. Besides injecting the infra-orbital nerve, the superior maxillary nerve is also injected deeply at the point of its emergence from the foramen rotundum in the sphenomaxillary fossa. To reach this point the needle is pushed through the cheek in front of the coronoid process and below the malar bone, a little in front of the line of the posterior border of the orbital process of the malar bone. The needle is pushed upwards and inwards until the front edge of the external pterygoid plate is reached. The point of the needle is then passed in front of this bone so as to enter the pterygomaxillary fissure, through which it is pushed, always at the same angle upwards, until the nerve is reached. This will vary in depth from $1\frac{1}{4}$ in. to $2\frac{1}{8}$ in., according to the size of the skull. Besides a certain degree of risk of vascular injury there are two most serious dangers to beware of after the point of the needle enters the sphenomaxillary fossa—namely, the danger of piercing the optic nerve or of injecting it with alcohol, and secondly, of passing the needle into the sphenoidal fissure and there damaging the oculomotor nerve. The optic foramen lies not more than $\frac{3}{8}$ in. beyond the orifice of the foramen rotundum in the same straight line, and it is therefore imperative never to sink the needle to a greater depth than 2 in., or even $1\frac{3}{4}$ in. in the case of a small skull, as of a

woman of 5 ft. 2 in. in height. When the nerve is reached the syringe, ready filled with 80 per cent. alcohol, is fitted tightly on to the needle, and from 15 m to 20 m of 80 per cent. alcohol injected slowly, a few drops at a time. If no general anæsthetic has been given, as is the practice of the Continental and American workers, the patient will cry out with pain as soon as the point of the needle reaches the nerve, and the further injection of the alcohol causes a sharp burning pain over the area of distribution of the nerve, a pain which is repeated with each push of the syringe. I prefer to have a general anæsthetic given before puncturing the skin, and when my needle is approximately in the correct position for making the injection I allow the patient to come partially round from the anæsthetic (*see* Case XIII). Moving the point of the needle then against the nerve will provoke a reflex spasm of that side of the face, as though pain were felt, and the injection can then be made safely, without the patient remembering anything of the pain afterwards.

In addition to the deep injections there are other foramina which can be injected with the greatest advantage in different cases—namely, the supra-orbital notch or foramen, infra-orbital foramen, mental foramen, inferior dental foramen, and the palatine foramina, both anterior and posterior. The inferior dental foramen on the inside of the ramus of the lower jaw may be injected with advantage in cases where the attacks of pain are limited to the lower jaw, without affecting the territory of the lingual nerve, so as to escape causing numbness and loss of sensation in the tongue also, as would result from injection of the inferior maxillary nerve at the foramen ovale. There are two routes to the inferior dental foramen, either inside the mouth or outside the cheek, round the ramus of the mandible. In either case a stout curved needle is necessary; Schlösser practises the external route, though there is a real danger with this method of producing facial palsy from the alcohol running backwards along the needle.

A successful injection, besides producing a sensation of numbness and burning for some hours over the area of the nerve destroyed, always causes a certain amount of œdema and a sensation of stiffness of the parts. The burning sensation passes off after a few hours, the œdema disappears after two or three days, whilst there is left behind a sensation of numbness; and there is anæsthesia to touch and pin-prick on the skin and mucous membrane, corresponding to the anatomical distribution of the nerve. This will last for weeks or months according to the amount of destruction of the nerve, but will generally begin to diminish after three to four months. The attacks of pain cease immediately after

injection, and the delight of the patients on finding they can talk, eat, use their false teeth, or rub the face, is proof that the nerve has been successfully reached. An accident that will occasionally happen is a hæmatoma from puncture of the artery issuing with the nerve out of the supra-orbital, infra-orbital or mental foramina. Such happened in my Case X, where considerable ecchymosis of the lower eyelid and upper cheek occurred from puncture of the infra-orbital artery when injecting the infra-orbital nerve at the foramen on the cheek.

Case I.—The first case in which I used alcohol injection into a nerve is an excellent illustration of the mode of action upon a nerve-trunk. Last August I was consulted by a lady with extreme hemiplegic contracture of the flexors of the fingers of the left hand, of five years' duration, the result of embolism due to mitral stenosis. She was able to flex the fingers if they were passively extended, but she had no power whatever of extension of the fingers, so that the fingers were constantly strongly flexed into the palm of the hand, which was a source of constant worry and annoyance to her, as she had fair power over the wrist and the rest of the arm. Knowing of Schlösser's injection treatment with alcohol for the destruction of sensory nerves, it occurred to me that the condition of spastic flexion of the fingers would be at once relieved by an injection of alcohol into the median nerve above the elbow. This I did after anæsthetizing the skin by eucaïne and adrenalin infiltration, then making a $1\frac{1}{2}$ in. incision along the course of the nerve in the middle of the arm and dissecting out the nerve. I then injected the nerve with 5 m of 80 per cent. alcohol, considerable pressure being required to force even that amount of the fluid into the nerve; the result was instant relaxation of the spastic condition of the hand, and after the wound was sewn up and dressed the hand was found anæsthetic to light touches and analgesic to pin-prick over the anatomical distribution of the median nerve, though there was perfect sensation on the hand before. Flexion of the fingers was still possible by means of the ulnar nerve and the flexor profundus, and the patient was delighted with the success of the treatment. Eight days later there was still partial anæsthesia and analgesia over the same skin area, though ordinary touches and pressure on the fingers were perceived quite well. The electrical reactions now showed typical reaction of degeneration of the flexors and of the muscles of the thenar eminence; no pain whatever, and no return of spasticity. Four months later there was still no return of spastic contracture.

The injection of alcohol directly into a nerve-trunk thus evidently

requires considerable pressure in the syringe, and I am perfectly convinced that it is quite impossible to inject a nerve-trunk with any large amount of fluid, such as Lange claims in the treatment of sciatica with eucaine and normal saline, in quantities up to 60 c.cm. to 100 c.cm. Any quantity more than a very few minims must be injected around the nerve, not into it. At the same time my case proves that the injection of only 5 m into the median nerve of 80 per cent. alcohol was quite sufficient to paralyse its motor and sensory fibres, and to set up reaction of degeneration in the muscles. It is obvious, therefore, that it is highly dangerous to attempt to inject a mixed motor and sensory nerve, such as the sciatic, with a destructive agent such as strong alcohol, although Schlösser claims that his treatment is equally successful for sciatica as for trigeminal neuralgia, and he states that the motor fibres are more resistant than the sensory to the action of the drug. Finkelnburg, of Bonn, by means of the experimental injection of nerves with alcohol, has completely disproved this statement of Schlösser's, and, in fact, cases of severe paralysis of the leg have been recorded from this method. It is practically certain that any good results obtained in sciatica by attempts to inject the nerve with alcohol are produced by the injection being perineuritic, not into the substance of the nerve. Indeed, as I shall show presently, cure of the pain in trigeminal neuralgia may be brought about by a perineuritic injection of the alcohol in the neighbourhood of and around the nerve-trunk, without producing any paralysis of the nerve, or any recognizable anaesthesia to careful testing on skin or mucous membrane.

In three cases of *clonic facial spasm* on one side, or "tic convulsif," I have produced complete cessation of the spasms in two cases, with transient slight facial paresis for a few weeks, by the injection of 10 m of 80 per cent. alcohol in front of the mastoid, upwards and inwards, in the direction of the exit of the facial nerve from the stylomastoid foramen. In the third case very slight facial paresis only was produced, and the clonic spasms have not entirely disappeared, although they have been so diminished in severity that they do not incommode him now in any way, though for the previous twelve months they were so frequent and severe as to be a considerable hindrance to his work. In this case the incomplete success was due, I am convinced, to my not having penetrated sufficiently deeply with the needle, owing to the patient, a man aged 25, complaining greatly of the pain of insertion of the needle.

My remaining nine cases are all cases of severe pain in one or more divisions of the trigeminal nerve, in two cases the pain being bilateral.

Case V.—One of these, a man aged 40, was suffering, no doubt, from a tumour at the base of the skull, probably involving the sphenoid bone. He had had persistent headache for some months, when he consulted Mr. Cruise for progressive loss of vision. Gradually the sight was lost, first in one eye and then in the other, until both eyes became absolutely blind, with later pallor of the discs developing, though there was never any optic neuritis. Paralysis of the left sixth nerve developed, and then he began to complain of pain on the right cheek, and slight anæsthesia over the second division of the right fifth nerve appeared. This was followed by similar pain appearing in the left cheek, and the pain now increased so much in severity as to be almost unbearable. Mercury and iodide administered during many weeks having failed completely to arrest the disease, or in any way to improve his symptoms, I determined to inject both infra-orbital nerves with 80 per cent. alcohol. I took him into St. Mary's and performed this injection last September, injecting some 15 m of the alcohol into each infra-orbital foramen, without using any anæsthetic. Although a sharp stab of pain was produced by the puncture of the nerve by the needle, and a temporary burning sensation in the cheek and upper lip for some minutes, the man was completely relieved from pain afterwards, and was quite free for the remainder of his stay of another week in hospital. Later severe pain developed again in the left side of his face, chiefly along the lower jaw. This indicated that the growth was now commencing to involve the third division of the left fifth nerve at the foramen ovale, and I therefore again injected with the alcohol, this time under chloroform anæsthesia, passing the needle through the sigmoid notch of the left lower jaw upwards to the base of the skull to reach the foramen ovale. I was able to feel the edge of the foramen with the point of the needle, and could, therefore, be sure of the alcohol being injected at the right spot, although there was in this case no evidence afforded by the patient of pain produced by the puncture of the nerve owing to the chloroform narcosis. I have not seen the patient since, though Mr. Cruise informed me several weeks afterwards that he had heard through his doctor that the man had had no more pain at all in the face since the last injection, though he was becoming steadily weaker, as was of course only to be expected.

Case VI.—A woman, aged 65, with severe neuritis of the left fifth nerve, with pain radiating over all three divisions of the nerve, and anæsthesia and patchy wasting of the skin of the forehead on the left side, exactly resembling post-herpetic scarring, a condition known as

morphœa. In this case the pain, though it varied in intensity, was more or less constant, thus differing from the convulsive attacks of pain of true "tic douloureux." I have made two attempts to inject the foramen ovale in this case, in order to attempt to destroy the ganglion or part of it with the alcohol, but in each case, through faulty technique, I have not succeeded in reaching the foramen, as evidenced by the absence of any anæsthesia or other disturbance of sensation in the lower lip and chin. I intend, however, to make another attempt, and I feel no doubt that I shall be able to produce the desired result, now that I have fully mastered the technique.

Case VII.—A man, aged 30, with severe daily periodic neuralgia of the ophthalmic division of the fifth nerve on the left side, following a catarrhal condition of the nose and sinuses, probably influenzal. The pain came on regularly at 10 a.m. and lasted until 6 p.m., a severe, constant aching, but no sudden intense pains as in tic douloureux. There was slight anæsthesia to touch and pin-prick over the whole of the skin area of the first division of the fifth nerve on forehead, eyebrow, and side of the nose. Injection with 15 mm. of the alcohol with eucaine over the supra-orbital notch at once relieved the pain. To make certain of its non-recurrence I got his doctor to inject some eucaine and morphia into the same spot on the two following mornings, half an hour before the usual onset of the pain, and I hear that he is completely cured. This periodic daily recurrence of severe supra-orbital pain is characteristic of a post-influenzal neuritis, and during the period of the pain I have found in three cases definite diminution of sensation to touch and pin-prick over the ophthalmic division of the fifth nerve, though as soon as the pain disappears later in the day the anæsthesia disappears also. Morphia alone, given about half an hour before the usual time of onset of the pain, will also prevent its onset, and given thus daily for a few days will cure the disorder, though with considerably more discomfort to the patient than with alcohol and eucaine.

Occipital neuralgia is similarly treated by Schlösser with alcohol injections, though I have not used it myself as yet for this nerve.

Case VIII.—A man, of about 50, sent to me from the surgical outpatient department at St. Mary's, with inoperable carcinoma of the tongue, causing very severe pain in the left lower jaw and tongue. I gave him at once an injection of 15 mm. of 80 per cent. alcohol into the left foramen ovale, using no anæsthetic. Though the injection itself caused considerable pain, the man has been completely free from pain ever since, about four weeks, with the exception of a feeling of slight

rawness along the edge of his tongue. As a result of the injection anaesthesia of the lower lip and chin was produced.

Case IX.—Trigeminal neuralgia, or "tic douloureux." Mrs. A., a lady aged 55, who for seventeen years had been subject to attacks of pain in the face of increasing severity, first on the right side, and for the last two years on the left side. The pain seemed limited to the second and third divisions, being especially severe in the lingual nerve, necessitating frequent painting of the tongue with a solution of cocaine. I injected alcohol on two occasions, in September and October last year, into the left infra-orbital foramen and also in an attempt to reach the left foramen ovale. Though the pain disappeared for two days after the first injection the result was a failure, owing, I am convinced, to my failing to reach the foramen ovale, due to my employing Lévy and Baudouin's line, directing the needle backwards, thus engaging my needle amongst the roughnesses of the spine of the sphenoid. Since the last injection I have heard from her doctor that the pain has recurred upon the right side of the face, the pain being now bilateral—a very rare condition in trigeminal neuralgia.

Case X (this patient was shown at the meeting).—Mrs. I., aged 63. *Trigeminal neuralgia*, left second division. Typical history of "tic douloureux." Pain began eleven years ago as apparent toothache in the left upper jaw, for which the left upper canine was extracted, though the tooth was found to be sound. The next tooth was then extracted, with similar result, and no relief of the pain. Since then she had had all her teeth extracted at various times, once eleven at a sitting, without curing the pain. After three years the pain began to start in sudden severe spasms, radiating from the left ala nasi. The pain was thought later to be due to a left antral abscess, and the antrum was operated on two years ago, and again a year ago, when an abscess, she says, was found. The pain was never relieved in the slightest by either of these operations. Dr. William Hill saw her in October last and pronounced the antrum now free from disease, and sent her on to me. The recurring spasms of pain were now very frequent, started by eating and talking, or blowing her nose, the pain being so frequent and severe that she said "she thought she would go mad." On October 15, 1908, under chloroform anaesthesia, I injected the left infra-orbital foramen with about 15 m of 80 per cent. alcohol. This was followed by anaesthesia and analgesia of the whole of the anatomical distribution of the infra-orbital nerve, and considerable ecchymosis and swelling of the lower eyelid. The spasms of pain were now much improved, all the pain

starting at the ala nasi having disappeared, but there was still considerable pain along the gum and palate, and she was able to eat no better than before. A fortnight later I therefore injected the left palatine foramina with alcohol, under chloroform anæsthesia, injecting about 10 m into both the anterior and the posterior palatine foramina. This second injection was followed by anæsthesia of the gum and hard palate, and by complete disappearance of all her pain, so that she was able to eat, rub her face, and blow her nose like a normal person, without starting the intense paroxysms such movements always produced before. For nearly four months she has been perfectly well, but during the last two or three weeks she has noticed a very slight return of pain at the ala nasi and on the gum, but not sufficient to cause her any serious trouble, though in all probability the nerve will have to be injected again within the next few months, as the anæsthesia on the lip is beginning to disappear. Next time I shall inject the trunk of the superior maxillary at the foramen rotundum, as well as the peripheral points of pain, and shall thereby probably obtain a longer period of immunity from recurrence.

Case XI.—Trigeminal neuralgia, affecting the right third division of the fifth nerve: Mrs. T., aged 78, for the last three years has been subject to increasingly frequent and severe spasms of pain in the lower jaw and chin. Of late these have been so bad that she cannot swallow anything except slops, and very little of that. Any touch in the neighbourhood of the right mental foramen starts the spasm, and eating and talking practically invariably do so. I saw her twice with her doctor in the country last November and December, and under chloroform anæsthesia injected the right mental foramen and its neighbourhood with 80 per cent. alcohol, and also the right inferior maxillary nerve at the foramen ovale. After the first injection the pain ceased for four days, but then recurred with equal or greater severity, and I must have missed the nerve, as no trace of anæsthesia was produced. A fortnight later I repeated the injection, this time with greater success, as she has been entirely immune from all pain up to the time of my hearing from her on February 16.

Case XII.—Trigeminal neuralgia, affecting the left ophthalmic division of the fifth nerve: Mrs. H., aged 54. Her trouble began eight years ago as slight pain over the middle of the forehead. She suffers from asthma, and she was using an asthma cure soon after, when she sneezed, and felt a sensation as though something in her head had snapped, with an immediate "sudden dreadful pain all over

the head on both sides; thought she was going to die." The same day the pain began to settle down on the left side, and ever since she has been subject to recurring attacks of intense supra-orbital neuralgia, always beginning as a sudden sharp stab of pain at the outer edge of the left eyebrow, where the skin, and that of the upper eyelid immediately beneath, is excessively sensitive, so that the slightest touch there starts the pain. There was also another sensitive spot on the chin, in front of the left mental foramen, rubbing which would also start the supra-orbital pain. On January 23 last I injected 15 m of 80 per cent. alcohol around the left supra-orbital notch, and also into the inferior dental nerve at its point of emergence from the mental foramen. As a result of this the pain almost entirely ceased; the skin was anæsthetic over the inner part of the eyebrow and on the chin up to the mid-line and the level of the mucous membrane of the lower lip. She still, however, had slight attacks of pain every morning, starting from the outer part of the eyebrow, where touching the skin would still start the pain. I therefore injected her again on February 6, this time injecting the skin under the outer part of the eyebrow as well as the region of the supra-orbital notch. This time the injection was perfectly successful, and no more pain was experienced while she was under observation for a week, and she was able to rub her face anywhere without starting the pain. No anæsthetic was used for either of these injections; there was distinct paresis of the left frontalis and corrugator, and considerable temporary swelling of the upper eyelid after the injection.

Case XIII (this case was shown at the meeting).—*Trigeminal neuralgia*, affecting the left second division of the fifth nerve: M. H., aged 40, a cook. Six years ago she was in a very hard place as cook, when she began to suffer from bad headaches at the top and back of the head, and across the eyes, for three months. Then one night in October, 1902, she woke up at 3 a.m. with severe pain in the eyes, and head felt as though pulled back, and "all her nerves going" down the left side. She was in an agony of pain and woke up everybody in the house with her screaming. She returned home next day, the severe pain continuing in paroxysms every half hour or so day and night. Was given morphia continuously for the pain for three weeks. She has never been free from the pain ever since, though it is much worse at times, and has never been able to do any work since, as eating, touching the cheek or upper lip, or blowing the nose almost always started the pain. Spontaneous attacks are preceded by a sensation spreading up the left side of her body, not painful; this is then followed by a sensation as of

a "click" at the left ala nasi, and the intense pain is then upon her, causing her to scream out and clasp her hands to her face, and moan and rock with pain. Her attacks were the most severe I have ever seen in trigeminal neuralgia, especially when she was taking large doses of strychnine as a tonic. More also than any other case that I have met with, owing to the peculiar aura which preceded her attacks, this deserved the name of "epileptiform neuralgia" which Trousseau gave to the disease more than forty years ago. On February 6, under chloroform anæsthesia, I injected the left infra-orbital foramen, and also the trunk of the second division of the fifth nerve at the left foramen rotundum in the sphenomaxillary fossa. On this occasion I allowed her to come round a little from the anæsthetic after I had got the point of the needle close to the nerve. Then pushing it on through the pterygomaxillary fissure up to the nerve where it leaves the foramen rotundum, I was able to judge when the needle reached the nerve by a sudden twitch of the face as though pain had been felt. The result was perfect, complete disappearance of the pain in the face, and she has since been able to rub the face, blow her nose and eat in comfort, which she has not been able to do for the past six years. As the result of the injection there was considerable œdema of the lower eyelid, cheek and upper lip, which passed off after three days. The anæsthesia of the skin and mucous membrane of the area of the second division of the fifth nerve still persists.

It is clearly fair to claim for this method of alcohol injection almost certain and immediate relief for the pain of trigeminal neuralgia, hitherto uncontrollable except by resection of nerves or extirpation of the Gasserian ganglion. Although when the nerve regenerates after it has been destroyed by the alcohol it is likely that a recurrence will set in sooner or later, the same treatment can be applied again with equal success.

Neuralgia of other nerves, as the occipital, and with due precautions of mixed nerves, as the sciatic or brachial plexus, may also perhaps be amenable to this method. The intense pain in the face from disease of the base of the skull, or from inoperable cancer of the tongue or jaw, can also be certainly relieved by this method.

Motor nerves may also be treated by it for conditions of spasm, such as clonic facial spasm, or carefully selected cases of hemiplegic contracture.

Spasmodic torticollis would no doubt be much more difficult to deal with by injection of alcohol, though it should, I think, be tried before neurectomy is performed.

DISCUSSION.

Dr. PURVES STEWART said the technique as described by Dr. Harris was to make a landmark of the sigmoid fossa of the lower jaw. But the lower jaw was a movable feast; moreover, if the patient was edentulous, that fossa was higher up, and if the teeth met in front, then it was lower down. An equally convenient and more accurate landmark was that described by Lévy and Baudouin, in which they took a fixed point from the zygoma, which did not move, measuring from its descending root, in front of the external auditory meatus. He went $2\frac{1}{2}$ cm. in front of that, and there, keeping close to the lower border of the zygoma, an average depth of 4 cm. brought one to the foramen ovale, without any error due to movements of the lower jaw. To reach the foramen rotundum he prolonged a line from the posterior border of the orbital process of the malar downwards to the lower edge of the zygoma, and then went $\frac{1}{2}$ cm. behind that. If the coronoid process was in the way, the patient simply opened his mouth, and the mandible moved out of the way. He had treated over a dozen cases by deep alcohol injections into the foramen ovale and foramen rotundum (having first practised it on the dead body), and the technique was fairly easy. It was not necessary to give the patient a general anæsthetic. Any initial pain felt during the injection was due to piercing the skin, which should be frozen by chloride of ethyl. The remainder of the procedure was painless until the trunk of the nerve was reached. Then a transient pain was felt along the distribution of the affected nerve. That pain was moderately acute, but less severe than the paroxysms to which the patient had been accustomed. That accession of pain told the operator that he was on the right spot. When passing a needle through the face into the base of the skull, it was important that the needle should not be a sharp-pointed, unguarded one. It should be fitted with a blunt stilette projecting beyond the point, by means of which blunt end all the penetration was done, after piercing the frozen skin with the sharp point; then when it was known that the desired spot had been reached, the stilette was withdrawn from the needle, and its charge of alcohol could be pushed home. He could corroborate from his own experience all that Dr. Harris had said as to the results. One man, aged 69, came from the country to have his Gasserian ganglion removed; he had had trigeminal neuralgia of the second and third divisions for seven years. All his teeth had been extracted, and innumerable drugs had been tried without relief, and so he determined to have something done. Mr. Ballance saw him with a view to Gasserectomy, and found him looking very ill; he had a feeble heart, he was cedematous in lungs and abdomen, and his urine was loaded with albumin. Mr. Ballance pointed out the unwisdom of risking an intracranial operation, and advised him against it. The alcohol injection treatment was therefore tried, through the face, into the foramen ovale and foramen rotundum. After the first injection the patient called out for his teeth, which he had not had for seven years, and for his first mutton chop. Four injections were given in

a fortnight, and though that was five months ago, there had been no pain since. He had another male patient, aged 49, who had had trigeminal neuralgia for ten years in the second and third divisions of the fifth. He also was injected several times and was completely cured. He had also had two women, who had had typical tic douloureux for three years, and they were each cured by a single injection. He had done it in more than a dozen cases since, but those he had described were the only ones he had watched for over five months. To quote results of short duration, such as three weeks, as in one of Dr. Harris's cases, was not of much use for statistical purposes. Peripheral injections into the mental or infra-orbital foramen had been known for a number of years, and were only palliative for a few weeks. It was Schlösser who made the advance of tackling the nerves at their exit from the skull. As Dr. Harris had said, the method could also be applied to the motor nerves—*e.g.*, to the facial nerve, for the relief of facial spasm. In mixed nerves, such as the sciatic, the results could not be expected to be so good. He had treated an obstinate case of sciatica by deep alcohol injections, but the patient, though relieved of his pain, developed external popliteal paralysis, yet he said he preferred that to his old pain. The method was a distinct addition to the medical armamentarium, especially in cases where surgery was contra-indicated.

Dr. WILFRED HARRIS, in reply, said he was familiar with Lévy and Baudouin's results, to which Dr. Purves Stewart referred; he had studied and tried that method, but abandoned it in favour of the one he had detailed. It was much more difficult to feel the descending zygoma and then measure the number of centimetres in front of it, which, it must be remembered, would vary for different-sized individuals. It seemed easier to go to the bottom of the sigmoid fossa, and if the bone was hit with the point of the needle, the operator knew where he was. In the majority of skulls the foramen ovale lay straight inwards from that point. For the superior maxillary nerve he preferred to go in front rather than behind the descending line of the orbital process of the malar bone, as it was certainly a disadvantage to hit the coronoid process. He was pleased with the results he obtained with his method both in practice and in the post-mortem room. One could always make certain of sinking the needle in at the middle line of the orbital process. With regard to the knotty question as to whether the patients should be given a general anaesthetic, he had considered that a good deal. The Continental and American workers used no general anaesthetic, their guide being that when they reached the nerve the patient called out. It seemed a clumsy method, and if the patient could be saved pain, that should be done. The puncture caused considerable pain, and there was still more when the nerve was hit off. If the exact relation of the parts were studied in the post-mortem room, sudden pain on the part of the patient was not necessary as a guide in the operation. Moreover, the patient's feeling of sharp pain, which he had if no general anaesthetic was given, militated against the operator's practice, because, unless he were quite callous, he could not do so well with the patient in pain as without it. Certainly in convulsive tic he preferred a general anaesthetic.

On the Exact Origin of the Pyramidal Tracts in Man and other Mammals.¹

By GORDON HOLMES, M.D., and W. PAGE MAY, M.D.

RECOGNIZING how vague and unprecise is the information on the exact origin of the pyramidal tracts that is given in even the best known text-books on the anatomy of the nervous system, it appeared to the authors advisable to undertake an investigation—

(1) To delimit exactly the region of the cerebral cortex from which the pyramidal tracts spring.

(2) To determine from what cells of the cortex they take origin.

(3) To determine the relation of the area of origin of the pyramidal fibres to the so-called "excitable motor cortex."

(4) And its relation to any of the structural fields that have been mapped out by Campbell, Brodmann, and others.

The method which was employed in this investigation depended on the well-known fact that when an axis-cylinder is injured reactionary changes generally supervene in the cells from which it springs. On cutting across one pyramidal tract, therefore, it might be expected that after a suitable interval definite and easily recognizable changes could be found in cells from which its fibres take origin, and the distribution of such affected cells would then evidently determine the exact origin, and region of origin, of the pyramidal tracts. When it had been once demonstrated that reactionary changes do constantly occur in the cells of origin of the pyramidal fibres after section of these this method seemed free from error and capable of giving precise and unequivocal results; it is certainly without the dangers that arise from the adventitious injuries and the spreading circulatory disturbances that are so liable to accompany the only other method we could have employed—viz., the extirpation of limited regions of the cortex in animals, and the determination of those which send fibres into the spinal cord.

The mode of procedure, therefore, was to divide in the animals used one side of the spinal cord in the upper cervical region, and after a certain interval examine systematically the cerebral cortex of both

¹ An abstract; the paper will appear in full in a coming number of *Brain*.

hemispheres by some modification of Nissl's method. To insure accurate results it was evidently necessary to use only cases in which the pyramidal tracts were the only corticofugal fibres injured. This condition would scarcely have been possible if the pyramidal fibres had been divided above the level of the medulla oblongata, as the corticopontine tracts and other fibres of cortical origin would be almost inevitably injured as well. Consequently the present communication can deal only with the pyramidal fibres that enter the spinal cord—that is, only with those subserving the movements of the trunk and limbs—and the corticobulbar fibres and the upper motor neurones concerned in the movements of the eyes must be neglected.

The origin of the pyramidal fibres in man was similarly investigated in cases in which these tracts were interrupted by acute transverse lesions of the spinal cord. As reactionary cell-changes are by no means so constant or so typical after slowly progressive destruction of their axis-cylinders, only cases in which the pyramidal lesions were acute were employed, but it will be shown in a later communication that the cortical changes that occur in system degeneration or local disease of the pyramidal tracts entirely confirm the conclusions detailed in this paper.

The investigation was carried out on dogs, cats, lemurs, monkeys of the macaque family, a chimpanzee, and on man. The animals were kept alive after hemisection of the spinal cord for periods varying between twenty-three and one hundred and fifty-seven days. One of the human cases survived the accident one hundred and eight days, the other two hundred and twenty-nine days.

The characteristic reactionary changes in the cortical cells need but a very brief description; they consist essentially in that process which is generally known as retrograde chromatolysis or *reaction à distance*. In the early stages, say three weeks after hemisection of the spinal cord, the affected cells may be slightly swollen and more rounded than normal; the Nissl bodies they contain are partly disintegrated, especially in the perinuclear zone, and the nuclei are frequently displaced towards one side of the cell. Soon, however, many of the affected cells begin to shrink and atrophy, their processes disappear, and they may be frequently seen surrounded by neurophages. Eventually they disappear completely; thus in a macaque monkey, which was allowed to survive the operation forty-nine days, only about one-half or one-third of the affected cells were still recognizable, and in another monkey killed after one hundred and fifty-seven days practically all the affected cells had disappeared; a more or less similar condition was found in the two human cases.

It may be pointed out here that in contrast to what is the rule in the cells of the lower motor neurones, cortical cells that undergo acute reactionary chromatolysis apparently never recover but invariably degenerate completely. This fact is of great practical importance, as it alone excludes the possibility of the regeneration of pyramidal fibres.

In all the brains which were examined these changes were found only in the large giant-cells which were originally described by Betz and generally bear his name; these are present only in the infragranular layer of pyramidal cells of a small region of the cerebral cortex. In the primates they are found only in front of the central sulcus or the fissure of Rolando; in the lemur and the carnivora they occupy less easily definable areas. In neither the monkeys, the anthropoid ape nor in man could there be found after division of the pyramidal tracts any changes in the cortex behind the fissure of Rolando; and in the carnivora, as well as in the primates, the only cells affected were the giant-cells of Betz.

A monkey of the macaque tribe in which the right side of the spinal cord was completely divided by the electrocautery 157 days before the animal was killed may be first described. After the brain had been photographed, so that the region from which each section was taken could be afterwards identified, the whole of the Rolandic region of each hemisphere, as well as the greater part of each frontal lobe, was examined in almost complete serial sections. The changes found in the left hemisphere, that contralateral to the spinal lesion, were intense and unmistakable. They consisted of the almost complete disappearance of the giant-cells from a portion of the anterior central gyrus and the paracentral lobule. In the latter the affected area extended to the callosomarginal fissure, narrowing downwards, but it lay only in front of the upper end of the fissure of Rolando; on the lateral surface it extended down to the knee of the sulcus arcuatus. On the lateral surface too it lay only in front of the fissure of Rolando, but extended to its bottom. The precentral sulcus was included within it. No changes could be found in any of the cells of this region except in the giant pyramidal cells.

Another macaque monkey was killed forty-nine days after section of the left side of the cord in the first cervical segment, and in this animal the area containing affected cells was practically the same (fig. 1). But as this animal survived the operation a much shorter period the area was distinguished chiefly by chromatolytic giant-cells, but these cells were in addition reduced to almost a third of their normal number.

The brain of a chimpanzee which was killed twenty-eight days after

division of the spinal cord in the third cervical segment was examined in the same way. The area marked out by the characteristic changes in the giant-cells included the anterior portion of the paracentral lobule and the whole of the anterior central gyrus almost to the level of the inferior genu of fissure of Rolando. On the upper part of the lateral surface of the hemisphere it extended considerably in front of the precentral sulcus. Within this area the giant-cells were reduced to almost half of their nor-

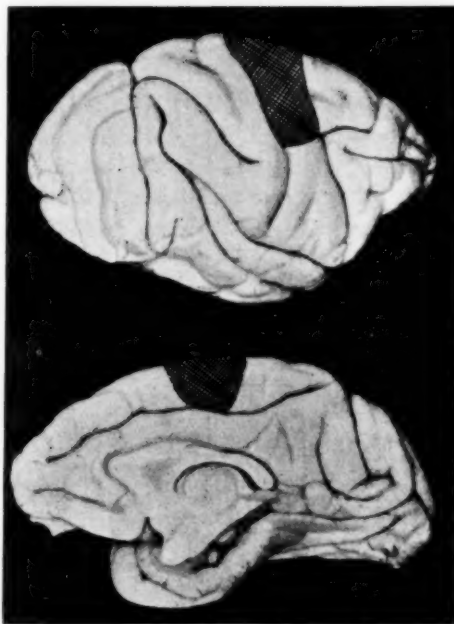


FIG. 1.

Brain of *Macaque monkey*; the shaded areas represent the origin of the fibres of the pyramidal tract that subserve the movements of the limbs and trunk.

mal number, and of those that were recognizable only about a seventh could pass as normal—that is, only about one-fourteenth of the whole were unaffected.

Both human brains in which the origin of the pyramidal tracts was accurately mapped out were from cases where there had been

destruction of these tracts, as a result of trauma, in the seventh cervical segment of the spinal cord. In the one case, in which death occurred two hundred and twenty-nine days after the accident, there was, in addition to complete paralysis of the lower limbs and trunk, total paralysis of the movements of the wrists and fingers, and of extension of the elbows; flexion of the elbows and all the shoulder and neck movements were, on the other hand, unaffected. Fig. 2 represents the region in which definite changes were found. The continuous area in the paracentral lobule and the upper portion of the ascending frontal gyrus undoubtedly represent the origin of the pyramidal fibres that subserve movements of the lower limbs and trunk. It is succeeded by a portion of intact cortex, in which it may be safely assumed that the movements of the shoulders and neck with flexion of the elbows (the movements which were not paralysed) are represented; and below it there is another strip of affected cortex, which can be safely correlated with the destruction of the pyramidal fibres that subserve the movements of the wrist and fingers. The changes which characterize these areas were unmistakable; in the paracentral lobule and the upper fifth of the ascending frontal gyrus there was an almost complete absence of Betz cells, and in the lower affected area very few persisted, and most of these were in chromatolysis.

Cases were also examined in which the pyramidal fibres were destroyed by lesions in the internal capsule; as by such lesions cortical projection fibres other than the pyramidal tract are destroyed, it was always found that, in contrast to the cortical changes which result from destruction of the pyramidal fibres alone, in addition to the Betz cells, many of the large pyramidal cells of both the infra- and supra-granular layers, but especially of the former, were also involved. Further, these cortical changes were not limited to the region from which it has been shown that the pyramidal fibres take origin.

In the lemur the area that gives origin to the pyramidal fibres lies chiefly in front of the small depression on the lateral surface of the hemisphere, which is by Ziehen regarded as the homologue of the sulcus centralis. In the carnivora the same area lies chiefly behind the sulcus cruciatus.

The following conclusions may be drawn from these observations:—

- (1) In the primates it is only from the cortex in front of the fissure of Rolando that corticospinal fibres take origin.
- (2) It is only from the giant-cells that generally go by the name of Betz, which lie in the so-called precentral area, that these fibres spring.

An interesting question arises from this conclusion—viz., Do these Betz cells give origin only to corticospinal fibres? It is probable that this may be answered in the affirmative, as in the leg areas of the human brains, in which cases the pyramidal tracts were the only corticifugal fibres affected, no normal Betz cells persisted. In the monkeys, however, about one-fifteenth of those of the affected area remained intact, but the explanation of this is evidently that it was from these cells that the pyramidal fibres that remained homolateral in the cord at the level of the



FIG. 2.

Human brain. The shaded areas in the paracentral lobule and the upper portion of the ascending frontal gyrus represent the origin of the pyramidal fibres that subserve the movements of the lower limbs and trunk; the lower shaded area the origin of those for the hand and finger movements. The intervening unshaded portion of the ascending frontal gyrus probably gives origin to the pyramidal fibres for the movements of the shoulder and flexion of the elbow.

spinal section spring. This assumption is supported by the fact that on electrical stimulation of the affected region of the cortex in these monkeys chiefly homolateral limb movements were obtained; and, further, by the affection of a small proportion, approximately one-fifteenth, of the Betz cells of the opposite hemisphere.

(3) A question of more general interest is the relation of the areas of origin of the corticospinal fibres to the excitable cortex that contains the primary centres for the movements of the limbs and trunk; for the reasons already stated the cortical motor centres for the bulbar functions and for the movements of the eyes cannot be considered here. But in attempting to discuss this question the difficulty at once appears that "the excitable motor cortex," as it has been employed, is a purely relative term, and the regions to which it has been applied have been mapped out very differently by different observers. It is, however, interesting to compare the areas delimited by the histological methods employed in this investigation with the excitable areas figured by the chief recent observers. If the composite diagram recently published by the Vogts, which represents the results of an extensive series of experiments on macaque monkeys, be selected, it will be seen that the area on the lateral surface of the hemisphere that contains the primary centres for the movements of the limbs and trunk is practically identical with the area from which the corticospinal fibres take origin. Similarly there is a striking similarity between the excitable area delimited by Grünbaum and Sherrington in the chimpanzee and the areas to which the authors ascribe the origin of the pyramidal tracts in this animal. The same close correspondence between the physiological and anatomical areas was found in the lemur, the dog and in the cat.

DISCUSSION.

The PRESIDENT (Dr. Ormerod) thanked the authors, in the name of the Section, for their very important and elaborate paper.

Sir VICTOR HORSLEY said it was an extremely beautiful piece of work, and his purpose in rising was to congratulate the authors upon it. Of course, the method had its limitations, but he would like to know whether there were no other changes in the smaller cells of the cortex. After the enormous amount of work which was involved in the present paper, it seemed very ungrateful to suggest some even more elaborate, but it would be interesting to know whether one would find other changes in the nerve-cells if a numerical estimation of their number were undertaken. So far as the present work went, it brought

together harmony subsisting between anatomical and physiological investigation, and he warmly congratulated the authors upon it.

Dr. FARQUHAR BUZZARD said he understood Dr. Holmes to say that the absence of any signs of recovery in the condition of the Betz cells at any period after section of the pyramidal tracts afforded an explanation of the fact that those tracts did not regenerate. Was this his considered opinion or did he not allow that the failure on the part of the tracts to regenerate might exert some influence in producing complete destruction and disappearance of the Betz cells?

Dr. HINDS HOWELL said that he had examined the spinal cord and the parts of the brain below the cortex of one of Dr. Holmes's cases, that of the man who had survived his injury for a period of more than 200 days. The methods used had been the Marchi and Weigert-Pal stains. He (Dr. Howell), like others who had examined similar cases, had found a certain amount of degeneration in the pyramidal tracts above the level of the lesion, but the degeneration was very slight. He would have expected to find more marked degeneration in the pyramidal tracts if the Betz cells were the cells from which those tracts arose, as Dr. Holmes had shown that there was a very great reduction in the number of the Betz cells in all his cases. The generally accepted view was that if the cell was destroyed its axis-cylinder degenerated, and this view of the nutritional function of the cell was held quite independently of the truth or otherwise of the neuron doctrine. He should like to ask Dr. Holmes how he would explain the survival of the axis-cylinder after the disappearance of the cell from which he had assumed that it arose.

Dr. S. A. K. WILSON asked if Dr. Holmes could say whether he had found any numerical relation between the number of Betz cells which had been affected or disappeared in the cortex, and the number of fibres of the pyramidal tract cut across at the upper cervical level.

Dr. GORDON HOLMES, in reply, said he could answer Sir Victor Horsley's question in the negative. Brains had been examined from five days to seven months after lesions of the pyramidal tracts, yet at no time had they found any suspicion of change in any of the small cells of the cortex. On the other hand, when there was a lesion above the level of the medulla which involved other projection fibres from the cortex, some of the ordinary pyramidal cells of the supra- and infra-granular layers were also affected. It seemed probable that the Betz cells were only large pyramidal elements for connexion with the cord, specialized as the brain developed that connexion in the course of phylogenesis. Dr. Buzzard raised a question of cause and effect, and he did not feel prepared to give a positive reply to it at once. But the fact that within a short time—thirty days in one of the animals—a considerable number of the affected cells had entirely disappeared was much against the view that they disappeared because the fibres did not regenerate. The point raised by Dr. Howell needed considerably more discussion than there was time for at that hour. According to the neuron theory, the pyramidal fibres above the level of the lesion should degenerate, or at least atrophy, but there were a number of

similar instances well known. After section of the spinal cord in the upper cervical region in an animal, for example, there resulted an almost complete disappearance of the cells of Clarke's column, though below the level of the lesion one might not get any degeneration in the direct cerebellar tracts. He (Dr. Holmes) pictured to himself that, as the dying off of the cells was a gradual process, the fibres passed into some state of mummification, becoming functionless, but still maintaining in some degree their morphological identity, and consequently were still stainable by chemical methods. He was not prepared with an answer to Dr. Wilson's question, because even the histological work had occupied more than eighteen months, and if in addition they had attempted to count the degenerated fibres, the result could not have been presented yet.

Neurological Section.

April 22, 1909.

Dr. J. A. ORMEROD, President of the Section, in the Chair.

Early Paralysis Agitans.

By WILFRED HARRIS, M.D.

W. B., A MAN aged 27, for the past five months has noticed progressive weakness of his left leg and arm. No history of fright or mental strain. Gait: Hemiplegic, dragging left leg; the left arm hangs stiffly by the side, with the fingers flexed, and rhythmical tremor of hand and forearm. Right arm swings freely. Typical rigid facies, and slight weakness of left angle of mouth. All reflexes normal.

Dr. HARRIS said that in his opinion it was certainly a case of paralysis agitans, occurring in quite a young patient. He had tried to exclude organic lesion such as tumour, and he felt convinced that such organic disease was absent. The case began with pain and weakness, and he thought the facies and gait were characteristic of the disease.

Myopathy with Gigantic Œdema and Albuminuria.

By WILFRED HARRIS, M.D.

G. F., AGED 24, has noticed slowly progressive swelling of the legs below the knees since he was aged 9. No pain or any feeling of illness. He has never been able to flex the three inner fingers of either hand at the interphalangeal joints.

Present condition: Enormous œdema of both legs, especially of the left, resembling elephantiasis. The swelling pits on pressure, and diminishes considerably if he is kept in bed. Considerable hyperidrosis

of feet and legs. The œdema for the past two years has been invading the forearms and backs of hands. Family history *nil*. Pulse soft; no cardio-vascular changes. Wassermann syphilis reaction positive. Urine: Specific gravity 1020, albumin one-twentieth; no casts; chlorides normal. Daily excretion of urine normal. Muscles: Some atrophy of thenar eminence and interossei on each side. Complete absence of supinator longus on each side, and of flexor carpi radialis and flexors of the three inner fingers. Partial fixation of interphalangeal joints of these fingers.

DISCUSSION.

Dr. T. D. SAVILL said there was an explanation which had no doubt occurred to Dr. Harris concerning the case. This was a case of congenital myopathy, and it was known that in many of these myopathies there was a distinct want of vasomotor co-ordination, and consequently a marked tendency to congestion of the extremities, with blueness and severe swelling. This patient had had albuminuria, and Dr. Savill suggested that the affection of the hands, instead of stopping at an ordinary vasomotor condition, had come on to œdema. He thought there was, in the first place, a localized œdema, a congestion which had become an œdema both in hands and feet. By reason of the more prolonged duration of the congestion in the lower extremities—and partly for mechanical reasons—the congestion and œdema had become a hyperplasia, finally resulting in the elephantoid condition of the legs.

Dr. PARKES WEBER said he believed the case fitted in with the cases that had been described in both France and England under the name "trophœdema" of the extremities. The condition might affect only one limb, sometimes both lower extremities were swollen; but he could not remember having heard of a case in which all four extremities were symmetrically affected. The pathology of the cases which had been described under that name was still uncertain; there had been very few post-mortem examinations on them. One or two cases, he thought, of supposed trophœdema had been associated with slight albuminuria. The suggestion that in Dr. Harris's case the albuminuria was caused by a disturbance in the kidneys similar to that which in the subcutaneous tissues led to the chronic swelling of the extremities would amply account for the absence of decided signs of nephritis. He suggested that Dr. Harris's case was an exaggerated example of the cases which had been described as trophœdema. He used that name simply as a name, not implying that the œdema in those cases was, strictly speaking, "trophic." Trophœdema might be congenital or might develop during childhood or later in life. It might occur in only a single member or in several members of a family.¹

¹ Cf. F. P. Weber, remarks at the Meeting of the Clinical Section, December 11, 1908, *Proc. Roy. Soc. Med.*, ii. (Clin. Sect.), p. 61.

Dr. F. E. BATTEN rather doubted whether the case exhibited any sign of actual myopathy. The man certainly had a congenital weakness in the fingers, but—except for some loss of substance in the supinator longus—he could not detect weakness or wasting of any other muscles. Certainly the case corresponded to none of the well-recognised types of myopathy; that, however, was no argument that it might not be a myopathy. The condition was not progressive. The man said his strength was good; he could ride a bicycle and could swim, so the case had, at any rate, not taken the ordinary course of a case of myopathy. He did not consider that there was defined weakness of the orbiculares.

Dr. FARQUHAR BUZZARD said he entirely disagreed with what Dr. Batten had said, and thought there was very definite weakness, and probably some wasting, of more muscles than the supinator longus. There was a definite weakness of the triceps and of the extensors of the wrist on the left side as compared to the right. He had found on examination that there was slight but definite weakness of the orbiculares palpebrarum. The weakness of the muscles referred to could not be explained by the œdema, which did not affect some of them at all.

Dr. WILFRED HARRIS, in reply, thanked members for their expressions of opinion. He gathered that Dr. Savill would explain the œdema from two points of view: firstly, from the paralytic condition of the muscle which one was accustomed to see in myopathy with blueness. But he (Dr. Harris) did not think that would apply in that case, because the man had no muscular atrophy in the legs. So it was not a myopathic œdema in the leg, whatever it might be in the forearm. The only point suggested in connection with renal disease was the albuminuria; but there were no casts and the salt-content was good. He felt sure the man had no form of Bright's disease. The sweating was excessive, rather than diminished. He regarded it as leakage-albuminuria, just as he looked upon the œdema as a leakage condition, some congenital weakness of the capillary wall, allowing albumin to leak into the urine and the serum to leak into the tissues. Therefore one might possibly bring it into line with the muscular condition, looking upon that as a congenital defect in development of the muscular tissue. Trophœdema, which Dr. Parkes Weber mentioned, was described as either congenital or acquired, and usually familial. In the present case it was neither congenital nor familial; those cases, too, which had been described were mostly not symmetrical. He also suggested that albuminuria was against trophœdema, as it was generally absent in the latter. He could not prove his diagnosis of myopathy, as the condition did not appear to be progressive, but there was a fairly wide distribution of atrophy in the upper part of the body, affecting the supinator longus, the flexors of the fingers, the flexor carpi radialis and the thenar eminence. He also agreed with Dr. Buzzard that the orbicularis palpebrarum was weak. He concluded it must be myopathy, but of a type with which he was unfamiliar.

A Case of Optic Atrophy.

By G. DE B. TURTLE, M.D.

MALE, aged 37, single, printer. Complains of failing sight during the last four years and shooting pains, especially in his left leg, during the last few months. Has also had tinnitus in his right ear like a gong or fog horn since rheumatic fever last March.

Previous history: Rheumatic fever three times, at the ages of 21, 28, and 34. Syphilis at 20, for which he was treated for a period of four months. Heavy beer-drinker at times.

Present condition: Marked primary optic atrophy, left more than right; no Argyll-Robertson pupil. Vision: Right $\frac{5}{60}$, left $\frac{4}{60}$. Fields contracted. Slight Rombergism. Slight ataxy when asked to toe and heel a line. Right knee-jerk obtained only on reinforcement, left present and active. Both ankle-jerks present, and plantar responses flexor in type.

Dr. TURTLE added that during the last three years there had been little alteration in the case. In the eyes there was reaction to light and to accommodation, although the pupils dilated very slowly under shadow. The movements of the ocular muscles were all good; no ptosis, diplopia, or nystagmus. The ataxy was now less marked than three years ago, and the same was true of Romberg's sign. Just before Christmas his general health was bad, and his fields were smaller than previously. Dr. Guthrie took him into the Maida Vale Hospital, and since then he had improved in every way: his fields were much better and his general health good. He did not doubt the condition was tabes. The knee-jerks had never altered; the left was quite active, the right only present on reinforcement. There was a definite specific history. As he had not had lumbar puncture done, he could not speak as to lymphocytosis.

Compression Paraplegia.

By T. GRAINGER STEWART, M.B.

R. C., AGED 57, a farrier, married; 4 living children.

History: Past—venereal and risk denied. Present, December, 1907—pain in lower lumbar region, shooting down thighs (right more than left), absent when in bed, made worse by exercise. Some general

weakness of both legs; got much better in three weeks. Then weakness in both legs combined with stiffness progressed. April, 1908—weakness increased, precipitancy of defæcation. June, 1908—weakness increased, delay in starting micturition, had to give up work. September, 1908—shooting pains round the groins. Since then—no change beyond increasing weakness in legs.

Present condition: Cranial nerves—upper extremities normal. Motor system—difficulty in raising himself to sitting position, but recti act equally and well right and left. Lower extremities—spastic, equally right and left; all voluntary movements present, weak; left leg weaker than right. Sensory—relative anæsthesia and analgesia in patchy distribution in both legs; small patch of hyperæsthesia in right groin. Reflexes—abdominals present right and left; knee-jerks brisk, right and left; ankle-jerks not obtained, right or left; no clonus. Plantars—extensor, right and left.

Course: Weakness. Sensation now impaired up to level of about eighth dorsal segment. Almost complete analgesia and complete anæsthesia up to this level. No pain beyond aching across back in lower lumbar region; no tenderness of spine; no impairment of mobility of spine. Motor: Weakness of the abdominal muscles in their lower two-thirds with rising of the umbilicus. Reflexes: Deep, all brisk; plantars, extensor; abdominal, absent; epigastric, present.

A Case of Persistent Visual Aphasia.

By GORDON HOLMES, M.D.

THE patient is a man aged 44. Almost five years ago a large superficial tumour was removed from the left angular gyrus. Since that time he has been unable to write or to learn to write, and he is unable to read; only recently he has learned to fish out a few words from a page of print. He is a fairly well educated man; previous to the growth of the tumour he was a vanman, and had no difficulty in keeping his books. He was evidently able to read and write easily. Speech is otherwise very little impaired. He expresses himself quite easily, and understands all that is said to him. His condition has been quite stationary for the past three or four years.

A Case of Abortive Muscular Dystrophy.

By GORDON HOLMES, M.D.

A MAN, aged 60, sought advice for a slight attack of hemiplegia. On examination there was found extreme weakness of the orbicularis palpebrarum of each side, slight weakness of the orbicularis oris, and the cheeks were remarkably flat and thin. Since he was about 10 years of age the patient had not been able to whistle properly. The muscles which depress the arms were also weak, and there was considerable wasting of the lower part of each pectoralis major and atrophy of the upper portions of the trapezii. The patient was unaware of these muscular abnormalities. The question arises as to how the conditions found should be interpreted. Is it a myopathy which had ceased to develop in its earlier stages, or is it a congenital defect of muscles? We could not at first get from him any history of any such disease in his mother's or father's family, or among his brothers and sisters, but later the patient casually mentioned that he was the father of a case which had been shown before the Society by Dr. Taylor, and that two of his brother's sons were similarly affected. In both instances the disease had been transmitted through the male line. His daughter and nephews had typical myopathy, which made the diagnosis of myopathy in his case very probable. Though he could not now whistle, he believed that he could as a lad, though not since he was aged 10. He was not conscious of the other muscular defects. The case is probably one of myopathy which appeared early but ceased to develop at a certain stage of its evolution. Very few similar instances have been recorded, but Erb has referred to a case of pseudohypertrophic muscular palsy which undoubtedly recovered.

DISCUSSION.

Dr. F. E. BATTEN said that the type which Dr. Holmes had described had many features closely allied to the Landouzy-Déjerine type. Surely it was recognized that so far as weakness of the face was concerned, this might remain quite stationary for many years. In many cases it was admitted that there was evidence that the patients had had weakness of the face at birth, and when seen between the ages of 30 and 40 years they had still the weakness of the face muscles, but no evidence of any progress of the disease. Dr. Holmes referred to the transference through the male. In the family he (Dr. Batten) had under observation the transference was through the female;

and he thought it was generally recognized that this type was transferred through and affected males and females alike.

Dr. STANLEY BARNES said he had seen two cases of the Landouzy-Déjerine type who were aged over 50, and in each of them there was no reasonable doubt that the pectoralis major had been absent for twenty-five years and that the myopathy had not progressed for many years. He had always thought that the Landouzy-Déjerine type of myopathy was the most benign of the types known; there was a much better chance that the condition might be arrested and not progress to a generalized myopathy. One would assume that the case shown was myopathy, seeing that it had the distribution characteristic of the Landouzy-Déjerine type. He asked what were the minimal signs on which one ought to make a diagnosis of myopathy? In the last three months he had seen three cases in which the whole or the sternal part of the pectoralis major had been absent on one or both sides, without signs of wasting of any other muscles. Must those cases be regarded as potential myopathies, or merely congenital absence of muscle? He inclined to the latter view, and thought that cases might crop up of congenital absence of other muscles. The case shown by Dr. Harris might be one of congenital loss of a certain number of muscles in another situation. He did not think one should consider that every case which had lost the pectoralis major, or which had never had it, should therefore be regarded as even a potential myopathy.

Dr. GORDON HOLMES, in reply, said he showed the case as one which he hoped would be of interest to the Section; not because he regarded it as exceptional. He raised the question whether in this case one was dealing with a congenital muscular defect on the one hand or with a progressive muscular disease on the other. Dr. Batten had referred to a type in which from birth there was a congenital weakness of the face which did not progress. But he did not think such cases could be compared with the present case, because in this there was a history that the man could whistle when a lad, but gradually lost the ability to do so when about aged 10. The cases referred to by Dr. Batten of congenital weakness of the face were distinct from his own, which must be regarded as an abortive myopathy. The disease may be transferred through either the female or the male, especially the Landouzy-Déjerine type; but he had merely pointed out that in two branches of the same family transference took place through the male. In answer to Dr. Stanley Barnes as to where the line should be drawn for myopathy, he must say he did not think the congenital absence of one or both pectorals could justify this diagnosis. As a matter of fact, congenital bilateral absence of the pectoral muscles was very rare: only one or two cases had been recorded. Such cases constitute a condition quite distinct from myopathy; as in these there is definite agenesis or non-development of muscle, or at least congenital absence, while in myopathy muscles which are apparently normal at birth degenerate in later life.

Congenital Cerebellar Ataxy.

By LEONARD GUTHRIE, M.D.

P. E., AGED 3 years and 3 months, was delivered by forceps; much cyanosed at birth, and only resuscitated after two hours' artificial respiration. No definite injury to head was observed. He was a weak and delicate infant; suffered from abscesses on face and under chin at 2 months, and from bronchitis at 8 months. Has never seemed to have normal use of limbs.

Admitted to hospital June 6, 1908, on account of muscular weakness and inability to stand or walk. Intelligence not high, but not markedly deficient; memory poor. Speech drawling, sing-song; articulation very indistinct. Cranial nerves: Vision—optic discs, pupil reactions normal; nystagmus present on external deviation, especially to right; no strabismus nor paralysis of ocular muscles; fifth, seventh, eighth, ninth, tenth, eleventh and twelfth nerves normal; sensation normal. Motor functions: No definite paralysis of any muscles, but generalized weakness present; head tends to loll in various directions owing to weakness of neck muscles. Upper extremities: Marked coarse ataxy and tremor on volitional movements. Lower extremities: No muscular atrophy, but all muscles are weak; he is quite unable to walk or to maintain equilibrium in the erect position; sways in all directions, and would fall if not supported. Reflexes: Superficial are normal; deep are all somewhat exaggerated; plantar response is extensor; sphincter actions are normal, and also electrical reactions.

Progress in nine months: General health and muscular strength have greatly improved. Nystagmus is less marked than on admission. Ataxy is unaltered. He is quite unable to preserve equilibrium in erect position, or to walk, but crawls and hops on hands and knees with much agility. Speech is still very indistinct, and memory bad; learns letters of alphabet, but forgets them next day.

Dr. FARQUHAR BUZZARD said he had seen that week a case which was interesting because he thought it seemed to show what might be anticipated to be the result in the case before the Section in a few years' time. The child he saw was aged 6, was a twin, the other child never having matured, and had been born at 8 months. There was nothing special about the birth. He had never been fed at the breast and had had no special illness. He did not walk until 3½ years of age, and did not talk until he was 4. Even now his articulation was so bad that it was not easy to understand what he said. There was

inco-ordination in all four limbs. Although he was now able to walk he could not raise himself on to his feet from the ground. This difficulty was due to the fact that he could not balance himself sufficiently while he was straightening his trunk out. If he tried to go beyond a certain point he would fall over. He walked on a broad base, and threw out his legs to a certain extent. He had no defect in any of the organs of articulation, and he believed that articulatory inco-ordination explained his indistinct utterance and corresponded to the inco-ordination of his limbs. He thought that some day Dr. Guthrie's patient would learn to walk, and would probably learn to speak better than now.

Case of Combined Tabes and Paralysis Agitans.

By T. GRAINGER STEWART, M.B.

A. P., AGED 53, married; no children. Family history good; no similar affection in any member of the family. Previous health good; denies venereal disease; wife has had no miscarriages. Present illness commenced gradually ten years ago after a period of strain and worry. Patient noticed that his right hand shook, and since then up to the present time the tremor has slowly but steadily increased and involved both arms, the head, and to a slight extent the legs. Three years ago his sight began to fail, and he suffered at times from sharp, cramp-like pains in the extremities. For the past year he has been quite blind and unable to see light.

Present state: There is primary optic atrophy of both discs with loss of the pupillary reaction to light. The other cranial nerves are normal and the face is quite mobile. Motor system: When sitting at rest his position is suggestive of paralysis agitans. The head is bent forward, and the arms, which are flexed at the elbow, rest upon his thighs. The hands and fingers are kept in the paralysis agitans position, and a coarse tremor is observed on both sides, the movements taking place being of a pill-rolling character, with a rotatory flexion and extension at the wrist joint. The tremor sometimes ceases during rest and is increased by emotion or active movement. There is little volitional control over the tremor. When standing the attitude is not characteristic, and the gait is that of a blind person. There is no rigidity, the power is good, and there is no ataxia. Sensory system: Has had sharp, cramp-like pains in the extremities. Objective examination reveals no definite loss of cutaneous sensibility, but a slight blunting to deep pain and some loss of sense of position in the toes and fingers. Reflexes: Deep, all absent; superficial, normal; sphincters, unaffected.

DISCUSSION.

Dr. F. E. BATTEN said the case to which Dr. Grainger Stewart had referred was that of a man who was under his, Dr. Batten's, care at the National Hospital; he had more of the typical appearance of paralysis agitans than had Dr. Grainger Stewart's case. It was only on physical examination that he was found to have definite symptoms pointing to tabes, viz., Argyll-Robertson pupils, diminution of the knee-jerks, and absence of muscle-pain sense. The lack of facial expression was very striking, as also was the position of the body, and the fine tremor of the hands. The patient had not, however, the typical gait of paralysis agitans. That was the only case he had seen in which paralysis agitans was superimposed upon tabes.

Dr. WILFRED HARRIS said that cases of conjunction of paralysis agitans and tabes had been recorded, just as had those of a conjunction of tabes with Graves' disease, or with hemiplegia, &c. He took it to be a purely accidental combination. In Dr. Grainger Stewart's case he could only see one sign which was a very definite one in paralysis agitans—namely, the characteristic tremor. Was one to diagnose paralysis agitans on that alone? He thought not, because a tremor exactly like the pill-rolling of paralysis agitans had been known to exist apart from that disease. Many members would remember a case which was shown by the late Dr. Bryant at a former meeting of the Neurological Society, that of a young man with what he thought to be paralysis agitans following typhoid fever, with the characteristic pill-rolling tremor. But he had none of the facies of the disease; he swung his arms freely, and his face was mobile. Most of those present disagreed with Dr. Bryant's diagnosis. The patient got well eighteen months later, and it must have been a post-febrile tremor. The face of to-day's case was mobile, he swung his arms freely; and he, Dr. Harris, thought rigidity was a much more important sign than was tremor. If it was not paralysis agitans, what could the tremor be due to? Was there any familial history of tremor?

Dr. FARQUHAR BUZZARD said that perhaps the only test of the question under discussion was that of time. If there was paralysis agitans, it would progress. He had an out-patient who had been attending two or three years, a patient very much like Dr. Stewart's case, with more signs of tabes—for instance, lightning pains. He thought the tremor was like that of paralysis agitans when he first saw him, and he marked the case "? paralysis agitans" as well as tabes. The signs of paralysis agitans, however, had not increased during the two years that he had been under observation, and it was probably incorrect to apply that title to his condition.

Dr. GRAINGER STEWART, in reply, said he agreed that tremor was probably the least reliable of the symptoms of paralysis agitans. He pointed out that there were two distinct types of paralysis agitans: one which began with tremor, without rigidity, but which afterwards conformed to the typical condition; and the other which began with the rigidity, or with a combination of rigidity and tremor. He could not obtain any history of tremor in other

members of the patient's family. In the present case the tremor had gradually developed during a course of ten years. Therefore one must put out of court any sudden lesion, either vascular or post-febrile; and there were no physical signs of lesion in the mid-brain, such as ocular paralysis, anæsthesia, or affection of the reflexes on either side. He thought the case was interesting and should be watched to see whether it would eventually develop along the lines of paralysis agitans.

Case of Facial Paralysis resulting from Herpes of the Upper Cervical and Genuiculate Ganglia.

By T. GRAINGER STEWART, M.B.

M. A., FEMALE, aged 37, single. Patient was quite well till six months ago, when she was suddenly seized with a pain "like a stiff neck" on the right side of her neck. Two days later a rash (herpes) appeared on the right side of the neck and shoulder and back of head, extending on to the face below the right ear (areas of C 1, 2, 3, 4), and also had a spot inside the right ear. Four days later the right side of the face became paralysed, with loss of taste on the right side of the tongue. She also noticed that sounds were painful in the right ear. When first examined, two months after the onset, the scars of the herpetic rash were present, the right face was severely paralysed, and there was still loss of taste on the anterior two-thirds of the right side of the tongue. Hearing was normal.

The "Diaphragm Test" for Binocular Vision.

By N. BISHOP HARMAN, F.R.C.S.

THIS test is the reverse of Javal's well-known "bar-reading" test. Instead of a bar, which the patient's eyes must negotiate, there is a screen with a single hole in it; through this hole the patient can look with both eyes, quite naturally, and without suspecting the manner in which his vision is being dissected.

The test rests on a phenomenon that occurs to everyone many times daily; when a window is looked through the man with binocular vision sees more widely than he who has but one eye. To demonstrate the principle of the test the following experiment may be made: Stand

facing a wide view, hold up both hands, palms towards you, 6 in. from your face and on a level with the eyes; let the hands be separated so that the little fingers are distant from each other three fingers' breadth. It will be found that the hands cover the eyes, yet a perfectly clear perception of the view is obtained. But if one eye be closed half the view is lost, for the right hand obscures the direct vision of the right eye and the left hand that of the left eye; but the eyes see crosswise through the space between the hands. The paths of the vision are shown in

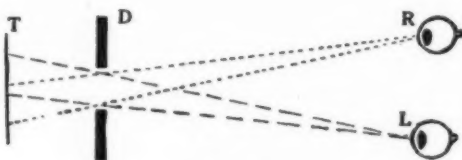


FIG. 1.



FIG. 2.

fig. 1, and the manner in which right and left monocular and also binocular vision is arranged for.

The instrument made to apply this test in practice is very simple, but a series of most varied and surprising tests may be made with it. They are so simple that the operator is not likely to be tied up in examining a patient, yet they are so subtle that when an expert is submitted to the test he can only escape confusion by stating simply what he sees.

THE INSTRUMENT.

A length of wood like a flat ruler 44 cm. long is fitted with a rack at one end to receive the test cards, and a screen measuring 9 cm. by 6 cm. fixed at 11 cm. from the rack. In this screen, or diaphragm, a hole is cut; it is either square or round, and measures 1.7 cm. square or in diameter. A movable pin is fixed to the diaphragm, so that it can be projected into the hole as a point of fixation in certain experiments. A handle is fixed beneath the base board¹ (fig. 2).

Method of Handling.—In use the patient takes hold of the handle with both hands and places the free end of the rule (this is washable) against the upper lip just beneath the nose. The surgeon stands facing his patient and holds the other end of the instrument to keep it steady. When the instrument is in position the patient is asked to look either through the hole or at the pointer projecting into it, according to the test desired, and to tell what he sees through the hole.

Test Cards.—There are three sorts: (1) Printed matter, of any size from diamond in set paragraphs to canon in single capitals; (2) black or coloured squares variously disposed; (3) pictures for children. A number of test cards are issued with the instrument, but the surgeon can make and vary them indefinitely.

The Diaphragm.—The screen with the square hole is the most generally useful; for the reading tests particularly, and when we wish to demonstrate the presence of binocular vision where it is denied. When the patient looks clear through the hole at the test the margins of the hole are seen doubled; the square becomes an oblong. This change escapes remark save by the most observant. On the other hand, when it is desired to demonstrate weakness of binocular vision or in fusion experiments the round hole is the better, as reduplication of the circle and overlapping of the two images is very noticeable.

THE USAGES OF THE DIAPHRAGM TEST.

The test is of value for the following purposes:—

- (1) To determine the equality of visual acuity in the two eyes.
- (2) To determine the presence, the absence, or a defect of binocular vision.
- (3) To exercise the vision in squinting eyes.

¹ The instrument is made in suitable materials, in excellent fashion, by Messrs. George Culver, Ltd., of White Lion Street, London. It costs only a few shillings.

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- (4) To detect malingerers feigning monocular blindness.
- (5) To demonstrate certain physiological phenomena connected with the perception and suppression of images.

(1) *Equality of Visual Acuity of the Two Eyes.*

A paragraph of printed matter is put in the test rack; the patient is desired to read it. The plan of the instrument is such that three kinds of vision are required to pass the test. The right half of the test is read by the left eye, the left half by the right eye, and the middle strip by both eyes (fig. 3). If the patient can detect no difference in the clearness of the letters on the card the eyes have equal vision. The test is very delicate. Further, this cannot be passed unless there be good balance of the oculo-motor muscles.

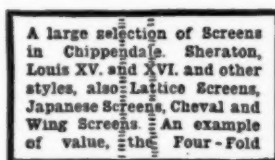


FIG. 3.

The dotted lines show the overlapping of the crossed images of the hole in the diaphragm.

(2) *Binocular Vision and its Defects.*

The preceding test, when small type is used, is the most delicate test of this nature. A small degree of latent squint will prevent a man from reading the paragraph accurately. The phenomena produced by the various orders of latent squint can be best shown by the use of the test card with a single line of letters or figures:—

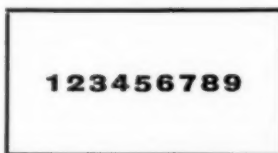


FIG. 4.

The man with good binocular vision reads 1234 with the right eye, 6789 with the left, and 5 with both eyes (fig. 4).

In latent convergence (esophoria) the middle letters overlap and are suppressed, so that the man reads something like this :—

126789

In latent divergence (exophoria) the middle letter is reduplicated, and the man reads :—

12345 56789

In latent vertical displacement (hyperphoria) the parts of the line of letters seen by each eye are on different lines or levels, thus :—

12345

56789

All these phenomena can be seen by the surgeon himself who has perfect binocular vision, when he causes disturbance of his muscles by putting a prism before one eye, or more simply by just displacing one eye slightly by gentle pressure with one finger. It is astonishing how readily this simple device displays latent irregularities in the muscle balance of the eyes.



FIG. 5.

One of the children's test cards.

In the case of children suspected of squint, their capability for binocular vision can be ascertained by the diaphragm test more easily, and at an earlier age, than by any other means. There are a series of bold and simple drawings of familiar objects supplied with the instrument (fig. 5). The child looks at a pair of pictures through the hole of the diaphragm; if they are named promptly there is good evidence that the vision of one eye is not suppressed. The youngest patient I have yet tried with the test was a girl aged 2 years and 5 months. She had not seen those particular pictures before, but she named them all correctly, looking at them through the hole, and she wanted to see some more.

(3) *Exercises for Squinters.*

Monocular alternate fixation and binocular fusion can be practised through a series of graded tests, from coloured patches, pictures, and large letters to small print.¹ When one eye, by reason of disuse, has a lesser visual acuity than the other, the superiority of the better eye can be reduced by paralyzing the accommodation of that eye with atropin, by shading the half of the tests to be seen with that eye, or by placing such a glass before that eye as will reduce the vision to an equality with the weaker eye.

(4) *Detection of Malingerers.*

We cannot have too many tests for the detection of feigned monocular blindness, but a good test must be *simple*. Here is one that is so simple that the surgeon cannot get tied up in using it, yet it is so subtle that an expert can be trapped.



FIG. 6.

Arrangement of test card with coloured patches.



FIG. 7.

The same test card seen in the hole of the diaphragm when the eyes are fixed on the pointer projecting into the upper part of the hole.

There are test cards with squares printed thereon, coloured red and green, or black for use with the colour blind. The squares are printed right and left; any number may be used, but one each side is enough; and the patches are on different lines, so that they cannot be fused by convergence (fig. 6).

The cards are reversible, so that the red and green cards can be exhibited four different ways: red to right, to left, above, or below.

One of these cards is put in the rack, and the patient asked what he sees: (1) He may be told to look through the hole; then he sees the

¹ A very cheap form of instrument, with test cards, is made for patients' use.

patches as they are on the card and must name them. Suppose, an extreme improbability, the man knows the patches are seen by crossed vision and correctly evades naming the patch seen by the pseudo-amaurotic eye; yet he does not escape, for the surgeon watching the man's eyes will see *the co-ordinate movements of the eyes as he looks from one to other area of crossed vision*. A man truly blind of one eye does not do this. In fact the occurrence of this co-ordinate movement of the eyes is so definitive that it is not necessary to invite the man to perjure his soul by requiring an answer by his mouth—the true answer is given unconsciously by the movement of the eyes! (2) The man may be told to look at the pointer and so converge; then he gets homonymous diplopia for the patches, and one appears above the other (fig. 7). When seen in this latter fashion it is impossible for the patient to guess whether one or other eye, or both eyes, sees the patches; even the cards may be changed so that the red is seen by the right and left eyes alternately, yet the change will not be detected.



FIG. 8.

The tests are so good that there is no objection to the patient seeing all the test cards laid out on the table beforehand. During the testing the surgeon has the man's eyes under perfect observation; at the suggestion of a wink on the part of the man the test can be dipped and obscured. Lastly, even when the eyes differ considerably in visual acuity the test with coloured patches can be used successfully, for colour can be perceived when form is obscure.

(5) *Physiological Alternation of Perception and Suppression of Diverse Images.*

There is one test with which very curious results may be obtained. On one half of a test card is drawn a cross of St. George, on the other half is drawn a cross of St. Andrew (fig. 8). The crosses are placed in such relations that when the test is in position and the pointer is set in the hole of the diaphragm and the two eyes are fixed upon it, the images of the crosses are superimposed and fused. Now for a moment

the fused images of the crosses present the appearance of a star of eight points. But it will be found that this appearance is not constant for the whole time of the observation; there succeeds an alternation in the perception of the images seen by the right and left eyes, so that, as though by an electric flashing sign, the crosses of St. George and St. Andrew pulsate upon the screen. The experiment can be varied by the use of a variety of geometrical figures, parts of circles, &c.

The effect is very curious, and the seemingly definite rhythm of the alternation suggests some reason for the phenomenon. There are two possible explanations: (1) that fixation is not constant and that unconsciously it falls off, but is renewed when the fading of the images occurs; (2) that, since in perfect binocular vision the two maculæ and the two halves of the brain have learned to view and perceive but one object of fixation at a time, the brain is now incapable of retaining constantly the perception of two dissimilar objects seen by the two maculæ. The brain does it for a moment, at the instant of the first attempt, but then the image of one and then of the other object is perceived and suppressed in turn. We may suppose the visual apparatus reverts to a primitive bilateral and separated condition.

CONCLUSION.

In conclusion, I suggest that the "*Diaphragm Test*" puts a very useful and handy every-day servant in the hands of the ophthalmic surgeon, one that will tell him many secrets with a minimum of explanation and direction to the patient; and that the test is not unworthy to be considered one of the simplest methods for the experimental demonstration of binocular vision.

Neurological Section.

June 24, 1909.

Dr. J. A. ORMEROD, President of the Section, in the Chair.

Amyotonia Congenita.

By JAMES COLLIER, M.D. (with GORDON HOLMES, M.D.).

At the clinical meeting of this Section held in the summer of last year I brought before you several cases of amyotonia congenita which aroused considerable interest and discussion; and the discussion was principally concerned with the relation of amyotonia to the myopathies, and several of those who took part in the discussion held that amyotonia was a variety among the myopathies. It was held that one of the cases that I showed as a case of amyotonia was indeed a case of myopathy, and should have been separated from the other cases. Now this patient died of broncho-pneumonia in St. Bartholomew's Hospital early in the year, and, owing to the kindness of Dr. Ormerod, we were able to obtain the pathological material. At the time this material was obtained I had under my care in the National Hospital the very striking case of amyotonia which has been brought for your examination to-night, and I obtained the consent of the parents of the child to remove a large piece of one of the affected muscles for microscopic examination.

Our object to-night is to demonstrate to you the pathological changes that we have found in these two cases, and especially to draw your attention to the condition of the muscles in two stages of the disease: at a late stage, for the one case died at the age of seven years; and at an earlier stage, for the case that you have seen to-night is but three years old. And, further, to point out the striking clinical features of the

disease as seen in the case presented, and to invite your discussion of the correlation between certain of the clinical and pathological features that present some difficulties.

The case before you is that of a child aged $3\frac{1}{2}$. He is the fifth and youngest child of healthy parents, and the other children are strong and well. The birth was natural and occurred at full term. From the time of birth his mother noticed that she could put his napkins on much more easily than those of her other children, because his legs went into the right position for this operation so easily, and that he did not kick like the other children. She noticed no other peculiarity till the child was a year old, when she tried to stand him up and found that his legs doubled up anyhow under him. He has never been able to stand or to walk. When 18 months old he learnt to creep round the table by wriggling his body about. As he got older and heavier he lost this accomplishment. He can sit up, but the least touch sends him over, and when over in any direction he cannot regain the sitting position. He cannot turn in bed or move his legs from the position in which they are placed. There is no affection of the muscles of the face. Swallowing is normal. There is no obvious wasting of the muscles of the neck, but the power in the neck muscles is very poor. The muscles of the abdomen and back are weak; the child can sit up, but he cannot raise himself if lying down. The diaphragm and intercostals act well, as seems to be an unvarying rule in these cases. In the upper extremities the muscles have a peculiar, soft, homogeneous feel, and cannot be differentiated from the skin and subcutaneous tissues. The muscles are small, but they are equally small throughout the limbs, and there is no local muscular wasting. Passive movements at all the joints are very free. Some hyper-extension is possible at the elbow-joints, and this is very conspicuous at the wrist and at the finger-joints. If the arm be held at the elbow and shaken, the flail-like movements of the hand and fingers are very striking. The power in the upper extremities is very poor, and the weakness seems to be the same in the proximal parts of the limb as in the periphery. He can make very few useful movements with the hands. In the lower extremities there is apparently no volitional movement, either at the hip or at the knee. All movements of the ankles and toes can be performed, but these are very feeble. The hypotonicity of the muscles of the thigh and calf is very pronounced, and the feet can be easily touched together behind his head. There is a little contracture of the calf muscles and hamstrings. The hyper-extension at the ankle so that the dorsum of the foot comes to lie in

contact with the front of the tibia, that has been a feature of many of the recorded cases, is not present in this case, neither are the feet conspicuously long and padlike. The deep reflexes have been persistently absent. The superficial reflexes are normal. The sphincters are unaffected. Sensibility is unaffected. There is no lowered sensibility to the faradic current as has been observed in many of the cases. This child objects so strongly to the faradic current that no satisfactory examination of the electrical excitability of the muscles has been made.

Since he has been under my care this child has not shown any sign of improvement, but, on the other hand, it seems quite clear that he is weaker than he was four months ago, in spite of treatment. This is the only case to show an increase of the weakness under observation, either in our own experience or in the literature of the subject.

We present to you this case as a typical case of amyotonia congenita of somewhat severe degree, the distribution of the affection being the classic one: the face, tongue, and muscles of mastication are exempt, as are also the respiratory muscles. All the rest of the skeletal muscles are affected, the limbs more than the trunk, the lower extremities more than the upper extremities. I wish especially to draw your attention to the extreme and unusual degree of the affection in the thigh muscles, in which paralysis is practically complete, and to point out to you that it is from one of the thigh muscles that our specimens to be shown you directly by Dr. Holmes are taken.

In drawing your attention to the striking features which the recorded cases of amyotonia have presented, I wish to point out how little these cases have differed in clinical aspect. Oppenheim's original description would serve almost word for word for more than one half of the thirty published cases, and the other cases differ chiefly in the degree of the affection and in the extent of its distribution. The absence of any family tendency has been without exception, and it is unfortunate that Sorgente, an Italian physician, should have published two cases under the name of amyotonia who were children of the same mother. In these cases complete motor and sensory paralysis came on soon after birth and proved rapidly fatal. No pathological examination was made. These cases were obviously not cases of amyotonia. They were clinically identical with a familial disease that came under my observation some years ago, in which five out of eight children of the same mother were affected fatally at periods varying from a fortnight to six months after birth. One of these cases was examined pathologically by Dr. Batten, and published by Dr. Beevor in *Brain*.

While the pathological changes which we are putting before you this evening bring amyotonia congenita closely into line with the myopathies from the standpoint of morbid histology, yet, clinically, these diseases are very distinct, and we wish in this place to point out and to emphasize the very striking clinical dissimilarities between the two diseases. The chief points of distinction are as follows:—

- (1) The absence of any familial tendency.
- (2) The onset, either congenital or appearing rapidly after acute illness.
- (3) The course, tending usually to amelioration.
- (4) The distribution of the muscular affection.
- (5) The invariable absence of any tendency to spread to regions previously unaffected.
- (6) The absence of any local muscular weakness or wasting.
- (7) The condition of the deep reflexes, absent at first and subsequently returning.

The tendency to a familial distribution of the disease is so constant in all the types of myopathy that the absence of any history of familial incidence among the large number of cases of amyotonia that are now on record is an important clinical distinction between the two maladies, and it argues a fundamental pathological difference between them.

The symptoms of amyotonia have in nearly all cases been apparent at the time of birth, whereas in the myopathies the subjects are normal at the time of birth, and the symptoms appear slowly and without apparent cause at a very variable period after birth, as in other atrophic diseases. In the rare cases where amyotonia has appeared post-natally, and three only of such cases have been recorded—namely, those of Leclerc, of Comby, and of Collier and Wilson—the symptoms have appeared after acute illness, and have reached their maximum intensity within a few days—an event that has never been recorded in connexion with myopathy.

Speaking broadly, the course of the affection is an opposite one in two maladies, for while there are cases of myopathy which cease to progress, such cases are the rare exception, and the usual course is one of progressive deterioration; while in amyotonia there are cases which show no improvement, yet progressive amelioration is the rule, and this may proceed so far as to give useful physical power to the affected limbs. The distribution of the affection, again, is quite different in the two conditions. In myopathy the muscles are affected one by one locally, but in amyotonia the affection is general and equal in all the

muscles of the affected limbs. Again, in myopathy there is always some one muscle or group of muscles which shows weakness and wasting out of all proportion to that shown by the other muscles, as, for example, the lower part of the pectoralis major in the idiopathic and pseudo-hypertrophic forms and the muscles of the pectoral girdle in the Landouzy-Déjérine type, but in amyotonia such local weakness and wasting is never met with. The periphery of the limbs, which in amyotonia often shows the more conspicuous affection even at the earliest period, is the least attacked, and the last to be attacked in any form of myopathy. How often is any affection of the intrinsic hand muscles seen in myopathy? Yet in amyotonia the affection of these is invariable if the upper extremity be involved. The diseases which we call myopathies spread from one group of muscles to another with a slow march, but such a spreading to muscles previously unaffected never occurs in amyotonia.

In myopathy the deep reflexes are present or absent according to the degree of involvement of the muscles concerned in the deep reflex. At first present, they diminish and disappear for ever as the muscular affection increases. In amyotonia, on the other hand, the deep reflexes are always absent from the first in the affected regions, and in several cases they have appeared where considerable improvement has taken place. The reappearance of the deep reflexes, after they have been persistently absent for months and years, is unknown in the history of myopathy.

It is upon these grounds that we contend that amyotonia is a separate clinical entity from the myopathies, and that there is no utility in departing from the name which Oppenheim has given to this disease, and burying it among the group of diseases termed myopathies with which it seems to have no etiological relations and to which it bears only the most superficial clinical resemblance.

PATHOLOGICAL FINDINGS.

The muscles of the case in which a post-mortem examination was obtained, and those excised from the patient presented before the Section this evening, were fixed in Zenker's fluid and sections were prepared by the celloidin method. To the unaided eye the muscles were very pale and were much infiltrated by fat, and in the portion of the biceps cruralis excised from the younger patient scarcely any muscle tissue could be recognized. Very few bundles of normal fibres could be found

in the more severely affected muscles. The majority of the fibres were much reduced in size and were atrophic, their average diameter not exceeding 12μ as a rule. In cross section (fig. 1) they appeared very irregular in shape, generally round or oval, but when cut in their length they were seen to be fairly uniform in calibre (fig. 2). The cross striation was fairly well preserved. In many of the atrophic fibres of the younger patient the nuclei seemed to be scarcely in excess of the normal, but in other fibres, and in practically all the atrophic fibres of the older patient, there was a considerable proliferation of the sarcolemmal nuclei. A smaller number of the fibres, on the other hand, were

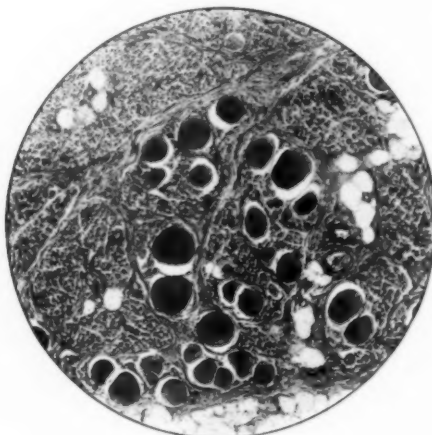


FIG. 1.

enormously enlarged, measuring up to 150μ in diameter. These large fibres were almost round in cross section, their cross striation was fairly distinct, and their nuclei were rarely in pathological excess. Definite regressive changes were, however, present in many of these hypertrophied fibres, some were invaded by the sarcolemmal nuclei or contained vacuoles, while others were seen undergoing longitudinal fission (fig. 3).

There was a great increase of connective tissue in the affected muscles, arranged both in massive bundles and in delicate strands penetrating between the individual fibres. Much of the muscle tissue was replaced by fat. The muscle spindles were normal. In the younger

patient the intra-muscular nerve fibres were small and were poorly myelinated. In the elder patient, upon whom a post-mortem examination was made, the anterior spinal roots were very small, and there was a marked deficiency and atrophy of the anterior horn cells.

Drs. Collier and Wilson [2] have recently, in their review of amyotonia congenita, abstracted the reports of Spiller, Baudouin, and Bing, who have examined muscles from cases of this disease. The pathological changes that we describe here are practically identical with those observed by Baudouin [1], whose examination was far more complete

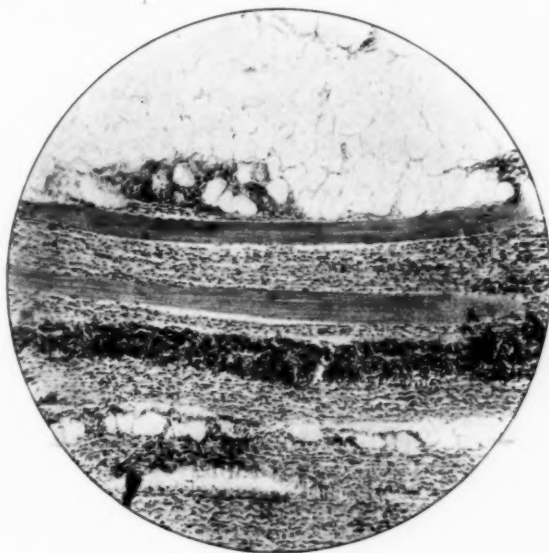


FIG. 2.

than those of Spiller and Bing. Spiller [4] found somewhat similar changes in the muscles, though he does not record the presence of hypertrophied fibres, and in his case he describes the spinal cord as normal. We may therefore conclude that the changes we describe here are those that characterize the disease. It seems that the muscle changes are the essential pathological lesion. Their significance may be further discussed, as on the one hand they may be regarded as due to regressive changes occurring in previously normal fibres, and on the

other hand as being due to defective development. The proliferation of the muscular nuclei and the great increase of the connective tissue and of the fat in the muscles make the former interpretation the most probable.

A point of special interest is the relation of amyotonia congenita to the myopathies from the pathological point of view. There is certainly a great similarity between the muscle changes in the two conditions, and the spinal changes similar to those observed in one of our cases have been repeatedly observed in cases of true myopathy [3].

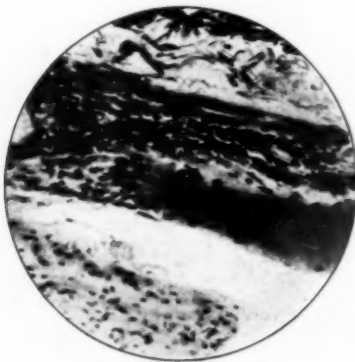


FIG. 3.

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- [1] BAUDOUIN. *Sem. med.*, 1907, xxvii, p. 241.
- [2] COLLIER and WILSON. *Brain*, Lond., 1908, xxxi, p. 1.
- [3] HOLMES. *Rev. Neurol. and Psychiatry*, Edin., 1908, vi, p. 137.
- [4] SPILLER. *Univ. Pennsylv. Med. Bull.*, Philad, 1904-5, xvii, p. 342.

DISCUSSION.

Dr. COLLIER said that in the paper on the subject which Dr. Wilson and he published last year he mentioned that the first cases of the kind published in this country were those shown before the Society by Dr. F. E. Batten. Dr. Guthrie had now told him that in 1899 he showed before the Clinical Society a case which he described as congenital paralysis due to myopathy, in a boy aged $4\frac{1}{2}$. Dr. Guthrie had sent him a description, and from that there was no doubt that it was a typical case of myotonia, and he apologized to Dr. Guthrie for not having looked more carefully through the literature. In the next contribution which he (Dr. Collier) published, which he hoped would be shortly, he would put the matter right.

Dr. F. E. BATTEN said the demonstration which had just been given was a most admirable one, and on the pathological side he thought he need say very little. Dr. Holmes seemed to have demonstrated pathologically that these cases of "myotonia congenita" showed the characteristic changes of myopathy. At a former meeting of the Section at which similar cases were shown he (Dr. Batten) had on clinical grounds expressed the opinion that they were cases of myopathy, but pathological proof that they were so was wanting. There was now definite pathological proof of the nature of the changes present in the muscles. There were one or two points which Dr. Collier had referred to as evidence that the disease which was named myotonia congenita was essentially an entity, and different from myopathy. It would take a long time to go into all the various points, but he would like to criticize one or two statements which Dr. Collier had made. He said that myopathy was never present at birth; but it might be pointed out that the facial weakness in the facio-scapulo-humeral type of myopathy was almost certainly present at birth and could be definitely recognized in the first few weeks of life. It was, of course, difficult to prove in other forms of myopathy that weakness was present at birth, but the history in many cases pointed to such being present at a very early period of life. Dr. Collier said myopathy was always progressive; but many cases of Landouzy-Déjérine paralysis remained stationary for years. He might go on and criticize the various points which Dr. Collier had raised in the clinical distinction between myopathy and myotonia congenita, but he thought that enough had been said. Dr. Batten said if Dr. Collier would take the trouble to look out cases under the title of myopathy, he would find fairly numerous cases similar to those described by Dr. Guthrie. Some of these were described as having hard calf muscles, but without hypertrophy, and some with contractions; but practically they were cases allied to those of the "simple atrophic type" of myopathy, and he would find them in Erb's monograph. He had expressed his opinion clinically at a former meeting that these cases of so-called "myotonia congenita" were but one of the types of the great group of myopathies, and pathologically it had now been shown that they belonged to that group.

Dr. LEONARD GUTHRIE joined in the thanks to Dr. Collier and Dr. Holmes for their most extremely interesting and valuable communication, and he thanked Dr. Collier especially for his kindly reference to a case which he (Dr. Guthrie) showed ten years ago, one which, if shown to-day, would undoubtedly be regarded as an instance of amyotonia congenita. On the other hand, he agreed with what Dr. Batten had said, that, although the case was not described as such, similar cases had no doubt been described elsewhere; and he was very much of Dr. Batten's opinion, that the cases were pathologically cases of myopathy, though differing, perhaps, from other cases of myopathy which had been described. Speaking of the two cases of post-mortems, those of Spiller and Baudouin, he thought some discrepancy between those cases might be explained by a consideration of the ages of the patients. Baudouin's patient was aged 4 months, and in that case, in addition to the changes in the muscles described, he found certain very definite changes in the spinal cord, perhaps more advanced than those which Dr. Holmes had described, and a very great decrease in the size of the anterior roots, a deficiency in the cells of the anterior horns, and signs of want of myelination, which he attributed to want of development in the nerves themselves. In Spiller's case the child died at the age of 1 year 10 months, and no changes in the nervous system were described. The suggestion might therefore be made, though he did not wish to press it, that those cases might be both myelopathic and myopathic; that the changes in the muscles might accompany want of development also in certain tracts of the spinal cord; but that, in time, the spinal tracts underwent the development which they should. Therefore the myopathy outlasted the myelopathy. He thought a myelopathic origin of the condition was still possible, and that they need not be purely myopathic, although he thought the evidence was strongly in favour of their being myopathic. He did not quite follow Dr. Collier's reasons for excluding them from being regarded as myopathies.

Dr. COLLIER, in reply, said he thought Dr. Batten's point with regard to the affection of the face very early or at birth in cases of facio-scapulo-humeral myopathy was not a very great argument, because he was not sure it was proved that the muscles affected in those cases were not congenitally absent, and he did not think the slight affection of the face was necessarily comparable to an exceedingly severe affection of all the muscles. If Dr. Batten convicted him of being dogmatic for purposes of clearness, he could say Dr. Batten was mistaken, as he, Dr. Collier, did not say all myopathies were progressive, as he had just heard from an old patient with Landouzy-Déjérine paralysis, aged 67, the father of a well-known patient at Queen Square, whose condition had not progressed at all in the last sixteen years during which he had known him. In regard to Dr. Guthrie's case, it was with reference to publications in this country to which he (Dr. Collier) referred. It was the first case of the kind in English of which he had any knowledge. Dr. Guthrie's remarks with regard to the ages of patients, and that the changes in the spinal cord might disappear while the changes in the muscles persisted,

were very interesting and demanded further investigation. With regard to the question of myopathy or myelopathy, he thought that question must remain unsettled until there were much finer methods of examination. He had always rather leaned towards the myelopathic origin of cases of so-called myopathy, but it did not depend upon definite facts. His contention was for the clinical entity of the disease, and not for its pathological separation from the myopathies, and he held to his opinion that it was clinically very distinct from the myopathies. Dr. Batten, less than anybody else, should fall into the danger of making a mistake between the diagnosis of it from those other forms of myopathy which he knew so well.

Dr. GORDON HOLMES, in reply, said he had no definite point to reply to, but he would emphasize that spinal changes quite analogous to those observed by Baudouin and themselves in amyotonia congenita had been observed in a large number of cases of myopathy. Whether they were primary, or merely secondary to muscle changes, or merely a coincident disease, was a very difficult point to discuss, particularly with regard to cases in which there was evidence of disease from birth. He would remind his hearers—and it was a point which required to be seriously considered in discussing those cases—that till birth the nutrition and development of the muscle fibres was independent of the central nervous system, as is shown by the normal development of the muscles in cases of complete amyelia.

A Critical Description of the Brain of a Degenerate (convict and murderer).

By ALBERT WILSON, M.D.

Two years ago I showed several criminals on different occasions at meetings of the Medico-Psychological Association. I had formed the opinion that they differed from the normal type of mankind. Physically fit, some showed stigmata of degeneration, others had none, but all were alike unstable. They were impulsive, emotional, vain, open to suggestion, and devoid of self-control. I described a criminal as having the body of a man, the impulse of youth, and the control of a child. In the discussion that followed there were two decided but apparently opposite opinions:—

(1) That the criminal is a normal being, but directed and thrown out of his true course by adverse circumstances. Such I should describe as a derelict.

(2) That the criminal is born, not made. This one I term a degenerate or a psychopath.

Both opinions are correct, for both classes exist. It is with the latter that I am concerned to-night.

Sometime later one of the men I had examined died. He was very degraded and also a murderer. His clinical history was briefly that his occupation was a labourer, and he managed to keep a home till he was aged about 40, so that he could not easily be described as an imbecile. He was very wayward, bad-tempered, and at times lazy. During a quarrel he murdered his wife, and in remorse tried to kill himself after. He drank moderately, and his father was a drunkard. He was a man below medium height, about 5 ft. 4 in., but the only stigmata about him were his ears, which were so crumpled as to appear devoid of helix. He had nothing otherwise abnormal in appearance, except that his skull, somewhat Mongolian, rose high in the parietal region. He had a large square head and regular features. After he had been some years in penal servitude he described his condition as "broke." The doctor described it as "weak-minded."

Post-mortem Examination.—His brain weighed 50 oz. without the membranes. The convolutions were large and coarse, the sulci were shallow, and the pattern was simple (*see fig. 1*). In the frontal lobe

there were four convolutions. The calcarine fissure was normal. There was, however, a marked abnormality in the occipital lobes which presented a very shrunk appearance, and their convolutions were very small. They were separated at the poles by a gap $1\frac{1}{2}$ in. wide.

I made a thorough examination of every convolution in the right hemisphere, and almost the same in the left side.

In general terms the cortex is shallow, equal to about four-fifths or less of the normal. Throughout the cortex Layers III, IV, and V, otherwise known as the granular layer, the layer of Baillarger, and the

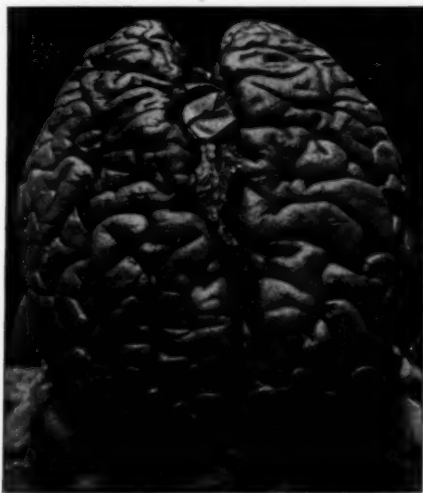


FIG. 1.

The brain of the degenerate. Note large convolutions, simple pattern, shallow sulci, and the shrivelling and separation of the occipital lobes.

polymorph layer, are as near normal as it is possible to be. That is, though shallower, they are well supplied with cells, both as to shape and number. Layer II, the pyramidal layer, is "unfinished," and in consequence Layer I, the tangential or zonal layer, is shallow.

As I am only concerned with the unfinished pyramidal layer, I must as a preliminary go over the cortex of the foetus and new-born infant. Brodmann, Bolton, and Watson have shown that not until the sixth month of foetal life is there any differentiation into cell-layers.

Previously the grey cortex is made up of the zonal layer (I) above and the remainder of neuroblasts. As we all know, the foetal cortex about the time of birth is only two-thirds of the depth of the adult. There are so many sources of error in measurements that I shall not go into

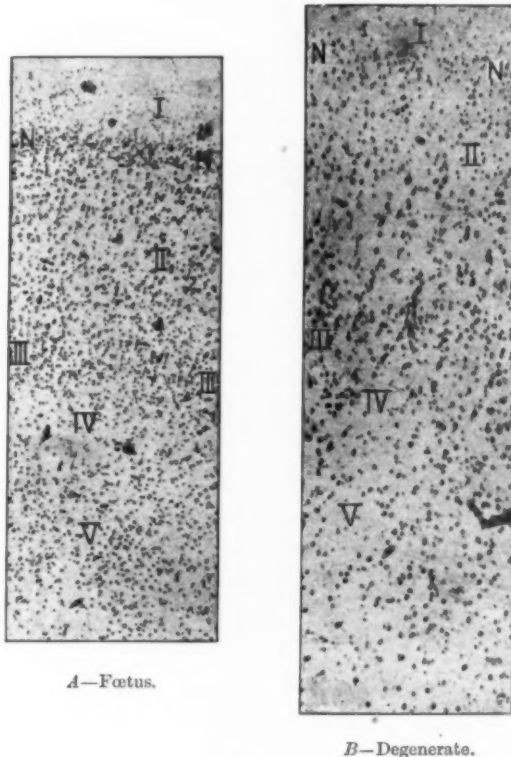


FIG. 2.

A shows the prefrontal cortex in the foetus at the time of birth (8½ months); it is magnified 70 times. Observe all the layers are distinct, but at the top of layer II—the pyramidal layer—there is a distinct band of cells, marked *N*; these are the neuroblasts, which develop postnatally into the layer of small pyramids. In the degenerate shown their development has been arrested. *B* shows the prefrontal cortex in a degenerate (magnified 35 times only). Observe at the top of layer II, beneath the zonal layer, I, that there is a band of cells; these are chiefly undeveloped neuroblasts. In normal brains these are all well developed, and form the layer of small pyramidal cells. Layer III, the granular layer, is distinct; so also the layer of Baillarger, IV, and the polymorph layer, V.

detail. At birth Layer II, the pyramidal layer, is two-thirds of the adult depth, and Bolton considers the increased depth of the whole cortex is due to the further development of the pyramidal layer.

By the following photographs (figs. 2*A* and 3), taken from a foetus between the eighth and ninth month, I wish to demonstrate that the pyramidal layer (II) is divided naturally into two parts—deep and superficial. The deeper four-fifths or five-sixths consist of perfectly-shaped

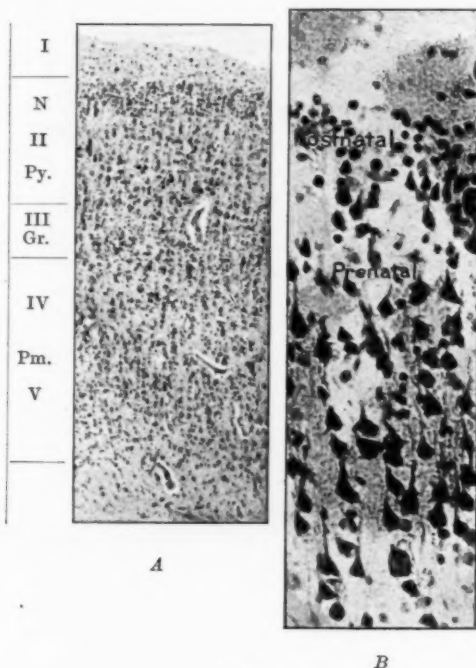


FIG. 3.

Fetal cortex at the time of birth. Ascending parietal. Note here, in *A*, the band of neuroblasts, marked *N*, which develop postnatally, and the same, highly magnified, in *B*. In the degenerate these remain mostly undeveloped.

and equipped pyramidal cells. Above these, and immediately below the zonal layer (I), is a distinct band of undeveloped neuroblasts. This band may be one-fifth or one-sixth of the depth in areas which develop late, as association areas, while it is only about one-tenth in motor areas. Clearly, Layer II at birth has a layer of prenatal development and one

which develops postnatally. This further thickening of the grey cortex is evidently due to the postnatal development of the neuroblasts, and also the fibres therefrom.

It would appear correct, then, to divide Layer II into the prenatal pyramids, which, if we may speculate, bring into existence the ancestral

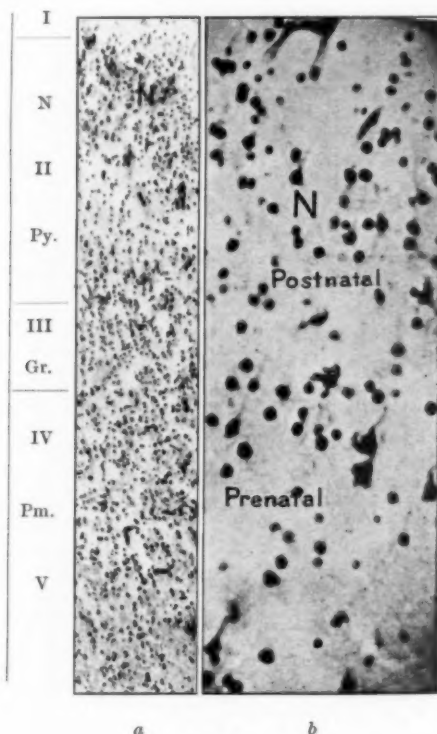


FIG. 3a.

Fœtal cortex, eighth to ninth month. First left temporal convolution near Heschl's gyrus. Observe in the lower magnification, *a*, the band of neuroblasts, marked *N*, at the top of layer II; these develop postnatally, and *b* shows them ($\times 320$) quite undeveloped. The cortex below is developed, therefore we might call that part of No. 2 which has the formed pyramidal cells the prenatal part.

traditions of the genus *Homo*, and the postnatal pyramidal layer, which develops during childhood from the fœtal neuroblasts. There is no room for disputing the fact that these embryonic nuclei can only

develop after birth into the small and, if we admit the further division also, the medium-sized pyramidal cells. We see in this late development the cause of the instability of these layers and the reason why they are first to decay in dementia. There is some foundation for believing that the development of the postnatal pyramidal layer is then

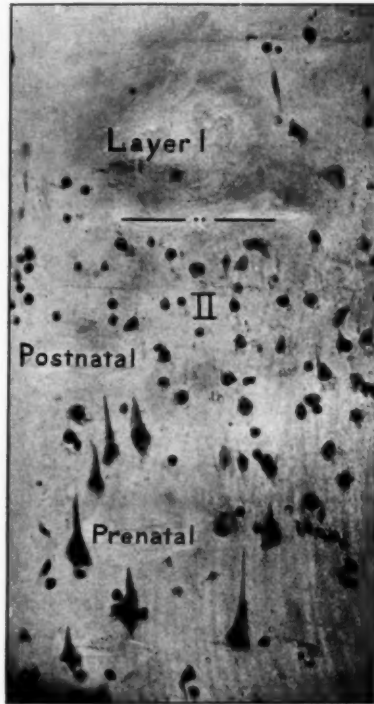


FIG. 4.

Murderer, left prefrontal. The prefrontal cortex in a degenerate ($\times 320$). The postnatal neuroblasts, already seen in the fetus, remain in an undeveloped condition. Below are some well shaped, large "prenatal" pyramidal cells.

the measure of human intelligence and the power of absorbing education. It also explains why Layer I, a layer of delicate association fibres, acquires increased depth after birth, when these neuroblasts develop their cytoplasm and fibres.

The preceding considerations unravel the mystery of the cortex of this degenerate. His pyramidal layer is normal as regards the prenatal portions, but when we examine the postnatal layers they are seen to have remained in an unfinished state. There are but few small pyramidal or stellate cells, while the neuroblasts remain in very decided



FIG. 5.

The portion of the left prefrontal immediately below fig. 4. The object is to illustrate the perfect development of the (prenatal) cells, but they are few in number and many neuroblasts among them. The prefrontal cortex is, however, the last to develop. The same backward appearance does not occur among the prenatal cells in other parts of the degenerate's cortex.

evidence (*see* fig. 4 and fig. 5). It is a fact that his postnatal development has been arrested. His brain has remained where it was when he was a child, and, if so, it furnishes a physical description of a criminal,

a man with the brain of a child (compare fig. 6 *A* with fig. 2 *A* and fig. 6 *B* with fig. 3 *A b*). This postnatal arrest is slight in motor and sensory regions, as the sections and photographs demonstrate, but is very marked in the temporal, parietal, and prefrontal regions. In the angular gyrus and in the occipital convolutions the postnatal develop-

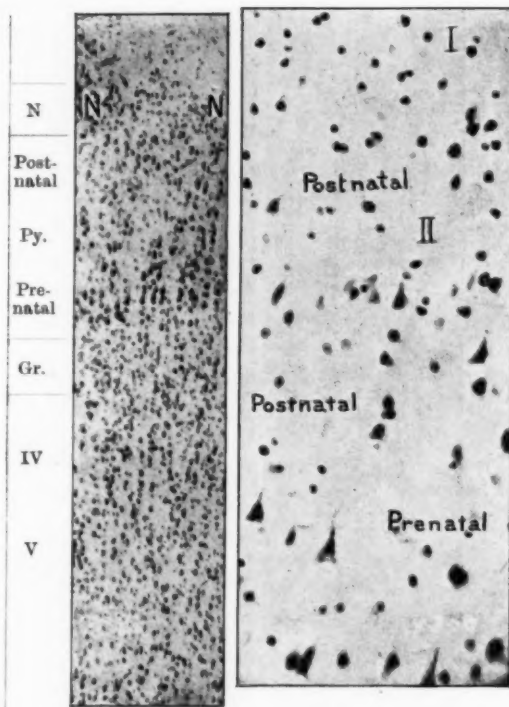


FIG. 6.

Murderer, second right frontal. The degenerate's cortex; it looks good under the low power, but note, at the top of layer II, the fatal band of neuroblasts, which should have developed postnatally. In the higher magnification the undeveloped neuroblasts are apparent, while the lower part shows good prenatal cells.

ment hardly counts, because so few of these neuroblasts have been laid down; so that almost immediately below the zonal layer we come upon the large prenatal pyramidal cells. It is essential to inquire whether

this appearance is normal or abnormal, and here I would point out the confusion in terms between what is normal and what is merely average in appearance. No unfinished structure can be normal, but amidst a very broad average there may be many defects (*see fig. 7*). Some neurologists maintain that undeveloped neuroblasts are common, but

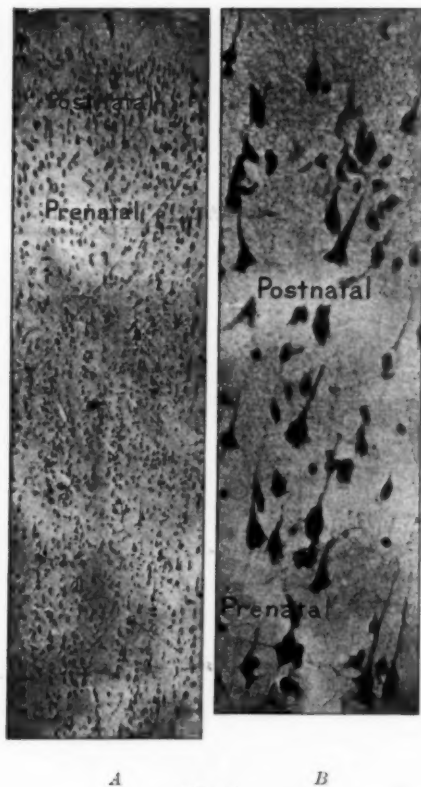


FIG. 7.

Normal brain. In A half of layer V is omitted for want of space. Note in B the perfect development of the small and medium sized pyramidal cells, which have developed from the fetal neuroblasts, and are undeveloped in the degenerate.

there have been no accurate reports of the mental condition in these cases. If abnormal, we have to decide if these appearances are evidence of insanity or imbecility.

This man clearly was considered sane at the time of the murder, nor did his brain reveal any type of insanity. He might be considered by some a high-grade imbecile, but here again we have not a scientific definition of the term "imbecile." In fact the imbecile is the connecting link between the speechless idiot and normal man, which is an unscientific

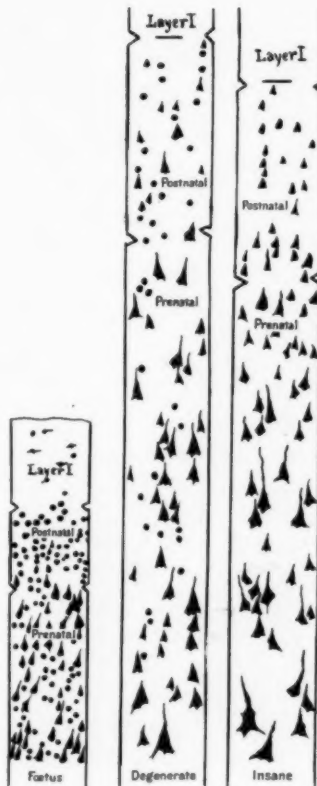


FIG. 8.

The prefrontal cortex (the pyramidal layer, II). In the left-hand column the foetal cortex, which being prefrontal is very backward (Bolton), shows many neuroblasts throughout. In the right-hand column (an adult who died of general paralysis) all the neuroblasts have developed. It has been a good cortex, but now is rapidly decaying. In the central column, which shows the degenerate, many neuroblasts all through the cortex remain, but they are seen especially at the top. It is a very unfinished cortex, hence the degeneracy (?). The cortex of the degenerate is therefore infantile in type.

position. On the other hand, the class who can go straight in favourable surroundings, but who fall or stray into crime under the stress of civilization, form a large and easily recognized section of the community. Their many symptoms hardly require to be recounted in this Society.

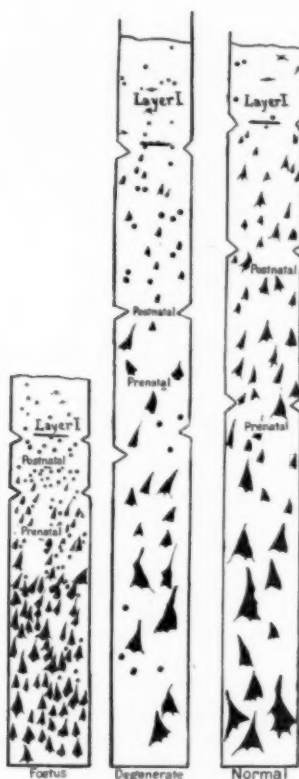


FIG. 9.

Ascending parietal convolution (the pyramidal layer, II). Note the postnatal neuroblasts in the foetal cortex, which are seen to be developed in the normal adult cortex (right side). In the degenerate (central column) many neuroblasts remain; it is therefore an unfinished cortex.

The prison physician calls them weak-minded. The Royal Commission called them feeble-minded, which is an unfortunate term, as feebleness is used to indicate a falling-away from strength or structure; whereas

these people never had strength given to them. We all agree that mentally this class is defective, and, as they have never risen to the level of normal man, we might perhaps correctly describe them as degenerates or psychopaths. They are degenerate from the type *Homo sapiens*, and they are diseased or suffering in their minds or souls. But we really require a new term, which implies the unfinished, incomplete, undeveloped, or neglected condition in which they are stranded amongst the rest of humanity.

What is that term to be? The term "ament" is too general for such cases. Another point of distinction is that imbeciles are born and not made, whereas the degenerate is made and not born. That is to say, in the imbecile cortex there is a deficiency of structure throughout, and it is prenatal; whereas in the degenerate, as shown here, the prenatal growth and structure are good. Nature is not at fault, but nurture, in the case of the degenerate.

I hesitated to bring forward a single case, but was encouraged to do so by the fact that what is evident physically is exactly what we expected to find with such mental characters and with the whole life-history of the individual. I think it would require many negative cases to annul the effect of this one. I received, however, great encouragement in reading A. W. Campbell's monograph, "Histological Studies on the Localization of Cerebral Function," in which he describes similar undeveloped appearances as to the prenatal cells in a single case of deaf-mutism. One case can only be suggestive as a workable basis. The subject requires confirmation, and there is unfortunately an ever-increasing amount of material. But in all such research a full investigation of the characters during life is essential. To search for the conditions I have described, without associating a complete life-history, would prove nothing. The critic must not lose sight of this factor. Further research should give a pathological basis to degeneracy of as much value as that of dementia or general paralysis. There are many interesting side-issues as to the possibility of transmission by heredity, curability, prevention, education, and other matters, but these do not fall within the scope of this Society.

DISCUSSION.

Dr. GORDON HOLMES said that Dr. Wilson's paper opened up a subject of very great importance, and it had been very interesting to him. But there is a good deal of danger in generalization, especially from the observations with regard to prenatal and postnatal development of the cortex. He did not think one should apply Bolton's observations to pathological work straight away. For the last couple of years he had worked almost constantly at the cerebral cortex, and he recognized the enormous difficulty there is in studying it in diseased condition. An investigation such as Dr. Wilson had just described could not be possibly completed in less than six months of continuous work, and it would be unfair to attempt to criticize the work of this time in the few minutes at his disposal. It was equally impossible to pass judgment on a few sections put up for demonstration at such a meeting as the present one. Further, he could not fairly criticize Dr. Wilson's work from his specimens, as he had not stated the technique he employed; for there was no organ in the body so liable to post-mortem change, in which the microscopical appearances might be so easily influenced by the methods of hardening, and especially by methods of preparing for sections and of staining, as the forebrain cortex. To pass judgment it would be necessary to examine a large number of sections prepared under the same conditions. In some of Dr. Wilson's specimens, however, he could not see any striking deviation from the normal. But he hoped Dr. Wilson would report his case fully, and give some further material on which to work and form conclusions as to the structural basis of criminality.

Dr. WILSON, in reply, said the man in question died eighteen months ago, and he (Dr. Wilson) had since been occupied in examining the cortex in his spare time. It was hardened in formalin, and the sections were stained with Heidenhain, polychrome, and Nissl blue, according to the technique employed at Claybury and other laboratories. No one could put cells or undeveloped nuclei into sections if they were not already there. The whole investigation shows that the foetal cortex and the degenerate's unfinished cortex are closely related, and so far differ from the normal adult. There was either malnutrition or some constitutional taint, such as that of syphilis, or the unknown inheritance of chronic alcoholism, which arrested development.

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VOLUME THE SECOND

COMPRISING THE REPORT OF THE PROCEEDINGS FOR THE
SESSION 1908-9

OBSTETRICAL AND GYNÆCOLOGICAL SECTION



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1909

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The Council think it right to state that the Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

Obstetrical and Gynaecological Section.

October 8, 1908.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

Dystocia due to Distension of the Urinary Bladder of the Fœtus, with Remarks on Renal Secretion *in utero*.¹

By JOHN EVAN SPICER, M.D.

DIFFICULT labour as the result of morbid enlargement of the foetal bladder is referred to in most works on obstetrics, and imperforate urethra is the cause assigned. At the beginning of 1907 a case occurred in the maternity charity of the London Hospital, while I was resident accoucheur, and it became my duty to deal with it.

I propose to give some details of this case in the present paper and to discuss certain of the problems which have arisen during my investigation of the foetus.

CLINICAL HISTORY OF THE CASE.

Mary H., aged 29, 3-para, a short but well-built woman. The patient was in good health and working as usual in her home, when labour pains unexpectedly began about the sixth month. She had had three previous confinements at full term, delivery being normal in each case. The children, who are alive and well, show no signs of malformation or disease. No history or signs of syphilis in either parent. No symptoms of tuberculosis. No history of maternal impression during pregnancy.

The only unusual points concerning the present pregnancy noticed by the mother were that her abdomen was larger than usual and that

¹ Part of a thesis on "Malformation of the Urethra in the Fœtus as a Cause of Dystocia," read at Cambridge for the M.D. degree, 1908.

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she had not quickened. Pains began in the morning of January 5, but continued slow and weak throughout the day. At 11 p.m. there was slight hæmorrhage. At 9 a.m. on January 6 my assistant was called to the case. The os uteri was by this time fully dilated, the membranes already ruptured, the vertex engaged in the brim and the cord prolapsed beside the head. The occiput was posterior. No exact information can be given as to the amount of liquor amnii, but from the appearance of the bed it was not excessive. If hydramnios was present, it cannot have been severe. The abdomen appeared nearly as large as at full term, but the fœtal parts could not be made out; the fœtal heart was not heard and the cord was pulseless.

On being summoned to the case at 10 a.m. I found the head in the pelvis and the other signs as above, but, in spite of the fact that the head was not large, the effect of the pains on the position of the fœtus was *nil*. I anæsthetized the patient and tried the effect of forceps. Traction on the head failed to deliver the body, and, instead, the head came off. By inserting the hand into the vagina and past the child's chest it was now easy to detect that the delay resulted from a fluid enlargement of the child's abdomen. Making use of a "perforator," therefore, I thrust the instrument through the chest wall and punctured the diaphragm. Immediately a considerable quantity of nearly clear ascitic fluid escaped from the peritoneal cavity, and I anticipated that delivery would forthwith be possible. But it was not so: the shoulders advanced only slightly. On passing the finger into the abdominal cavity through the opening made by the perforator in the chest and diaphragm, a large tense cystic swelling, which afterwards proved to be the enormously distended bladder, could be felt practically filling up the cavity, and it was not until this had also been punctured that delivery was accomplished. At once a far larger quantity of clear fluid gushed out. I greatly regret that I did not contrive means to measure the amount of this fluid and have it analysed. There was no further difficulty in extracting the child. The placenta followed in due course and the uterus contracted well. The puerperium was perfectly normal and the patient made a good recovery.

OBSTETRIC CONSIDERATIONS.

Cases such as the one just described are rare, but a considerable number have now been recorded, and their importance is well recognized. Until the abdomen of the fœtus has been opened it is quite

impossible, however, to tell whether the enlargement is due to distension of the bladder, to ascites, or both. In my own specimen, as in some others, both were present. In any case the points in diagnosis are precisely similar, and from the purely obstetric point of view the result is the same.

Diagnosis is difficult, chiefly because the abnormality is so rare that it is not suspected. As a rule it has not been discovered until prolonged delay has occurred. It seems from the history of the other cases to be found in the literature that an abnormally large percentage of the cases occur in primigravida, though by no means all—multipara of all degrees have been affected. As a recurrence it is of extreme rarity; I have only been able to find one recorded case, and of this there is some lack of detail. Breech presentation is relatively frequent, though the vertex generally presents. Labour is premature as a rule. The uterus is abnormally distended for the period of pregnancy reached, but foetal parts can seldom be recognized. Where the point is mentioned in the records, "hydramnios" or "twins" seems almost always to have been the diagnosis previous to the rupture of the membranes. It must be noticed, however, that *ballottement*, which is so characteristic in hydramnios, is generally absent. Hydramnios has been known to accompany the condition, and oligohydramnios is reported in a few cases, but neither the one nor the other is constant. After rupture of the membranes the uterus remains distended, and still no foetal parts can be made out satisfactorily. Labour is prolonged, the pains are weak, and little or no advance is made by the presenting part. In vertex cases the head is felt to be not abnormally large, and "excessive size of the shoulders" becomes the favourite diagnosis. If this diagnosis had never been accepted without at once excluding fluid enlargement of the abdomen by passing up the hand in front of the child's chest, much time would have been saved and less damage done. Frequently the true diagnosis has not been arrived at until the patient has been exhausted by long and unsuccessful methods of delivery, and more than one case of rupture of the uterus has supervened.

TREATMENT.

There can be no doubt as to the best form of treatment in cases of dystocia from fluid distension of the abdomen from whatever cause. In minor degrees delivery may perhaps be effected by traction on the head, and several such cases are reported, though very few have survived for more than an hour or two. But if the enlargement is great and this

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method does not succeed quickly, the fœtal abdomen should be tapped. It is not a very difficult procedure, and, if performed carefully, always leads to eminently satisfactory results at once. The "perforator" is perhaps the most useful instrument for the purpose. One or two cases are said to be on record where the child has been born alive after the use of a trocar and cannula, but I have not been able to trace these. I should recommend the use of this instrument on purely scientific grounds, for we need a more careful analysis of the fluid found; but a satisfactory form of trocar and cannula is usually not immediately at hand, and delay must under no circumstances be permitted. As the conditions found in these severe cases are incompatible with extra-uterine life, at any rate for more than a few hours, one should not hesitate to perforate at once, remembering to destroy the medulla at the same time if there is any likelihood of the child breathing.

One of the first cases to be published where fluid enlargement of the abdomen led to dystocia appeared in 1681, in F. Mauriceau's *Traité d. mal. des femmes grosses*, third edition, Paris. The author there gives a most graphic account of the delivery, which has been translated into English by Fordyce¹ and quoted by Ballantyne in his "Ante-natal Pathology."² This is a very interesting case and in many ways quite characteristic. The following cases of dystocia arising from distension of the fœtal bladder I have found in the literature:—

- AHLFELD, figured in Norris's "Text-book of Obstetrics." *Arch. f. Gyn.*, 1873, iv, p. 161.
 BALLANTYNE. "Ante-natal Pathology" ("The Fœtus"), 1902, pp. 379, &c.
 BILLARD. "Traité des maladies des nouveau-nés," 1828.
 BRUCE, reported by DUNCAN. *Edin. Med. Journ.*, 1870-1, xvi, p. 163.
 CORMACK. *Month. Journ. Med. Sci.*, Lond. and Edin., 1844, iv, p. 660.
 COUVELAIRE. *Bull. Soc. Anat. de Paris*, 1900, lxxv, p. 287.
 DELBOVIER. *Canstatt's Jahresh.*, 1843, i, p. 77.
 DEPAUL. *Bull. d'Acad. de Méd., Par.*, 1850, xv; "De la rétention d'urine pendant la vie fœtale," *Par.*, 1860.
Ibid. *Gaz. hebdom. de Méd. et Chir.*, *Par.*, 1860, vii, pp. 324, 342, 371.
 DEVILLE. *Bull. de la Soc. Anat. de Paris*, 1846, xxi, p. 109.
 DUNCAN, MATTHEWS. *Edin. Med. Journ.*, 1870-1, xvi, p. 163 (two cases).
 DUPARQUE. "Des maladies des femmes et des enfants," *Ann. d'Obstet.*, 1842.
 FABRIS, quoted by BALLANTYNE. "Ante-natal Pathology" ("The Fœtus"). 1902, p. 380.
 FEARN. *Lancet*, 1894-5, ii, p. 178.
 FORDYCE. *Teratologia*, 1894, i, pp. 61, 143 (several cases quoted).
 GALABIN. *Obstet. Journ.*, Lond., 1877-8, v, p. 192.
 GAUDON. (Three cases.) See DEVILLE and DEPAUL.
 HAY. *Med. Chir. Trans.*, 1835, xix, p. 239.
 HOWSHIP. "Urinary Diseases," 1823, p. 374.
 KENNEDY, see CORMACK. *Journ. Med. Sci.*, *Dubl.*, 1840.

¹ *Teratologia*, 1894, i, p. 61.

² "The Fœtus," p. 356.

- KING. *Guy's Hosp. Reports*, 1837, Ser. 1, ii, p. 508; museum specimen, 2551/47.
 LEE. *Med. Chir. Trans.*, 1835, xix, p. 238.
 LEFOUR. *Le Progrès méd.*, Par., 1877, 2 Sér., v, p. 413.
 LÖWE. *Frag. med. Wochenschr.*, 1893, xviii, p. 343.
 LUSK. *Trans. N. Y. Obstet. Soc.*, 1879, i, p. 399, and *Amer. Journ. Obstet.*, 1878, xi, p. 781.
 MARSHALL and LINDSAY. *Trans. Glasg. Obstet. and Gyn. Soc.*, 1900-2, iii, p. 81.
 MOREAU. *Bull. Acad. de Méd.*, Par., 1851, 2 Sér., xvii, pp. 904, 909.
 MUELLER. *Arch. f. Gyn.*, Berl., 1894, xlvii, p. 130.
 OLSHAUSEN. *Arch. f. Gyn.*, Berl., 1871, ii, p. 280.
 O'MEARA. *Trans. Obstet. Soc. Lond.*, 1887, xxix, p. 54.
 PATTERSON. *Brit. Med. Journ.*, 1904, i, p. 190.
 PORAK. *Arch. de Tocol.*, Par., 1885, xii, p. 1073, and *France méd.*, Par., 1885, ii, p. 1441.
 PORTAL, see DEPAUL and BALLANTYNE. "La pratique des accouchements," 1685.
 RAYNER. *Brit. Med. Journ.*, 1892, ii, p. 1384.
 ROBERTSON. *Glasg. Med. Journ.*, 1889, xxxii, p. 113.
 ROSE, quoted by DIENST. *Arch. f. path. Anat.*, Berl., 1898, cliv, pp. 81-138.
 SCHWYZER. *Arch. f. Gyn.*, Berl., 1893, xliii, p. 333.
 SILBERMANN. *Wien. med. Presse*, 1890, xxxi, p. 332.
 WALTHER. *Zeitschr. f. Geburts. u. Gyn.*, 1893, xxvii, p. 333.

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- DELOIGNE. *Ann. Soc. de Méd. de Liège*, 1847, i, pp. 76, 95.
 DERIVAUX. *St. Louis Cour. of Med.*, 1900, xxiii, pp. 167, 200.
 FRASCANI. Di un caso de grande distensione della vescica urinaria in un aborto di cinque mesi e mezzo," Pisa, 1891.
 FREUND. *Klin. Beitr. zur Gyn.*, Bresl., 1862-5, ii, p. 240.
 GUÉNIOT, quoted by A. HERRGOTT. "Des maladies fœtales qui peuvent faire obstacle à l'accouchement," *Thèse d'Aggrég.*, Par., 1878.
 HERRGOTT. *Mém. Soc. de Méd. de Nancy*, 1882-3, p. 90.
 HOWITZ. *Hosp. Tid. Kjobenk.*, 1862, v, p. 57.
 JANNY. *Klin. Beitr. zur Gyn.*, Bresl., 1864, ii, p. 240.
 JILDEN. "Ein Fall von Geburtshindernis in Folge übermässiger Ausdehnung der kindlichen Harnblase," Würzburg, 1890.
 KOVATCHEFF. "Dystocie fœtale par rétention d'urine," Lille, 1901.
 KRISTELLER. *Monatschr. f. Geburtsk.*, 1866, xxvii, p. 165, and *Verhandl. d. Gesells. f. Geburtsk. in Berl.*, 1866-7, xix, p. 5.
 MAURICEAU. "Traité d. mal. des femmes grosses," 3rd ed., Par., 1681.
 PATOIR. *Bull. méd. du Nord*, Lille, 1881, xxi, p. 581, and *Journ. de Sages-Femmes*, Par., 1882, x, p. 43.
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 VERMEIL. *France méd.*, Par., 1881, ii, p. 482, and *Arch. de Tocol.*, Par., 1882, ix, p. 93.
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 WESTPHAL. "Ein Fall von Geburtshindernis bedingt durch die übermässig ausgedehnte kindliche Harnblase," Königsberg i./Pr., 1896.
 WOLCZYNSKY. *Wien. med. Presse*, 1882, xxxvi, p. 1135.

Other instances of distended fœtal bladder are found among the cases of imperforate urethra quoted elsewhere, but dystocia is not actually recorded, although probable in several of them.

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THE FÆTUS.

The external appearances of the fœtus are well seen in the accompanying photographs.¹ It is a male child of about six months development, between 13 in. and 14 in. in total length, and nearly 10 in. (25 cm.) from vault of skull to coccyx. It appears to consist for the most part of abdomen. The girth could not be accurately measured, but it was not



FIG. 1.

¹ (Figs. 1 and 2 from photos. by Mr. E. E. Wilson, Photographer, London Hospital.)

less than 26 in. The head and face show signs of intra-uterine pressure, but there are no abnormal developments such as hare-lip, cleft palate, or supernumerary auricles. The chest appears small in comparison with the rest of the body, but with the exception of the lower ribs, which have been spread out a little by the abdominal distension, there is



FIG. 2.

nothing amiss. The limbs are normal, but the feet are tucked up under the abdomen, crossed, and in a position of extreme talipes equino-varus; this is probably only the result of pressure. The fluid having been

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evacuated, the abdominal walls are flabby, soft, and remarkably thin. Surrounding the insertion of the umbilical cord is a translucent membranous area, oval in shape, $3\frac{1}{2}$ in. long by $2\frac{1}{2}$ in. wide, through which the umbilical vessels and contents of the abdomen could be seen (see fig. 3). It forms a window in the anterior abdominal wall. Before the abdomen was tapped this must have protruded as a large hernia. The veins of the abdominal wall are dilated, as can also be seen in the

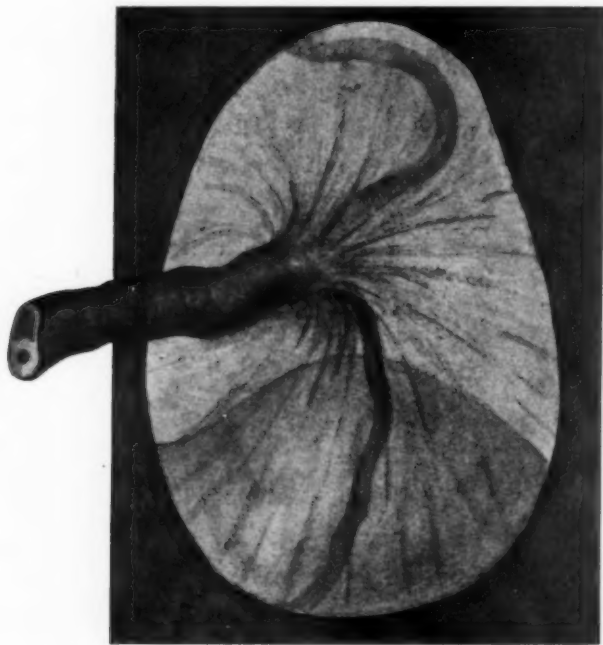


FIG. 3.

The Umbilical Ring.

Through the membrane can be seen: (a) the attachment of the bladder and its upper limit; (b) the umbilical vein; (c) the right hypogastric artery.

photograph. The penis and scrotum appear well developed; no epi- or hypo-spadias; preputial and meatal orifices not obliterated; testes undescended. There is no exstrophy of the bladder. The anus is imperforate; there is no trace of an opening, not even a dimple. No spina bifida or other external malformation. The cord was of the usual length and appearance; placenta and membranes seemed normal.

The fœtus has been added to the Museum of the Royal College of Surgeons, where no such specimen previously existed.

The result of an exhaustive dissection of the fœtus may be summed up shortly as follows: On opening the abdomen, the most noticeable feature was the urinary bladder. This was of enormous size (as can be seen in the drawing, fig. 4), and before puncture must have occupied



FIG. 4.
General view on opening the abdomen.

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most of the abdominal cavity and reached the diaphragm. An exact estimate of its capacity was impossible. The enlargement was eccentric, owing to hypertrophy of its walls in some parts and thinning and stretching in others. The wall measured nearly $\frac{1}{2}$ in. in its thickest parts. The interior was fairly smooth, but with wrinkling and puckering in parts as if by scar tissue contraction. The anterior wall was firmly

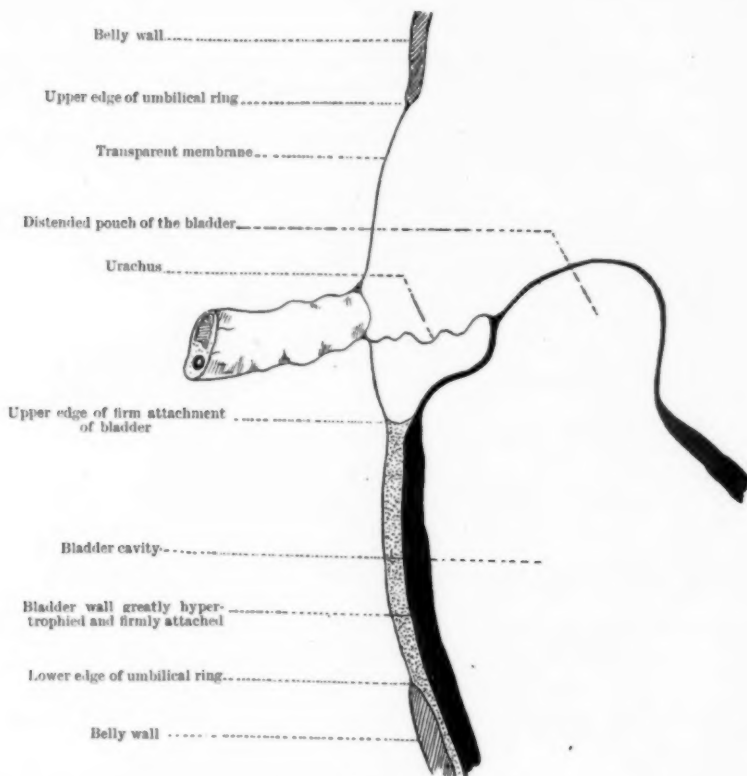


FIG. 5.

To show attachment of bladder to abdominal wall, urachus, eccentric hypertrophy of bladder, and the relation of parts at the umbilicus.

attached to the abdominal wall (fig. 5). The gut, which ended in a distended cul-de-sac, opened into the posterior wall through a pin-point opening. Both ureters were hugely dilated and thrown into tortuous coils.

The kidneys were in a state of hydronephrosis. The urethra showed, on microscopic examination, a general malformation of the spongy portion, with an obstruction in two parts. These obstructions I believe to have been complete. To investigate the point I made serial sections of the whole urethra, after failure to pass both air and water through the canal from either end. The placenta, chorion, and amnion appeared normal to the naked eye, but were unfortunately not retained, so that microscopic and other detailed examination has been impossible. This serves to remind one again of the importance of reserving the placenta, membranes and cord as part of every fœtus. The cord, on section, was found to contain one artery only and one vein (fig. 6). The vein pursued its normal course to the liver; the artery proved to be the right hypogastric artery, which was a direct extension of the right common iliac trunk. The left hypogastric artery was entirely absent (fig. 7).

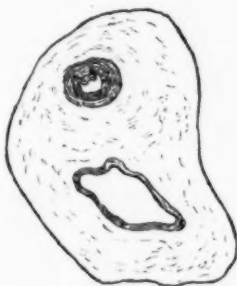


FIG. 6.

Transverse section of the umbilical cord
showing one artery and one vein.

WHAT IS THE RESULT OF IMPERFORATE URETHRA?

It seems to be assumed by writers of obstetric books and some others who have described specimens more or less like my own that the condition of dilatation and hypertrophy of the bladder, most commonly met with in such specimens, and the accumulation of a large quantity of fluid in that bladder, is the natural result of imperforate urethra in the fœtus. But is it?

Many infants have been born alive and at full term with an imperforate urethra. *In itself atresia of the urethra in no way interferes with the general growth of a fœtus*, nor does it of necessity create other

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complications *in utero* which render the life of the new-born infant impossible. It is more often found without enlargement of the bladder than with. It is an arrest of development comparable with hare-lip, cleft palate, or imperforate anus, with which, indeed, it is sometimes associated; but it may be found in an otherwise perfectly normal child. It is only when the renal function is stimulated and excessive secretion takes place that the malformation begins to assert itself and trouble ensues. Now, it has always been a matter of dispute whether the renal function is established *in utero* or not, and to this I am about to refer. Suffice it

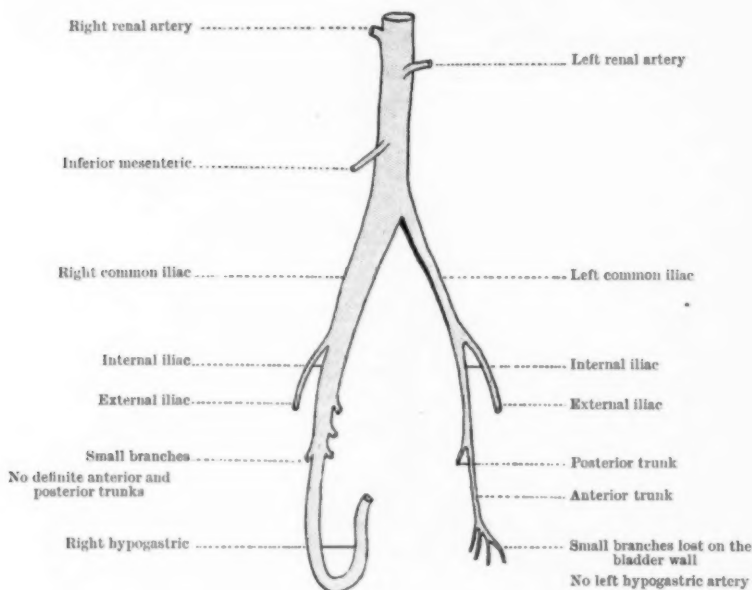


FIG. 7.

The abdominal aorta and its branches.

to say for the moment that a large number of children have been born alive with atresia of the urethra, and that apparently primary and complete, in whom no accumulation of fluid was manifest in the bladder until some hours, or even a day or two, after birth, and in whom the abnormality was not observed until absence of micturition drew attention to the condition of the genito-urinary organs. Whether or not renal secretion had taken place at all *in utero*, birth fully established the

function, and within a very short time operative procedure was rendered imperative. That this has often proved satisfactory will be seen from the records. An artificial opening has been made into the bladder by several routes (forcing a passage through the length of the penis, suprapubic puncture, perineal cystotomy), and in not a few cases the patient has not only recovered from the effects of the immediate operation but, in the terms of a popular phrase, lived happily ever after.

The following cases, chosen from many in the literature, will suffice to prove this point:—

- ADAMS. *Brit. Med. Journ.*, 1891, i, p. 221.
 ALLEN. *Med. Record*, New York, 1896, xlix, p. 801.
 BLAKE. *Boston Med. and Surg. Journ.*, 1856, lv, p. 508.
 CAMPBELL. *Brit. Med. Journ.*, 1891, i, p. 460.
 DAVIS. *Med. Record*, New York, 1896, l, p. 354.
 DUPLAY. Ashhurst, *Internat. Encyc. Surg.*, Lond., vi, p. 485.
 EDIS. *Lancet*, 1874, i, p. 893.
 FORSTER. *Brit. Med. Journ.*, 1885, i, p. 17.
 GRUBB. *Lancet*, 1874, ii, p. 857.
 JACQUART. *L'Union méd.*, Par., 1875, xix, p. 351.
 LANNEAU. *New Orleans Med. and Surg. Journ.*, 1889-90, N.S. 17, i, p. 22.
 WHITEHEAD. *Med. Times*, Lond., 1847, xvi, p. 594.

I can find no accurate observations on the mode of death in similar cases where operation was not performed, but I presume the bladder would rupture or that death would ensue from suppression of urine.

Cases of imperforate urethra therefore fall into the two classes: (1) those accompanied, and (2) those unaccompanied by excessive secretion of fluid into the bladder; and imperforate urethra becomes dangerous *in utero* only when accompanied by excessive secretion. With this condition morbid results are bound to follow, which, I take it, will differ according to the amount of fluid secreted, the rate at which it is poured into the bladder, and the date when it begins. Four possible results of such a secretion suggest themselves:—

(1) In the earliest stages, exstrophy of the bladder—a theory advanced by Andrew Duncan in 1805, and fully set forth in the *Edinburgh Medical and Surgical Journal*, 1805, i, pp. 43 and 132. This theory has been revived again lately and deserves much consideration.

(2) Urinary fistula at the umbilicus. This certainly occurs where no obstruction is evident, and indeed it is not mentioned in a single case of imperforate urethra that I can find in the literature.

(3) Distension and rupture of the bladder into the peritoneum. (See specimen in Guy's Hospital Museum, 2551/47, described by King.¹)

¹ *Guy's Hosp. Reports*, 1837, Ser. 1, ii, p. 508.

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(4) Dilatation and hypertrophy of the bladder, with other complications.

In the present paper I shall refer only to the last suggestion, namely, dilatation and hypertrophy.

Firstly, can a bladder which has no "exit," hypertrophy? In adult life hypertrophy of the bladder follows not complete, but partial obstruction of the urethra, and it is universally recognized as indicating repeated attempts by the organ to overcome a difficulty. It is a development of slow degrees. The bladder of the fœtus reacts in a precisely similar way if it is called upon to eject urine in the presence of congenital narrowness or partial obstruction of the urethra. This is exemplified in cases recorded by Macan, Walther, Seuvre, Commandeur, and others:—

COMMANDEUR. *Lyon méd.*, 1896, lxxxvii, p. 359.

MACAN. *Brit. Med. Journ.*, 1887, i, p. 513.

SEUVRE. *Bull. Soc. Anat. de Paris*, 1874, p. 174.

WALTHER. *Zeitschr. f. Geburts. u. Gyn.*, Stuttg., 1893, xxvii, p. 333.

But does it dilate and hypertrophy in the presence of atresia? Ballantyne, in his work on "Ante-natal Pathology," states that it is necessary to divide cases of enlargement of the bladder in the fœtus into two classes: (a) simple dilatation; (b) dilatation and hypertrophy.

After studying the reports of a large number of cases, however, I am convinced that although there may be a necessity for distinction, it is not possible from the description alone to classify the recorded cases according to this plan. The text is often far too indefinite. Even in the cases of simple dilatation quoted by Ballantyne himself, I cannot satisfy myself that hypertrophy can be excluded.

I may mention especially two of these cases—those of Fabris and Schwyzer—in neither of which is the fact clearly stated. Indeed, Fabris's note that the bladder walls were thick when contrasted with the thinned-out abdominal parietes is suggestive that hypertrophy may have existed. The dilatation is so striking a feature that hypertrophy is liable to escape mention. Moreover, one would expect cases of simple dilatation to end in rupture long before the bladder attained anything like the proportions given either in Fabris's or Schwyzer's case.

There is an admirable specimen of a four months' fœtus in St. Thomas's Hospital Museum (No. 2691), described by Shattock,¹ in which dilatation is the most marked feature. But Shattock says in this case:

¹ *Trans. Path. Soc. Lond.*, 1887-8, xxxix, p. 185.

"The post surface of this dilated bladder presents faint transverse ridges like those due to the muscular hypertrophy seen in most cases of ordinary vesical or urethral obstruction."

Hypertrophy does not take place in the bladders of young animals after the urethra has been completely obstructed by artificial means. Guyon¹ demonstrated this repeatedly. After tying a ligature round the urethra in dogs, for instance, he found that rupture of the bladder ensued in fifty to seventy hours. Dilatation takes place and rupture follows. Hypertrophy is usually the result of repeated contractions under difficulty. It is true that a muscle can contract without its points of origin and insertion coming nearer together, but it is difficult to think that it would persist in this for long and hypertrophy as the result, especially if it were being progressively stretched all the time by an opposing force.

It is quite as difficult to believe that a hollow viscus, distended and further distending with fluid, can persist in its efforts to contract and thereby hypertrophy to an enormous degree unless some of the fluid contained in it can escape.

It seems to me, therefore, that hypertrophy of the bladder wall is not a result that might be expected in a case of imperforate urethra. How, then, does it come about? Only, I believe, by means which entail an actual or virtual escape of the urine from the bladder, and again four possibilities occur to me to explain the cases in the literature:—

(1) Incorrect diagnosis. What is reported complete atresia of the urethra would, if serial sections had been made, have proved in reality partial obstruction only; and part of the fluid has been ejected into the liquor amnii with every contraction of the bladder.

(2) A partial or slowly increasing obstruction, which only became complete shortly before the child was born.

(3) That contractions of the bladder have produced a pouch or hernia capable of accommodating part of the fluid during contraction of the rest of the viscus.

(4) That regurgitation into the ureters has taken place, with a reciprocating action between these and the bladder during contraction and relaxation of the latter.

Secondly, is the dilatation of the bladder with copious secretion a natural result of imperforate urethra? It has long been a matter of dispute whether or not urine is secreted physiologically during foetal life, and

¹ "Maladies des vices urinaires," 1903.

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even now, although a vast amount of literature may be found on the subject, the matter is not finally settled.

Reasons which follow lead me to think that a physiological secretion does take place as a rule, but only to a very small extent, save at the very end of pregnancy. I do not believe that this is of excretory purport, but that it is a simple filtration of watery elements designed to clear the many tubules and ducts of which the urinary passages are composed. I hold that the specific function of the kidney, like that of most other organs, remains more or less in complete abeyance during fœtal life. Throughout the whole of this time (normally ten lunar months) development is slowly progressing, and although it is perfected about the seventh or eighth lunar month to a sufficient extent to enable the organs to carry on a post-natal existence of a sort should the necessity arise, they only take on true activity *in utero* as the result of some process which must be called "pathological." I suggest that the large amount of secretion in the case of the fœtus under consideration and others like it was an evidence of a pathological and not a physiological process.

(A) *Fœtal life can undoubtedly be supported and normal development of other parts of the body proceed in the entire absence of urinary organs.*

Arthur E. Giles¹ records a case of malformation of the rectum and bladder, absence of both kidneys and ureters, imperforate anus, and absence of the right hypogastric artery. The child was born alive. A similar case was described by Rissman, of Hanover,² in which the child was born alive shortly before full term; and another by Bonn.³ Straussman⁴ reports two others, one a living child at eight months, the other at "nearly full term." Mayer⁵ records the birth of a stillborn full-term male infant in which kidneys, ureters, bladder, renal arteries, and external genitals were all absent, as well as sigmoid, rectum, and anus, though the other organs were fully developed. Berry Hart⁶ records a specimen sent him by Mr. Miles of an eight months' fœtus with no external or internal genitals, no bladder, ureters, or kidneys. Absence of both kidneys is not rare in sympodial monsters, two of which are

¹ *Trans. Obstet. Soc. Lond.*, 1892, xxxiv, p. 129.

² *Centralbl. f. Gyn.*, Leipz., 1892, xxvi, p. 497.

³ See Connell, *Journ. Amer. Med. Assoc.*, Chicago, 1907, xxxvi, p. 637.

⁴ *Zeitschr. f. Geburts. u. Gyn.*, Stuttg., 1894, Ser. 1, xxviii, p. 181.

⁵ *Zeitschr. f. Physiol.*, Heidelb., 1827, ii, p. 36.

⁶ "A Contribution to the Morphology of the Human Urino-genital Tract," *Journ. Anat. and Physiol.*, xxxv, 1901, p. 330.

referred to by Shattock.¹ In both of these the bladder was normal, but there was no trace of kidneys or ureters.

If renal excretion and secretion were necessary to foetal life and development, no child could have been born alive without kidneys or reached a period of development anything like full term.

(B) *The foetal kidneys do not always secrete an appreciable quantity of fluid*, for in a child born alive with atresia of the urethra there may be no external evidence of distension of the bladder at birth. If a large quantity of urine is normally excreted during intra-uterine life (*i.e.*, a quantity anything approaching that found in my own specimen and others like it) the absence of such in these cases, provided, of course, that the occlusion is primary and complete—and there is no reason to doubt it in many cases—must be explained either by: (1) inhibition of normal excretion, which is unlikely, since these children are frequently well developed in other respects, or by (2) reabsorption of fluid from the bladder. It is difficult to prove conclusively that the latter cannot occur in the foetus; many experiments have been performed² which make it certain that absorption does not take place from the bladder in animals, and we can safely conclude that the same holds good in man and in the foetus.

If foetal life and development can be supported in the absence of the urinary system, and if foetal kidneys may be present and yet not secrete appreciably, it is obvious that the renal functions of excretion of urea and other catabolic products must be carried out, at any rate in these cases, by some other organ. As a matter of fact, it is now well recognized that throughout foetal life excretion is normally carried out, at any rate for the most part, by way of the placenta. Innumerable experiments have been performed which show the possibility of transference of various substances from foetus to mother and vice versa, and there is no doubt that all the foetal toxins can be passed by this route into the blood of the mother and by her excreted. All this depends, however, on the integrity of the combined circulation. Providing that the circulatory arrangements of the mother and child are undisturbed, it is possible for a foetus to carry on a parasite existence with a minimum exertion of its own organs, and, in the case of the genito-urinary organs, to live without them. This does not prove conclusively that under normal circumstances the foetus does not use the urinary system, if it

¹ *Trans. Path. Soc.*, xxxix, p. 10.

² "Experiments by L. Lewin and H. Goldschmidt," *Archiv. f. exper. Path. u. Pharmacol.*, Leipz., 1896, xxxviii, p. 60, and *Archiv. f. path. Anat. (Virchow)*, 1893, cxxxiv, p. 1.

has one; but taken with the following facts concerning the other systems it provides a strong argument that it uses it very little. Thus anencephalic fœtuses are frequently born alive and well developed, and, indeed, the central nervous system may be entirely absent and yet life persist. Malformations which completely discount the use of the digestive tract *in utero* do not interfere with the life and growth of the fœtus. For instance, cases of atresia of the œsophagus have not infrequently been reported, and I have myself recorded three such cases which occurred in my own experience.¹ These three children were all well developed and were born alive; in fact, they lived one, six, and nine days respectively. The liquor amnii contains so little albumin that even if it were swallowed (and there is evidence that it sometimes is) it could not sustain the life of the child.

It seems almost beyond dispute that the placenta is the most important, and probably the only channel by which nutriment reaches the fœtus. Again, while means of respiration are provided by the mother through the placenta, why should the child attempt to use its own immature organs? A fœtus is capable of using its lungs at seven months or even less, but it does not, because its mother supplies abundant oxygen. The respiratory centres remain at rest during the whole intra-uterine life, and ante-natal respiratory movements must be considered abnormal phenomena.

Doubtless the development of the lungs is more complete at nine months than at seven; but as soon as the maternal circulation is cut off or severely interfered with, stimulus is given to the infant's respiratory centres, whether it be at full term or not, *in utero* or not, and for the first time it puts its own lungs into action. In this particular case we know from experiments on animals that unless the placental circulation is actually disturbed, fœtal respiration will not be attempted.

Runge, Cohnstein, and Zuntz² experimented on the sheep, in which there is no fear of disturbing the placenta when incising the uterus. In the sheep at term they exposed to the air the head and forepart of the fœtus. Pinching, pricking the skin, tickling the pharynx and nasal mucous membrane provoked no respiration. The fœtus was then extracted bodily from the uterus and placed on the belly of the mother, without the cord being pulled on. The stimuli were continued again for several minutes. Insufflations of air into the nasal fossæ had no effect; there were only general reflex movements. The fœtus sucked

¹ *Journ. Anat. and Physiol.*, Lond., 1906, xli, p. 52, and *Lancet*, 1907, i, p. 157.

² "Fœtus," Richet, *Dict. de Physiol.*, 1904, vi, p. 543.

and bit the finger when this was introduced into its mouth and pharynx; following that, it executed spontaneous movements which altered its position; but only after the cord had been tied did the foetus attempt to respire. These experiments were repeated later by Runge,¹ with the same results. Cohnstein and Zuntz demonstrated the same results in the rabbit and, by exercising great care, in the guinea-pig. Heinricius² laid bare the muzzle of the foetus in a bitch at full term, and introduced into the nasal fossæ a mixture of water and ammonia in equal parts; it produced violent reflex movements of the face, but no respirations. He opened the mouth and put some drops of the solution into the mouth and pharynx; the animal made movements of deglutition, opened and shut its mouth, put out its tongue, but did not breathe. It was only when he disturbed the placental circulation in withdrawing the foetus from the uterus that he saw respiration commence.

A phenomenon which in some ways upholds the preceding results is seen when a foetus born in the caul makes respiratory movements and succumbs to asphyxia, though it has never left the liquor amnii or been exposed to cutaneous stimuli. Engström came to a similar conclusion, that arrest of the placental circulation provokes respiratory movements in the foetus, without the necessary addition of cutaneous stimuli. It may also be mentioned that Heinricius proved by further experiment that premature respiration could be produced without disturbing the placental circulation, by diminishing the flow of blood to the medulla of the foetus by ligature of the carotid arteries. By thus causing an insufficient supply of oxygen to the bulb, the respiratory centre was stimulated.

From all the evidence at our disposal it is beyond doubt that the all-important factor to the intra-uterine life of the child is the mother's circulation and its own. By this means respiration, nutrition, and excretion are all so perfectly provided for that its own major organs are not essential to the foetus for life and growth; in fact, it is possible for them to be dispensed with. Even its own heart is unnecessary, provided a satisfactory circulation can be borrowed from a neighbouring twin; and its lungs are never used.

It is reasonable, therefore, to conclude that under normal conditions excretion by the foetal kidney is unnecessary and improbable. If the mother is capable of carrying off the toxins efficiently, why burden the

¹ *Arch. f. Gyn.*, Berl., 1894, xlv, p. 512.

² *Zeitschr. f. Biol.*, Münch. u. Leipz., 1889, xxvi, p. 137.

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kidneys of the fœtus with work which they are less prepared to perform at, say, seven months than nine?

Now in the case of the lungs we have seen that a premature stimulus may be given to respiration by an interference with the normal circulatory arrangements *in utero*. It is easy to understand that the same may be true of excretion. The nature and degree of interference are probably somewhat different in the two cases, but I venture to contend that only as the result of a breakdown in the normal mechanism of excretion provided for through the placental circulation will the fœtal kidney take on excretory responsibility *in utero*. Should such a breakdown occur, previous filtration processes will be exaggerated and excretory efforts made. The immediate result will depend on the stage of development of the kidney and a free passage through the lower urinary tracts. Fœtal uræmia is a subject about which very little is at present known, but it is possibly not an uncommon sequel and cause of death.

Although excretion of toxins is amply provided for through the placenta, a reasonable purpose may be suggested for the filtration of small quantities of water in the washing away of epithelial debris which might be expected to accumulate in the multitude of tiny tubules developing in the kidney. The presence of such debris may account for the albumin noted in the analysis of the first water passed after birth. That the kidneys may secrete copiously during the last hour or two at any rate of intra-uterine life cannot be disputed, and is what one would expect if the previous theories are correct. The onset of labour—if not earlier circumstances, such as changes in the placenta preparatory to labour, but insufficient to stimulate respiration—brings about the beginning of a breakdown in the previous arrangements of excretion; a stimulus is given to the kidney and it starts work. Often enough perfectly normal infants micturate freely within a few minutes of birth, and in the case of breech birth not uncommonly during delivery. This fluid must be secreted *in utero*. It is not always present at birth, however, and the amount varies greatly when it is. In seventy-five cases of new-born children, Dohrn found urine in the bladder of 69 per cent. Schaller found the bladder empty in ten cases out of twenty-four. This variation in quantity may, of course, depend on whether or not the fœtus has evacuated the contents of its bladder into the amniotic cavity shortly before or during delivery. Analysis shows that the first water passed by the new-born infant is different from that which follows an hour or two later. It is watery and very little charged with extractives.

It is pale and nearly as clear as water. It has the specific gravity of 1009 to 1010, and is acid or neutral in reaction. According to the figures found in Richet's *Dictionnaire de Physiologie* ("Fœtus," p. 618), it contains 0.245 per cent. urea immediately after birth, 0.360 per cent. urea in the first twelve hours, and 0.921 per cent. urea in the following twelve hours. Albumin in small quantity seems to be frequent, and according to Virchow very nearly constant. These figures support the view that excretion of toxins is not a normal duty of the kidneys until after birth. The child's kidneys will excrete certain chemical substances given to the mother during pregnancy and parturition. Thus, methylene blue given to the mother during labour has been found in the first urine passed by the child.

L. Schaller¹ gave pregnant women phloridzin, a glucoside which produces glycosuria, and always found sugar present as the result in the urine of the new-born child. This proves the transference of chemicals between mother and child and activity of the foetal kidneys shortly before birth, but nothing more.

The bladder is found to contain urine in some human fœtuses and occasionally even at an early date. This proves renal filtration (unless it be a secretion from the bladder—see later), though it does not necessarily prove a physiological secretion or excretion, and it may have taken place only with the onset of abortion. Nagel² found, on four occasions, fœtuses of three to four months with the bladder in the form of a transparent vesicle the size of a bean, full of clear liquid. On the other hand, five or six other fœtuses of the same age had the bladder empty. Similar observations are published by other writers.

But we must not forget that there is unfortunately always an initial difficulty in accepting statistics drawn from observations on the human fœtus. We can seldom be sure that we are dealing with a physiological specimen. But for pathological circumstances, very few embryos or fœtuses would be in our hands. There is a specific cause for every abortion and for every foetal death, although in most cases we omit to trace them and treat the product as normal. A normal fœtus, provided, of course, we consider the whole fœtus (including placenta, cord, &c.), is probably much less common on our dissecting tables than we think. The very fact that a fœtus is there should lead us to suspect its apparent normality.

¹ *Centralbl. f. Gyn.*, Leipz., 1898, xxii, p. 321.

² *Arch. f. Gyn.*, Berl., 1889, xxxv, 131.

Slow pathological changes, which act through interference with the combined circulation and ultimately kill the child or precipitate abortion, probably create, meanwhile, sufficient stimulus to call the kidneys into premature use. In regard to the presence of urine in the fœtal bladder, I believe that experimental research on pregnant animals has led to somewhat similar results. The bladder occasionally contains a little secretion—usually not. Some writers have suggested that when fluid is found in the bladder it is due to the secretion of the bladder wall itself. There are several reasons to contraindicate this theory, and there is no proof at present that the fœtal bladder is provided with the necessary secreting mechanism. It was at one time commonly supposed that the liquor amnii was entirely composed of fœtal urine. That it often contains fœtal urine is probable, but that it is composed entirely, or even mostly, of this fluid can be no longer accepted. In cases of complete absence of genito-urinary organs, and in cases of imperforate urethra, liquor amnii is often found in normal quantity, and, indeed, hydramnios has been reported. Oligohydramnios is quite as rare in these as in other pregnancies.¹ Liquor amnii was certainly present in my own case of imperforate urethra.

In cases where the kidneys are stimulated into the production of a pathological excess of fluid (and I have elsewhere suggested an abnormality of the circulation as cause), and where no obstruction to the urinary passages exists, it is probable that the urine is ejected into the liquor amnii, and it may in this way take a share sometimes in producing hydramnios. If that is so we can suppose that in certain cases, my own included, well-marked hydramnios might have been present but for the atresia of the urethra.

CONCLUSIONS.

- (1) In itself, imperforate urethra is not antagonistic to fœtal life.
- (2) It is not necessarily accompanied by distension of the bladder, nor does it render dystocia inevitable.
- (3) It becomes dangerous *in utero* only in the presence of a large secretion of fluid by the fœtal kidney, and such secretion is pathological.
- (4) The fœtal kidney normally allows the filtration of a small quantity of watery fluid, but the amount is insignificant. The kidney is not employed as an *excretory* organ *in utero*, unless a

¹ See case by Ballantyne, *Edin. Med. Journ.*, 1895, xl, p. 858.

breakdown occurs in the normal mechanism by which the mother performs the whole of the necessary excretory function of her child.

(5) The kidney, like the other major organs, though capable of carrying on post-natal functions of a sort at an earlier date than the tenth lunar month, if necessity demands, remains in abeyance during foetal life.

(6) The effects of accumulation of fluid in the foetal bladder depend on the amount, the rate, and the date of secretion, and especially also on the possibility of evacuation.

(7) Hypertrophy and dilatation of the bladder can only arise where there is an actual or virtual means of exit for the contained fluid.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) said that the Section was much indebted to the author for his valuable paper, which contained a great number of facts. Until he had had time fully to consider those facts he did not feel able to give up the view he had held for many years that the foetus normally secreted urine and passed it into the liquor amnii during pregnancy. He had met with at least half a dozen cases of stenosis of the ureters in stillborn children in which the ureters were distended with urine above the obstruction; in one case the distended ureters gave rise to dystocia. In these cases urine had evidently been secreted during long periods and not merely during the act of birth, and it was much more reasonable to suppose that the obstruction interfered with the normal outflow of urine than that the obstruction, or an associated pathological condition which was not observed, was itself the cause of the secretion. When the kidneys were absent the liquor amnii had been observed to be very scanty. With regard to the statement that foetal secretion remained in abeyance till birth, it was certainly not true of the vaginal or the uterine secretion, for he had found the vagina and the body of the uterus greatly distended when the outflow of mucus was obstructed by stenosis or by tumours.

Mr. ALBAN DORAN observed that quite recently a case of oligohydramnios had been reported where both kidneys were absent in the foetus and the bladder was empty.¹ The suprarenal bodies were hypertrophied. The absence of the kidneys was held to support the theory that the liquor amnii was mainly supplied by the foetus, but Mr. Doran doubted if Dr. Spicer or any other competent obstetrician would be convinced on the evidence of a single case.

Dr. W. S. A. GRIFFITH stated that Dr. Spicer's admirable paper was one of great interest and raised points which were of great importance. The records of these cases showed that imperforate urethra was not necessarily followed by

¹ Hauch: "Oligohydramnios: Foetus sans reins," *L'Obstétrique*, August, 1908, p. 373.

distension of the fœtal bladder, and Dr. Griffith was prepared to accept Dr. Spicer's view that interference with the normal secretory apparatus of the fœtus through the placenta was the cause of the renal activity before birth, and that this activity during intra-uterine life was therefore pathological. The class of cases which he believed Dr. Spicer had not referred to in his paper was that of distension of the fœtal bladder without stenosis of the urethra, and he (Dr. Griffith) pointed out that, although the view that a fœtus could empty its bladder into the closed amniotic sac was commonly held, no one had brought forward any evidence that this was possible; and it appeared to be very unlikely, although cases of great hypertrophy, such as that reported by Dr. Spicer, in which the contracted bladder measured $\frac{1}{2}$ in. in thickness, appeared to show that under certain conditions it might be possible. From some work he did in the investigation of this subject some years ago, with the help of the lecturer on chemistry at St. Bartholomew's Hospital, in the examination of the fluid found in the bladder at birth, he was able to agree with Dr. Spicer's statement that the small amount of fluid found in the bladder at birth was little more than a watery filtration, and that the secretion was the same until the ingestion of food began to give it the ordinary urine characteristics.

Dr. SPICER, in answer to a remark by the President that hydronephrosis in a case of congenital narrowing of the ureter found in a stillborn fœtus proved physiological secretion by the fœtus, suggested that the fœtus would not have died *in utero* but for some pathological process. That process, ending as it did in death and abortion of the fœtus, was probably acting in the meantime as a stimulus to pathological secretion of urine, which happened to be retained owing to the malformation of the tract.

Two Cases of Uterine Fibroids showing Peritheliomatous Changes ; long Immunity from Recurrence after Operation.

By ALBAN H. G. DORAN, F.R.C.S., and
CUTHBERT LOCKYER, M.D.

CASE I.

Cystic uterine fibroid of large size in a 5-para, aged 49—Infection of a loculus after tapping—Fibroma of ovary—Supravaginal hysterectomy: removal of the fibromatous ovary—Opposite Fallopian tube and ovary not removed—Uterine tumour malignant (perithelioma) according to microscopic appearances—Convalescence protracted through infection, probably from the tapped loculus—Speedy recurrence expected, yet patient living and very corpulent four and a half years after operation.

Mr. DORAN thus reports the case: E. J., aged 49, was admitted into my wards in the Samaritan Free Hospital in November, 1903. She had been under the care of Mr. Boodle, of Sittingbourne, who informed me that the patient's abdomen had been enlarging for seven years. She had been tapped by another doctor five weeks before admission, and over a pint of ropy fluid had come away without much diminution in the size of the tumour. The periods had been regular until 1902, when they ceased for six months; then a slight show of blood was observed, and it reappeared about every six weeks. I may add that a distinct catamenial period occurred a few days after admission. Mr. Boodle had not detected any evidence of cardiac or pulmonary disease, and knew that the patient was of abstemious habits. The girth of the abdomen at the umbilical level a few days before admission was 54 in.

The patient had been married for thirty years and had borne five children, the last confinement occurring eighteen years before admission. There had been one abortion only, and it had happened between two of her earlier pregnancies. When aged about 18, a year before her marriage, she was laid up with a mild attack of rheumatic fever, and during convalescence there was, it appears, some kind of relapse

which her friends called low fever. She was subject to attacks of bilious vomiting. She declared to me that she had recently lost flesh.

Condition on Admission.—The patient looked fairly healthy, thin, yet hardly cachectic. Both legs were slightly œdematous. The abdomen was greatly distended, the measurements on November 5 being: girth at umbilical level, $55\frac{1}{2}$ in.; ensiform cartilage to umbilicus, $16\frac{1}{2}$ in.; umbilicus to symphysis pubis, $7\frac{1}{2}$ in.; right anterior superior iliac spine to umbilicus, $11\frac{1}{2}$ in.; left anterior superior iliac spine to umbilicus, $9\frac{1}{2}$ in. Thus the girth had distinctly increased since Mr. Boodle had recently taken the measurement. The integuments of the abdominal wall were neither glossy nor œdematous, and there were no enlarged inguino-femoral glands. There was universal dulness, excepting high up in the epigastrium, and fluctuation at every point, not transmittible from flank to flank nor from the upper to the lower limits of the tumour. There was a scar below the umbilicus, the result of the recent tapping, and the parietes were much less tense at that point than higher up, as the above measurements showed. This fact suggested loculi. The uterus could be plainly defined, bulky and anteverted by the tumour, no part of which came down below the brim of the pelvis. The temperature was normal, the pulse about 84, small volume, regular. The urine was free from any morbid deposit or albumin, the specific gravity was about 1018.

Operation.—On December 1, 1903, I removed the tumour, assisted by Mr. Butler-Smythe and Captain Illington, I.M.S. Dr. Llewellyn Powell administered gas and ether for the first half-hour and chloroform afterwards. When the peritoneal cavity was opened a little free fluid escaped, then a very vascular cyst wall was exposed. The use of the tapping trocar caused severe bleeding; I at once prolonged the abdominal incision to 4 in. above the umbilicus. The omentum was strongly adherent, with its vessels greatly dilated. An artery and vein were ligatured, the remaining vessels were clamped, and the omentum detached and tied in segments. Then I found that 4 in. of the transverse colon, including the splenic flexure, were firmly adherent to the cyst wall, a thin layer of which I detached, leaving it on the bowel, which was then set free. I was rather alarmed to find that the under surface of the transverse mesocolon, which adhered firmly to the upper part of the tumour, was torn in two places, but the lacerations were longitudinal and did not involve any of the big vessels. The fundus of the tumour could now be delivered through the abdominal wound; I broke down several big loculi to make its extraction easier—they were

full of pale lymph. One loculus burst spontaneously; it was full of foetid pus.

I now found that the ovaries and Fallopian tubes were separate from the tumour, except that they were connected with it by free soft adhesions. The tumour sprang from the uterus and from the root of the right round ligament, which bore an enormous artery. Dr. Powell and Captain Illington transfused $1\frac{1}{2}$ pints of saline fluid under each breast; in the meantime I ligatured the right ovarian vessels, the big artery in the right round ligament and some vessels in the left broad ligament. Then I amputated the uterus above the cervix, one assistant securing the uterine arteries as they were divided and the other holding the bulky and heavy tumour till it came away with the body of the uterus and the right ovary and tube. The flap of serous membrane made in front of the uterus during amputation was sewn over the raw surface of the uterine stump. Some large vessels in the right broad ligament had to be secured. I repaired the two lacerations in the transverse mesocolon, and carefully turned in the edges of the piece of cyst wall left on the colon by means of sutures. The peritoneal cavity was flushed with saline fluid, then I applied deep interrupted silkworm-gut sutures to the abdominal wound, flushed the peritoneum once more, and lastly tied the sutures, leaving several pints of saline fluid in the abdomen. The operation took up nearly two hours, but there was no evidence of severe shock when the patient was returned to bed. The parts removed weighed 15 lb.

Dr. CUTHBERT LOCKYER drew up the following report:—

Description of the Parts Removed.—The specimen consists of the uterus with its right appendages, the latter including a fibroma of the right ovary. The uterus is of the size of two fists. There is an interstitial fibroid in each of its lateral walls, its cavity is elongated and rendered convex on both sides by the bulging of the interstitial growths. The appendages have been removed flush with the surface of the uterus at the left cornu, and at this situation exists a large pedunculated fibroid outgrowth of the size of a Rugby football. From the whole width of the fundus proceeds upwards a cystic fibroid growth of the same size as the enlarged uterus itself. The right appendages are so distorted at their origin that the Fallopian tube appears to spring from the uterus several inches away from the origin of the right ovarian ligament. The right round ligament bears a small fibroid tumour of the size of a walnut. The Fallopian tube is hypertrophied, elongated and twisted.

The ovarian ligament runs into a solid fibrous tumour of the size and shape of a swan's egg.

Microscopic Appearances.—The large pedunculated tumour presents several areas of what appears to be peritheliomatous change when examined microscopically; such areas are not to be found in the other tumours. The mesoblastic changes consist in the appearance of small round-cells, which lie in close connexion with numerous blood-vessels and appear, in many instances, to arise from their outer coats. These blood-vessels have fairly thick walls and are in no sense embryonic, like the vessels within a more malignant sarcoma. The new small round-cells, however, are indistinguishable from sarcoma cells; they spread amongst the fibro-muscular tissue in columns, they take up hæmatoxylin more eagerly than muscle and fibrous tissue nuclei, and they show a tendency to aggregate in patches around blood-vessels like a round-celled inflammatory infiltration, but the cells themselves are larger than leucocytes and do not resemble them in any way. The fact, Dr. Lockyer adds, that there has been no recurrence since the operation may be accounted for partly by the limited area of the change, many sections showing no sign of the new disease, partly by the pedunculation of the tumour, and partly by the fact that peritheliomata are less malignant than the more ordinary types of sarcoma.

Mr. DORAN continues:—

Complications during Convalescence.—During the first week the patient suffered from slight mental disturbance, fancying that her nurses desired to kill her, and there was great flatulent distension on the third day. The pulse and temperature did not, however, rise very high, and the bowels were opened on the fifth day, nor was there any further trouble with them. On the eighth day an area of induration could be felt about the seat of the ligatured omentum; on the fifteenth day there was a very free discharge from the rectum of a greasy fluid mixed with old clots of a coffee-brown colour. The indurated area, which was palpable on the previous day, had disappeared completely. I suspect that one of the ligatures on the omental vessels or one of the sutures applied to the piece of cyst wall on the bowel had become infected either from the intestine or from the pus which escaped from the loculus which had been tapped. It was not until the twentieth day that the patient began to improve steadily, and I did not think it advisable to discharge her until the middle of January, 1904. The abdominal wound had by then healed perfectly, the abdomen was flat, the pulse good, and the patient no

longer irritable or suspicious. I greatly feared, however, that recurrence would speedily occur, judging from the naked-eye appearances of the tumour and the sections prepared by Dr. Lockyer.

After-History.—Eight months after the operation the patient wrote to me saying that she had quite recovered her health, though she still felt weak. Every month a little show appeared, associated with a great deal of pain in the back. During the spring of 1905 the patient complained of slimy and foetid vaginal discharge; ultimately a ligature came away and the discharge ceased. By April, 1906, she had grown very corpulent, weighing over 17 st. She was quite able to attend to her household duties. The menopause was not complete until early in 1907. I believe that, considering the patient was subject to slight mental disturbance, I did rightly in leaving one ovary, but I have discussed this subject elsewhere.¹ On April 3, 1908, the patient wrote to me stating that she continued to get stronger and was able to take fairly long walks. She was free from any pelvic symptoms. On August 5, 1908, she came to see me. She was clearly in very good health. The abdominal walls had become extremely fat and pendulous; the cicatrix of the abdominal incision was free from hernia or new growth. The cervix was small and fairly movable. There was no trace of any new growth or inflammatory deposit in the pelvis or abdomen.

CASE II.

Cystic uterine fibroid invading the left broad ligament and sigmoid mesocolon in a 2-para, aged 36: marked changes in left Fallopian tube and ovary—Deposit in parametrium, of doubtful nature—Uterus amputated above os externum, total hysterectomy appearing impracticable—Uterine tumour and growths in left tube and ovary malignant (perithelioma) according to microscopic appearances—Speedy recurrence expected, yet patient living and well two years and seven months after the operation.

The following is Mr. DORAN's report of the case:—

A. F., aged 36, was admitted into my wards in the Samaritan Free Hospital at the end of May, 1905. Fifteen months previously she had discovered a swelling in the left side of the abdomen, and recently it had grown very quickly. The patient stated that she had lost flesh. She

¹ "Subtotal Hysterectomy: After-Histories of Sixty Cases," *Lancet*, 1905, ii, p. 1310, and *Trans. Obstet. Soc.*, xlvii, p. 363.

had been under the care of Dr. Whittaker, of Elgin Avenue, Dr. F. McCann, and others.

The patient looked very healthy; she was dark-haired, rosy complexioned, fairly muscular, not stout, and in appearance might have passed for a woman aged 30. She had been married thirteen years and had borne two children, both in robust health. She had never aborted. The first labour, twelve years before admission, had been severe and the perineum was torn. The second, five years before admission, was rapid and spontaneous, but she was kept in bed for a month for some reason which was not clearly explained to her. "Torn or something," as she expressed it, seemed to imply an injury during the labour; I shall return to the point when describing the condition of the vagina and cervix. There had been no symptoms of pelvic disease since convalescence from the second puerperium. The significance of two normal pregnancies only, at a long interval, in this healthy subject was not clear. Since the first pregnancy the patient had been subject to varicose veins on the outer side of the left leg. Four years before admission, when her youngest child was 1 year old, the patient had two attacks of "constipation," relieved by enemata. There was no history of any other illness or of hæmorrhages.

Condition on Admission.—As above observed, the patient looked very healthy. The abdomen was irregularly distended by a tumour which lay more to the left of the middle line than to the right. The integuments seemed healthy, being free from œdema or glossiness. The tumour was deeply lobulated—a lobe on the right, somewhat soft, reached above the umbilicus; a still softer lobe made up the entire left portion; a firm lobe lay in the right iliac fossa; and a small, very hard body could be defined immediately above the pubes, almost in the middle line. The soft left lobe was very prominent, and on its surface towards the right was an area of resonance on percussion extending to its upper limits. The entire tumour was movable to a limited extent. The cervix was very firm in consistence and continuous with the hard lobe above the pubes. I could not find any laceration, but I detected a suspicious irregular bleeding growth on the vaginal wall in the left fornix, possibly the result of some injury during the second labour. I thought at the time that it was a malignant growth which had proceeded from the abdominal tumour and perforated the vaginal wall. After repeated examination, however, I found that when the tumour was moved there was no dragging on the left fornix. The right and posterior fornices were free. On inquiring about the catamenia,

I found that they appeared regularly at intervals of three weeks, as had been the case ever since their establishment, and were not associated with much pain. But during the last five or six months before admission they became much more free and a little show had occasionally been noticed between periods. On June 11, when the patient was under my care, a profuse show set in, at the right date. The patient said it was the most severe period that she had passed through. The urine was pale, clear, acid, of low specific gravity, and even catheter samples contained a trace of albumin. The pulse was 108, small, not quite regular; the temperature never rose over 99.4° F. I could not make out any morbid condition beyond the limits of the genital tract. The tumour seemed undoubtedly to be a fibroid, the catamenial history confirming the diagnosis, but it was not clear that it was entirely uterine, the resonant tract over the left lobe suggesting intestine pushed up by invasion of its mesentery.

I did not operate until after June 16, when the show of blood had ceased. In the meantime strychnine and ergot were administered to the patient.

The Operation.—I operated on June 17, 1905, assisted by Mr. Butler-Smythe and Dr. Ernest Travers; Mr. A. S. Morley administered the anæsthetic. The pelvis was raised high. On opening the peritoneal cavity I found the omentum strongly adherent to the surface of the big left lobe. The uterus, very bulky, formed the hard lobe which lay above the pubes. The right Fallopian tube and ovary were seen to be normal, the left tube was very long and thick, the left ovary was somewhat enlarged and closely applied to the big lobe, which had opened up not only the base of the left broad ligament but also the sigmoid mesocolon, so that the sigmoid colon ran down the surface of the big lobe towards the right, accounting for the area of resonance above described. Enucleation of the lobe, the base of which lay very deep, proved difficult. I detached the colon and its mesentery without damage to either, and tied and divided the left ovarian vessels. The whole tumour was thus set free, except from its connexions with the uterus and left ovary. The condition of the parts adjacent to the tumour, in the left iliac region, seemed highly unfavourable. There was a deposit in the left parametrium, between the cervix and the bladder, and in the left round ligament, which was very thick. I tied off the right ovarian vessels, cut through the broad ligament, secured and divided the round ligaments and then amputated the uterus above the cervix, which was fixed by the

deposit. The uterine vessels were secured as they were divided; they lay in very dense tissue and could not be isolated before ligation in the usual manner. The muscular walls of the uterus appeared very unhealthy and I could not dissect up a good peritoneal flap anteriorly. I made a V-shaped incision through the uterine walls above the cervix and sewed together the raw surfaces with No. 1 silk in two layers. The omentum, full of very big vessels, was resected close to the transverse colon, as it was unavoidably damaged when separated from its close adhesions to the tumour. There was much oozing in the left iliac fossa; the peritoneum was flushed with saline fluid, and as the pulse was failing Dr. Travers and Mr. Morley injected several ounces of saline fluid with mxxx of adrenalin into the right median basilic vein, and liquor strychninæ was administered hypodermically. The abdominal walls were closed in two layers. The tumour, with the body of the uterus, weighed $2\frac{1}{2}$ lb. During the enucleation of the big left lobe from the iliac fossa about a pint of pale yellow fluid escaped from a cystic cavity in its interior. Small solid masses, suggesting myo-sarcoma, grew from the outer surface of the lobe into the pelvic and iliac connective tissue.

Dr. CUTHBERT LOCKYER reports:—

Description of the Parts Removed.—The specimen (fig. 1) is made up of a thick-walled uterus, both appendages, and a large, adherent, partly cystic uterine growth, which has burrowed deeply into the left broad ligament. The uterus has not been removed entire, so that its cut surface presents a very broad base. Its walls vary from 1 in. to $1\frac{1}{2}$ in. in thickness; on section they show numerous small, smooth pea-like growths. Its anterior surface is roughened by recent deep red adhesions, especially marked upon the left side. The right appendages are represented by an elongated Fallopian tube, the ovarian ligament, and a cystic ovary. The right mesosalpinx and mesometrium are wanting. The left appendages are extensively altered. The Fallopian tube is 6 in. long and 1 in. in diameter, and is thickened and nodular. The left mesosalpinx is made thick by a nodular solid growth, which fuses with the large partly solid and partly cystic growth extending into the parametrium from the left side of the uterus. This growth measures 12 in. by 8 in. by 4 in. in its three diameters; its lower half is cystic and its upper part solid; the cystic portion is rough and devoid of peritoneum, whilst the upper part is covered by the expanded left broad

ligament. The left ovary is enlarged, and on section is found to contain a nodular growth similar to that which invades the tube and mesosalpinx.

Microscopic Appearances.—Sections have been prepared from the uterine wall (figs. 2 and 3), the left Fallopian tube (fig. 4), the mesosalpinx, the left ovary (fig. 5), the large tumour in the left parametrium and the left round ligament (fig. 6). They all show the respective organs



FIG. 1.

The uterus, tumour, and appendages from Case II, showing: (1) the appearance of the cystic myomatous perithelioma and its relations to the uterus; (2) the uterus amputated above the cervix, with deposits of new growth on the cut surface and in the walls, which are laid open to show the uterine cavity, and which also bears new growth; (3) the left Fallopian tube, much enlarged and elongated by new growth in its walls.

and tissues to be invaded by a perithelioma. The cells of the latter resemble in size, shape, and staining characteristics those cells which make up the lymphomatous stroma of the endometrium. This resemblance

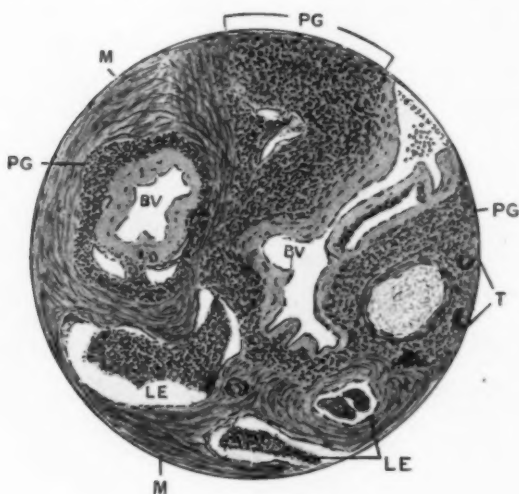


FIG. 2.

Microscopic section of the tumour from Case II. **BV**, blood-vessel; **LE**, lymphatic embolus; **PG**, peritheliomatous growth; **M**, uterine muscle; **T**, tubules from mucosa. (From drawing by Dr. Cuthbert Lockyer.)

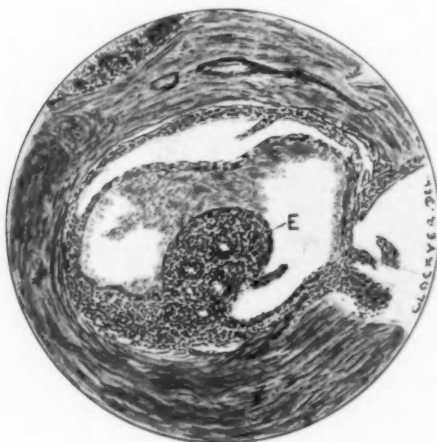


FIG. 3.

Microscopic section through an intra-uterine blood-vessel from Case II, showing peritheliomatous new growth at **E**. (From drawing by Dr. Cuthbert Lockyer.)

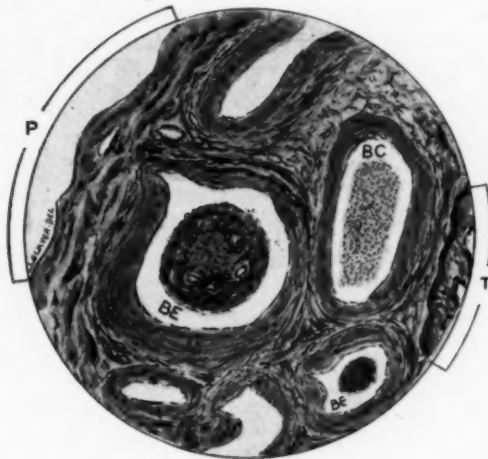


FIG. 4.

Microscopic section through the left Fallopian tube from Case II. **BE**, embolus in blood-vessel or lymphatic; **T**, tubal mucosa; **P**, tubal peritoneum. **BC**, blood-clot. (From drawing by Dr. Cuthbert Lockyer.)

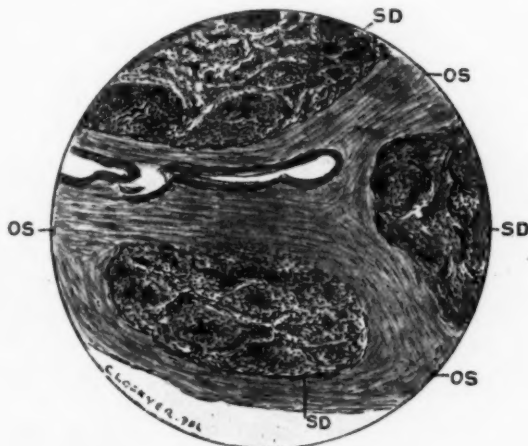


FIG. 5.

Microscopic section through the left ovary from Case II. **SD**, deposit in ovary; **OS**, ovarian stroma. (From drawing by Dr. Cuthbert Lockyer.)

is brought into prominence in the sections of the uterus to which hypertrophied endometrium is attached, and here the new growth fuses with the endometrium as if arising from it. The cells, however, are seen to spring from the adventitia of the blood-vessels; they form large oval collections, and lie in alveolar spaces amongst the muscle of the uterus and tube. The ovarian deposit consists of a uniform mass made up of oval clusters of these cells with a small amount of connective tissue binding the clusters together. The same may be said of the structure of the large solid uterine tumour in the left parametrium. In the mesosalpinx are seen large vessels with their lumina plugged by emboli



FIG. 6.

Microscopic section through the left round ligament, from Case II, enlarged to twice the natural size, as seen through transmitted light. The shaded areas represent new growth. (From drawing by Dr. Cuthbert Lockyer.)

consisting of these small lymphoid-looking cells, but wherever distributed their close connexion with the *outer* coats of the numerous blood-vessels is very apparent.

Mr. DORAN continues:—

Complications during Convalescence.—In this, as in the former case, there was not that speedy uncomplicated convalescence so usual after hysterectomy for fibroid. When the operation was concluded, I did not

expect that the patient would have survived for many hours, as I was under the impression that malignant material had been unavoidably left behind in the parametrium. We all know that when malignant tissue, more or less wounded by the knife and by ligatures, is left within the peritoneal cavity pernicious oozing is liable to occur, and speedy advance of the disease is certain. When, most unexpectedly, the patient not only recovered but was restored to her former good health I never expected that her respite would be of long duration. The microscopic appearances so plainly seen in the sections, which Dr. Lockyer prepared with great care, indicated malignancy most distinctly. Yet the after-history has, up to the present, belied our gloomy prognosis. On the night after the operation reaction was marked, the patient being slightly delirious. On the afternoon of July 18 an attack of dyspnoea occurred, lasting for forty minutes; the cause of this alarming symptom was not clear. The patient was soon able to take food by the mouth; on July 19 the bowels acted spontaneously; in the night there was slight delirium. On July 20 much solid motion came away after an olive oil enema. The patient has been constipated for months, and, although she had been carefully prepared for a fortnight before the operation, some old scybala had remained until the oil brought them away. The abdominal wound healed well. On July 23 the evening temperature rose to 103° F., and reached that height every evening until July 29. This alarming symptom was associated with foetid stools, followed by foetid discharge independent of the motions. The varicose veins in the left leg became inflamed on July 23. This troublesome symptom soon subsided, but I felt that the patient's condition was very precarious and did not allow her to leave the hospital until August 10, 1905. The formidable symptoms were probably due to parametritic inflammation, easily explained; yet it is strange that no suppuration occurred, nor did any characteristic infiltrations in the groin and above the vaginal fornices develop. I ascribed them at the time to a much more serious condition, and never expected to hear satisfactory news of the patient after her discharge from the Samaritan Hospital.

After-History.—To my great surprise the patient came to see me at the hospital on November 2, 1905. She had walked over two miles in the rain, yet was not tired and looked as well as she did before the operation. She complained of attacks of hot flushings and trembling about three times a week, and the periods had never been seen since the operation. On November 13 I examined the patient. The abdominal wound was well healed and there was no deposit to be felt in the parietes

nor any tumour to be defined behind them. The cervix was fixed, there was no trace of the bleeding growth on the vaginal mucosa in the left fornix noted before the operation, but simply a distinct thickening of the vagina bearing two small fleshy wattles. Thus, at least, there had been no extension of the suspected malignant disease into the vagina. I prescribed ovarian tabloids to relieve the menopause symptoms. About a year later I was informed that this patient had died, shortly after the visit to the hospital in November, from septic pneumonia following inflammation of the right saphena. This report seemed only what might have been expected, although I remembered that the post-operative phlebitis was in the left lower extremity. No doubt, I thought, the new growth had become active and obstructed the veins in the right leg.

I continued to watch Case I and to be somewhat surprised at the patient's immunity from recurrence. Yet, after all, I had probably removed all the new growth at the operation. In Case II it appeared clear that my operation was incomplete. It seemed strange that the patient ever left the hospital alive, but I was not in the least astonished to hear of her death within seven months after the operation. Early last year I intended to publish a parallel between the two cases which would have been most interesting and instructive were it not for the all-important fact that it would have been radically incorrect and misleading, for the second patient was all the time alive and well. Fortunately I delayed preparing my remarkable parallel!

On February 12, 1908, a patient came to see me at the hospital. She complained of dyspepsia, due, as far as I could make out, to excessive tea-drinking, but added that she had gone on wonderfully well since her operation. Then, on looking into my notebook, I found she was Case II, and there was the report of her death. On further inquiry it transpired that I had received a false report referring to some other patient who had never been under my care. The patient looked very healthy, just as she appeared before the operation, and had distinctly gained flesh. There had been no period since the operation, and the menopause symptoms had passed away soon after her last visit in 1905. On examination I found the abdominal cicatrix perfectly healthy and the parietes supple. There was no evidence of the existence of any abdominal tumour. The cervix had become very small and was still fixed; the two fleshy wattles in the left fornix had almost disappeared; there was a little resistance above the left fornix just definable on bimanual palpation; the right and posterior fornices were free. Such

was the patient's condition two years and seven months after the operation. In the whole of my operative experience I never came across a case where the after-history so thoroughly belied operative and pathological evidence.

These cases show the imperative necessity for careful observation of all patients from whom uterine fibroids have been removed. Only by such observation can we expect to gain anything like accurate knowledge as to the effects on the patient's organism of the removal of the fibroid uterus with or without cervical or ovarian tissue, so that we may see how far clinical experience agrees with the physiological experience of Blair Bell, Bond, Carmichael and Marshall, and others, and only by such observation can we expect to calculate with something like precision the chances of malignant change being overlooked or misinterpreted.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) thanked the authors for their communication of two rare and interesting cases. The specimens seemed to be correctly described as perithelioma, of which he had not previously seen an example. A considerable part of the body of the uterus, in addition to the cervix, appeared to have been left behind in one of the cases, and it was remarkable that no recurrence had taken place; the malignancy of perithelioma would appear to be slight. He was interested to hear from Mr. Doran that besides these two malignant cases he had met with one case of cancer of the cervix after 110 cases of supravaginal amputation for fibroids.

Mr. ALBAN DORAN, in reply, admitted that panhysterectomy might have been practised in the first case, but that any radical operation was quite out of the question in the second. In over 100 cases in his own practice to the end of 1906 cancer of the cervix had afterwards developed in only one patient.

**Carcinoma of the Body of the Uterus: Vaginal Hysterectomy;
patient free from Recurrence fifteen years afterwards.**

By ARTHUR H. N. LEWERS, M.D.

THE patient, from whom the specimen shown was removed, was at the time of the operation aged 53. She had been married twenty-one years; she had had no children, but two miscarriages, the last thirteen years previously. I saw her in consultation with Dr. Godson. The symptoms had been continuous bleeding for several months, with occasional more profuse losses of blood, and latterly severe pain in the hypogastric region. On bimanual examination the uterus was considerably enlarged, and the vaginal portion of the cervix was normal. The use of the sound occasioned free bleeding. The cervix was dilated with laminaria tents and Hegar's dilators. There was a hard, irregular condition of the inside of the body of the uterus, but nothing could be detached either with the finger or a curette.

Vaginal hysterectomy was performed on February 1, 1893, with the assistance of Dr. Godson and Dr. Sequeira, the latter of whom was at that time Resident Accoucheur at the London Hospital. The patient made a good recovery. A portion of the body of the uterus was sent to the Clinical Research Association for examination, and Mr. Targett's report on it was as follows: "The growth of the uterus is a columnar-celled carcinoma of the body of the villous type. It has deeply invaded the muscular substance." I saw this patient at my house on June 8, 1900, more than seven years after the operation, and she was then quite well. This year (1908) she wrote to Dr. Godson reporting herself to be well, and he very kindly sent on the letter (dated January 31, 1908) to me. It will be seen that it is fifteen years since the operation, and she remains free from recurrence.

At the present time I am inclined to prefer abdominal hysterectomy by Wertheim's method to vaginal hysterectomy, even in cases of carcinoma of the body of the uterus, unless the patient's age or general condition contraindicates the more severe operation.

(The section of the growth reported on by Mr. Targett was shown at the meeting.)

DISCUSSION.

Dr. GODSON felt that he was himself also to be congratulated on this very happy result for having selected Dr. Lewers to perform the operation for him, instead of doing it himself as had been requested of him. Dr. Lewers had already at that time achieved a well-earned reputation for his skill and success in this particular operation, while he (Dr. Godson) recognized his own inexperience, having but once performed it; so, in the interest of the patient, he became the assistant instead of the operator. She had been under his observation since July, 1892, when he saw her in consultation with Mr. Stott, for almost constant loss of blood, which had been going on, more or less, for upwards of two years, latterly alternating with gushes of purulent discharge, preceded by severe pain; so that the symptoms of the disease had existed for nearly, if not quite, three years before the operation. Under these circumstances it seemed remarkable that there had been no recurrence. Fifteen years was a long time to be free from such, but by no means the longest known to Dr. Godson. On May 21, 1883, he removed with the wire ecraseur a cauliflower growth of the portio vaginalis, along with about $\frac{1}{2}$ in. to $\frac{3}{4}$ in. of healthy tissue above it, from a woman aged 37; the uterus was fixed at the time of the operation by hardness in the left lateral fornix, which was thought to be due to malignant infiltration. It, however, cleared up entirely after the operation, and the patient made a complete recovery; and she came to see him on May 21 last in perfect health to report herself, on the twenty-fifth anniversary of the operation. Sir John Williams had what was removed, and described it in his Harveian Lectures as "a true cauliflower excrescence, which proved microscopically to be a squamous epithelioma growing apparently from the surface covered by transitional epithelium."

The PRESIDENT (Dr. Herbert Spencer) said that the specimen exhibited showed unmistakably under the microscope that it was malignant, and it was satisfactory to hear that the patient was well after fifteen years. Cancer of the body of the uterus was, however, much less malignant than cancer of the cervix, and it would be interesting if sections of Dr. Godson's case of cervical cancer, which had remained well for twenty-five years, could be shown before the Society.

A Fibroid Tumour of the Ovary.

By MARY SCHARLIEB, M.S.

A. H., MARRIED, aged 31, complained of pain in the abdomen about the level of the umbilicus; duration eighteen months, frequently accompanied by vomiting; relieved by lying down.

Examination.—Abdomen normal. *Per vaginam* there is a hard, smooth, globular swelling in the left posterior fornix, and the fundus

42 Scharlieb: *Broad Ligament Cyst with Twisted Pedicle*

uteri is pushed a little to the right. The tumour was thought to be a fibro-myoma growing from the left border of the uterus, between the layers of the broad ligament.

Operation (June 16, 1908).—At the time of operation a hard, nodular swelling, which had no peritoneal covering, presented on the posterior aspect of the left broad ligament. The left infundibulo-pelvic ligament and ovarian vessels were ligatured and divided. The broad ligament was divided below the tumour, which was then removed.

Pathological.—The specimen consisted of the left ovary, tube, and tumour. The tumour was an irregular, hard, more or less spherical mass; the anterior surface was lobulated and covered with hard round masses about the size of a pea. It was connected to a normal-looking ovary by a pedicle 1.25 cm. in length. The tube was slightly thickened in its outer third, and the lumen was patent. On microscopic examination the tumour was found to have the structure of a simple fibroma. The pedicle consisted of connective tissue containing strands of unstriated muscle fibres and thick-walled vessels. There was no ovarian tissue.

A Broad Ligament Cyst with Twisted Pedicle.

By MARY SCHARLIEB, M.S.

R. S., MARRIED, aged 34, two children. Abdominal pain for three weeks, period being then three weeks overdue; during the last four days before admission patient vomited, bowels did not act, and the pain was much worse.

Examination.—On admission she looked ill, with a furred tongue; mitral, systolic, and presystolic murmurs were present; the abdomen moved well and was resonant; *per vaginam* there was a mass in the posterior fornix, firm, elastic, rounded, and movable, lying low in the pelvis. It was thought that it might possibly be an ectopic gestation or some other distended condition of the Fallopian tube.

Operation (May 29, 1908).—At the time of operation a cyst was found behind the uterus, the size of a large orange, lying between the folds of the left broad ligament. The pedicle, which consisted of the Fallopian tube and broad ligament, was twisted twice anti-clockwise; it was untwisted and ligatured and the tumour removed.

Pathological.—Parts removed consisted of the left broad ligament cyst with the left tube and ovary. The cyst was irregularly globular,

tense, translucent, and thin-walled. It lay entirely between the layers of the left mesosalpinx. The tube was stretched over the surface and measured 5 in.; its lumen was patent, and the fimbriated end was adherent in part to the cyst wall. The ovarian fimbria was stretched out and adherent for 2 in. The parovarium was distinctly seen stretched out over the posterior surface of the cyst. The ovary was adherent to the inferior pole of the cyst, it was normal in size and contained one or two small cysts.

I have brought this specimen before you to-night because it is somewhat unusual to find a cyst between the layers of the broad ligament which not only has a distinct pedicle, but of which the pedicle is long enough to be twisted twice round.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) pointed out the confusion which arose when tumours were said to rotate "clockwise" and "anti-clockwise," without any reference to the point of view from which the rotation was regarded.

Dr. LEWERS said, with reference to the method commonly employed of describing the twist of the pedicle of an ovarian tumour as being either in the same direction as that of the hands of a watch or in the contrary direction, it seemed to him to be a satisfactory one. All that was necessary was to imagine a watch placed face upwards, and lying in the plane of the pelvic brim, the patient lying, of course, on her back. Then, if the ovarian tumour were held up by the operator at right angles to the pelvis, the twist could be accurately described as being in the direction of the hands of the watch, or in the contrary one.

Adeno-carcinoma of the Fundus Uteri.

By MARY SCHARLIEB, M.S.

E. J. R., AGED 58; first seen November 15, 1907; nullipara; menopause when aged 51, with severe floodings; three months' history; slight attacks of diarrhoea and blood-stained vaginal discharge.

Examination.—Fragment of growth found loose in vagina. Microscopic report: "This growth is a glandular carcinoma with a very reticular or alveolar structure which probably arose in the endometrium."

Operation (November 22, 1907).—Panhysterectomy and right ovariectomy. Pathological report: "Uterus contains small interstitial fibroids, and from the mucosa there has arisen an adenoma malignum."

Patient remained very well until April, 1908, when typhoid symptoms supervened. In my absence she was seen by Mr. Bland-Sutton, who found her in a practically moribund condition without any sign of recurrence *per vaginam* or *per abdomen*; he could not discover anything to account for her state. She died a few days afterwards. There was no post-mortem.

I have brought the case before the notice of the Section partly on account of the interesting fact of the association of malignant disease with fibroids and partly on account of the mode of death, some six months after the operation. In a few other cases I have known women with malignant disease of the uterus to die from some form of enteritis, which in the case of two patients dying in the Royal Free Hospital proved to be due to simple ulceration of the duodenum, but in two private cases no post-mortem was obtained. I should like to ask the members of the Section whether in their experience there is any real connexion between malignant disease of the uterus and ulceration of the duodenum.

Report of Pathology Committee.—"The Pathology Committee has examined the specimen shown by Mrs. Scharlieb, and is of opinion that the growth is one of adeno-carcinoma, parts of which are alveolar in character and other parts tubular."

Dr. T. W. EDEN showed a specimen of *simple villous tumour of the endometrium in association with carcinoma of the ovary*. This was referred to the Pathology Committee.

Obstetrical and Gynæcological Section.

November 12, 1908.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

An Analysis of a Second Hundred Cases of Operation for Fibro-myomata Uteri, with Special Reference to their Degenerations and Local Complications.

By MARY SCHARLIEB, M.S.

IN October, 1902, I reported 100 consecutive cases of operation for fibro-myomata uteri, with special reference to their degenerations. The paper was published in the *Journal of Obstetrics and Gynæcology of the British Empire*. Between October 18, 1902, and October 23, 1908, another 100 cases of "fibroids" have been operated on by me, at the Royal Free Hospital and in private. I now venture to report these cases to you, premising that, far from being in any way selected, they are simply consecutive operations reported just as they occurred. In order to facilitate comparison, I have followed the same plan and the same classification as in the report of the first hundred, and the same as that adopted by the late Dr. Cullingworth in his paper published in the *Journal of Obstetrics and Gynæcology*, January, 1902. Out of the 100 cases two patients died, viz., Cases 140 and 144.

DEATHS.

Case 140.—A. H., aged 49, single. Operation March 17, 1905. Her symptoms had been severe uterine hæmorrhage for three years and so great a sense of illness that she was incapacitated from earning her living as a nurse. At the time of operation small multiple fibroids

were found, also double hæmatosalpinx with numerous adhesions. There was, in addition, an inflamed and kinked appendix. The uterus and appendages were removed; supravaginal hysterectomy was done. Adhesions were broken down. The vermiform appendix was also removed—it was not suppurating, only kinked and adherent. The patient did not do well from the beginning, but without marked symptoms. She died somewhat suddenly on the fourth day from syncope. At the post-mortem the operation area was satisfactory, but she had a flabby, thin-walled heart weighing 9 oz.; there were old pleural adhesions, old perihepatitis, old peripleuritis, and there was some œdema of the bases of the lungs.

Case 144.—Mrs M. A. H., aged 46; one child. Operation July 24, 1905, for very profuse menorrhagia lasting three years. On admission to the hospital she was found to be anæmic, her legs were œdematous, she had a systolic aortic murmur with rigid and thickened arteries. The urine had a specific gravity of 1010 and was free from albumin; she had some incontinence of urine. On examination, a smooth, rounded mass, the size of a Jaffa orange, was found filling the vagina. Pan-hysterectomy was done. The parts removed consisted of the entire uterus containing several fibroids, two of which were calcified. From the endometrium, both of body and cervix, grew several glandular polypi; the left ovary contained two small cysts. The patient developed pneumonia of the right base on the third day after operation; septic peritonitis supervened and, although the wound was drained both through the vaginal vault and through the abdomen, she died of sepsis on the fifth day.

DEGENERATIONS OR DEPARTURES FROM HEALTH OF FIBROIDS.

Edematous and myxomatous	4 cases
Sarcomatous	1 "
Cystic or fibro-cystic	3 "
Calcareous	4 "
Necrotic { (a) Necrobiotic	6 "
(b) Infected	1 "
Sloughing	3 "
Adenomatous	1 "
					—
					23 "

Degenerations were found in nearly a quarter of the cases, and of these eighteen were such as to directly threaten life without any reference to hæmorrhage or pressure effects. In some cases more than one degeneration had occurred, *e.g.*, in Case 104 the tumour contained large cysts and was also sarcomatous.

A few of the most interesting cases may be briefly reported:—

SARCOMATOUS DEGENERATION.

Case 104.—Mrs. B., aged 50; six children, six miscarriages. Operation November 28, 1902. There was a large tumour in the abdomen which had existed for eleven years. During the last few years there had been much flooding and suffering from pressure on the bladder. Latterly the tumour had caused such oppression of the heart and lungs that the patient had scarcely been able to move about and was afraid to eat; she suffered much from cough. On examination, there was an elongated oval abdominal tumour rising $2\frac{1}{2}$ in. above the umbilicus; it was firm in consistence and was diagnosed as a fibroid. Abdominal supravaginal hysterectomy was done; the ovaries and tubes were removed. The patient made an excellent recovery and remained in good health until August, 1908, when she apparently developed appendicitis and is said to have died of that disease in Jersey about the middle of September. On opening the uterine cavity at the time of operation its vertical axis was found to be lying almost horizontally, the fundus directed towards the left sacro-iliac synchondrosis, its long axis being $3\frac{1}{2}$ in. Into the uppermost part of the cavity as it lay on its side projected a rounded mass the size of half an orange; this was the lower pole of an oval tumour the size of a six months' pregnancy, which appeared to arise from the neighbourhood of the right Fallopian tube at its junction with the body of the uterus. This tumour contained 3 pints of clear, reddish brown, spontaneously coagulable fluid. The cyst was surrounded by stretched uterine muscle and was lined with a smooth shining membrane; it contained in its walls congeries of other cysts varying in size from that of a hen's egg to that of a damson. After the fluid had been evacuated the mass weighed 44 oz. Both arteries and veins were much enlarged, the ovarian vein having a diameter of $\frac{3}{8}$ in. Parts of this growth were thought to be an example of perithelioma, but on submission to the Pathology Committee of the Obstetrical Society of London were pronounced sarcomatous.

CYSTIC.

Case 134.—D. L., single, aged 52. Operation January 3, 1905. Had been aware of an abdominal tumour for sixteen years, which had latterly increased more rapidly and had caused so much pain that her

doctor had administered morphia. The catamenia were regular and profuse, each lasting about a fortnight; she had imperfect control over her bladder. On examination, a large, irregular tumour was found rising out of the pelvis and reaching up to the ribs; there was tenderness on palpation in the right iliac fossa. Panhysterectomy was performed, and at the time of operation a large supravaginal mass needed extraction from the pelvis, and numerous adhesions between the coils of intestine and the posterior surface of the uterus were separated. While separating these adhesions, one of the fibroids which was necrosing ruptured, and grumous material, resembling that found in dermoid cysts, escaped. Some peritoneum was unavoidably stripped off the surface of the rectum, and there was oozing from the extensive raw surface in the pelvis; this was checked by temporary packing with iodoform gauze soaked in a solution of adrenalin.

The pathological report was as follows: The uterine mass weighed 9 lb. 4 oz., measured 18 in. long by 18 in. broad in its greatest diameters. It contained a large interstitial fibroid which had undergone cystic and necrotic degeneration. It showed a central cavity the size of a coconut containing grumous fluid.

The patient made a perfect recovery and has remained in good health.

NECROTIC.

(a) *Necrobiotic.*

Case 133.—M. C., single, aged 56; knew of the existence of a tumour for ten years. It grew slowly at first, but rapidly during the last few months. Although the menopause was long past she had discharge of blood *per vaginam*; her bowels were regular, but she had frequency of micturition. On examination, a large solid tumour was found filling the abdomen up to the ensiform cartilage and dipping into the pelvic cavity. At the time of operation (December 14, 1904) the myoma screw was introduced, but the substance of the tumour was too soft to afford any hold, and with some difficulty it was delivered by the hand. The parts removed consisted of the entire uterus, ovaries, and tubes. The uterus contained a tumour the naked-eye appearance of which resembled brain substance; it completely filled the uterus, which was stretched out over it like a shell, but it had not perforated the musculature at any point. The patient made a good recovery and was heard of as perfectly well in August, 1906.

Case 142.—Mrs. S. K., aged 39; two children. Complained of falling of the womb, and stated that she had amenorrhœa for three months, but a slight show was observed during the last three weeks. On examination, patient was found to be pregnant about three and a half months. The uterus was retroverted and right lateroverted; behind it and towards the right there was a smooth elastic swelling the size of a large hen's egg with a fleshy band about $\frac{3}{4}$ in. in length extending from it to the right cornu of the uterus. It was thought that this mass might be an unruptured tubal gestation, a tubal mole, or possibly an ovarian cyst. At the time of operation (March 28, 1905) a sub-peritoneal fibroid, the size of a large hen's egg, was found projecting from the posterior wall of the fundus uteri; its free end was bound by adhesions to the sigmoid flexure; the adnexa were healthy, and the left ovary contained a corpus luteum of pregnancy. The adhesions to the sigmoid flexure were separated and the tumour removed. The oozing from the uterine wall was stopped by buried sutures, and the peritoneal edges of the soft uterus were drawn together. The patient made an uneventful recovery and pregnancy went to term.

Pathological report: The specimen consisted of a pedunculated and adherent sub-peritoneal fibroid the size and shape of a large hen's egg; it was doughy in consistence and was undergoing necrobiotic change. It consisted of a capsule $\frac{1}{4}$ in. thick of fibro-muscular tissue, the interior being soft and cheesy in consistence.

(b) *Infected.*

Case 147.—Mrs. M. J., aged 50; no pregnancy. Operation September 28, 1905. Menopause when aged 45. She had an attack of retention of urine in 1903, with severe griping pain, which confined her to bed for five weeks; during this attack she first noticed a lump in her abdomen. In January, 1905, she had a flooding and lost about 2 pints of blood, also a clot the size of an orange; from this time she had an abundant yellow discharge. On examination, an irregular tumour could be felt rising out of the pelvis nearly up to the level of the umbilicus. *Per vaginam* the os uteri was open to the size of a florin and a sub-mucous fibroid was felt pressing into it. Behind this the lower part of the pelvis was entirely blocked by a round hard mass which filled the lower uterine segment and cervical canal. At the time of operation the omentum was found to be adherent to the abdominal wall, the bladder, and the pelvic viscera; it was divided and ligatured in sections. The uterus itself was not adherent, but both ovaries and tubes were buried in

adhesions in the pouch of Douglas; they were freed and the whole mass removed, but the lower portion of the fibroid was so firmly wedged in the pelvis that it had first to be bisected. The uterine cavity contained offensive purulent discharge, which was carefully mopped out and iodoform gauze was packed in. Gloves and instruments were changed and the operation easily completed. Curious to say, there was primary union and uninterrupted recovery.

Pathological report: The parts removed consisted of the entire uterus containing fibroids, also the right ovary and tube. The uterus contained a quantity of pus, also a submucous fibroid the size of a large foetal head. The fibroid distended the lower uterine segment and the cervical canal, the external os being the size of a florin. The right ovary and tube were covered by tags of adhesion, and the ovary contained a cyst the size of a pigeon's egg in which were papillomatous growths.

SLoughING.

Case 106.—Mrs. A. H., aged 46; four children. Admitted December 26, 1902. For one year she had had profuse and long-continued menstruation; for six months the interval had been reduced to between two and three weeks, and the loss had been so severe that the patient was incapacitated. On December 25, after pains resembling those of labour, she expelled a mass about the size of a full-term placenta from the vagina.

On admission, patient's face was blanched and the pulse was rapid. As hæmorrhage continued, the uterus was cleared by the house surgeon and some portion of a disintegrating fibroid was removed piecemeal. The finger in the uterus distinguished two cavities: posteriorly one the size of a Tangerine orange with rough walls, and anteriorly the smooth cavity of the uterus. The patient did fairly well, but had sufficient septic symptoms to call for hysterectomy on January 6, 1903. The operation presented nothing of interest. The parts removed consisted of the uterus and both appendages; the uterus was the size of a three months' pregnancy with the rough-walled irregular cavity before mentioned. Some superficial infection of the abdominal wall occurred, but the patient made a good recovery.

Case 123.—Mrs. M. F., aged 40; eight children. Patient was confined on October 3, 1903. The confinement was normal, but after the birth of the child the uterus remained large, and a mass the size of a water-melon was felt towards the right side of the abdomen, apparently

not separable from the fundus uteri. Patient had attacks of abdominal pain, but the lochia were normal and she had no pyrexia. She got up on the tenth day and was told to go to hospital, which she did not do. On December 1 she was seized with violent pain in the abdomen and hæmorrhage, which on the following day amounted to a flooding. On December 5 she came to the out-patient department complaining of pain and tenderness in the lower abdomen; pulse 94, temperature 99·2° F. On vaginal examination a mass the size of a foetal head was found protruding from the cervix and blocking the vagina, the lower part being just within the orifice of the vulva; the tumour was very soft. Patient was urged to come in immediately, but refused; finally, a week later, as she was getting worse, she consented. She now looked ill, had bronchitis, temperature 100·4° F., pulse 125; the hypogastrium was tender and the fundus uteri could be felt above the pubes. *Per vaginam* there was found a dark green, sloughing, and very offensive mass partly protruding through the vulva; the os uteri was fully dilated and admitted of easy examination. The mass was attached to the upper part of the uterine cavity by a base about 2 in. in diameter. The next day this mass was removed *per vaginam*; the uterus contracted. The tumour removed was the size of a full-term placenta, very soft and offensive; dark green below, reddish yellow above. Microscopic examination of the least altered portion showed it to be a sloughing fibroid. The patient's condition continued to be serious, pulse 120, temperature 101·4° F., and on December 15 panhysterectomy was performed.

Pathological report: The part removed consisted of the uterus, which was the size of a two months' pregnancy; it was globular, mottled, and doughy in consistence. On opening it there was an area on the posterior wall of the uterus near the fundus raised about $\frac{3}{4}$ in. above the general surface; it was yellowish green in colour, diffuent, soft, and exuding pus.

The patient had a long and tedious convalescence, and the abdominal wound suppurated; she, however, eventually made a perfectly good recovery.

Case 185.—B. G., single, aged 42. Patient was first seen October 1, 1907; she complained that in July she had had a flooding and again in September; she had also had a profuse brown, watery, and very offensive discharge for six weeks. She had latterly suffered very severe pain in the pelvic region, and had suddenly become thin and weak. On examination, patient looked thin and worn; nothing abnormal was felt *per abdomen*, but on vaginal examination under anæsthesia the uterus

was found to be enlarged to the size of a two months' pregnancy. The os uteri was open to about the size of a shilling, and in it was presenting the tip of a gangrenous fibroid; the finger passed freely all round the growth, which sprang from the upper part of the uterus by a broad base; the vagina was so extremely narrow and rigid that it did not appear possible to remove the growth by that route. The operation, pan-hysterectomy (October 4, 1907), presented no features of importance. The specimen consisted of the uterus, from the fundus of which an oedematous fibroid was growing; it had a base about 2 in. in diameter; the lower part of the growth had been constricted by the os uteri internum and was gangrenous. Patient went home quite well five weeks after operation.

MYXOMATOUS DEGENERATION.

Case 178.—Mrs. L. D., aged 26, married three years; no pregnancy. The periods were regular, or nearly so; there was no pain, but there was a steady enlargement of the abdomen without any of the usual signs and symptoms of pregnancy. The abdomen was distended far beyond the ordinary size of pregnancy, in fact it looked like a case of hydramnios, but, as before remarked, there were no signs or symptoms of pregnancy. On opening the abdominal cavity it was thought that a large retro-peritoneal cyst was recognized, for the surface of the tumour was white and covered with a veil of peritoneum in which were many small vessels. An attempt was made to pass the hand round the tumour, but at each side its capsule was continuous with the parietal peritoneum; the capsule was incised, but it was impossible to shell out the tumour. The incision through the abdominal wall was enlarged until it finally reached to the ensiform cartilage, but still the hand could not pass in over the upper end of the tumour; a trocar was plunged into the most prominent part of the tumour, but it had evidently penetrated a soft solid and no fluid was obtained. An incision made into the tumour revealed a pale, oedematous, apparently musculo-fibrous structure; five layers of capsule were cut into and stripped back without setting free the tumour in the least; it was useless to try the myoma screw, for the tumour would not have held it. Eventually, after great labour and considerable loss of blood, the tumour was delivered upper pole foremost; it was partly dragged out and partly squeezed out by pressure applied in the flanks. When the tumour was delivered it was found that the intestines were not adherent to it, that the ureters hung in loops along its sides, and that the lymphatics of the broad ligaments on both sides were so

distended as to resemble half-pound bunches of white grapes. The ovaries and tubes were greatly swollen and œdematous. Considerable difficulty was experienced in detaching the bladder from the front of the mass and subsequently in dealing with the enormously overstretched peritoneum. Finally the mass was amputated just below the level of the os externum; the operation was finished in the usual way. Patient made a very good recovery.

Pathological report: The tumour was a flabby spherical mass, which flattened out considerably by its own weight when placed on a hard surface. Its circumference was 42 in.; maximum diameter, 15 in.; weight, 23½ lb. The uterine muscle had undergone myxomatous change and was largely replaced by fibrous tissue.

COMPLICATIONS.

Cysts or cystomata of one or both ovaries	20	CASES
Broad ligament cyst	1	"
Thrombosis of veins or œdema of lower extremities before operation	2	"
Varicose veins of pelvis	2	"
Hydrosalpinx	1	"
Hæmatosalpinx	1	"
Pyosalpinx and tubo-ovarian abscess	3	"
Papillomata	4	"
Adhesions to viscera	12	"
Pregnancy	3	"
Inflammation of appendix	4	"
Broad ligament fibroid	1	"
Ulcer of rectum	1	"
Carcinomatous invasion	2	"
				57	"

CYSTS OR CYSTOMATA OF ONE OR BOTH OVARIES.

Under this head are grouped together all cases in which there was real cystic enlargement of one or both ovaries. In some cases these were quite moderate in size, but in others they were large, and in Case 118 the cyst was one of the chief features of the case.

Case 118.—Mrs. H. H., aged 37; no children. Patient was married on June 28, 1903, and her last monthly period occurred on July 14. She had an attack of abdominal pain and had noticed a lump in her abdomen in the previous April. Almost from the beginning of pregnancy patient suffered from frequent severe colicky pain in the abdomen. When first seen in the middle of September she appeared to be three months pregnant, and behind the uterus there was a firm, smooth swelling, which was thought to be a fibroid; a second smaller and more movable mass could be felt in the right side of the abdomen. The

patient had come into hospital on account of vomiting. Rest, suitable diet, and laxatives were advised; no operation. On October 10 patient was readmitted having had severe abdominal pain for three days with frequent vomiting. The pregnancy had progressed and the tumours had increased considerably in size; the upper mass on the right side had grown rapidly. An abdominal section was performed on October 27. The upper movable tumour was found to be a dermoid ovarian cyst with a long twisted pedicle; it was not actually strangulated. The lower mass was a subserous fibroid the size of a lemon, attached to the right side of the fundus by a base about the size of a five-shilling piece. The dermoid cyst was removed and the fibroid was enucleated without difficulty; the wound and the surface of the uterus were closed by discontinuous sutures of linen thread. Patient made a very good recovery; she went to the full term of pregnancy, a boy being born on April 11, 1904.

The parts removed consisted of the fibroid tumour and also of a dermoid cyst of the right ovary the size of a Jaffa orange; it was filled with sebaceous material and cholesterin the consistence of gruel. A tuft of hair grew from the wall at one point and a patch of ovarian tissue could still be detected at another.

Case 164.—L. B., aged 38; unmarried. No complaint except increasing size of abdomen until three weeks before admission, when she had a sudden attack of sharp pain in the left side of the abdomen which lasted a fortnight; menstruation was regular and normal. Patient looked well and strong. The abdomen was large, almost spherical, and moved well with respiration. The tumour, the size of a six months' pregnancy, occupied the lower part of the abdominal cavity; it was freely movable. The cervix was large and softer than normal, the os uteri small and round. Free fluctuation was obtained and a diagnosis of ovarian cyst was made. At operation the diagnosis of ovarian cyst was confirmed; 3 pints of fluid were drawn off and the cyst removed in the usual way. The uterus was then found to be studded with fibroid tumours varying in size from that of a pea to that of a billiard ball. Supravaginal amputation was done, the stump secured, and the peritoneum closed as usual. The interest of the case lies in the fact that although the ovarian cyst presented all the ordinary appearances of a simple unilocular cyst the internal surface was studded with patches of papillomata, some of them several inches in diameter.

The microscopical report was: A section taken across a wide mass in the cyst wall shows the structure of an adeno-carcinoma; the epithelium in most parts was single layered, but in others showed considerable

proliferation into the lumen and some invasion beyond the basement membrane.

The patient made an uninterrupted recovery and was discharged on October 12 in good health. Up to the summer of 1908 there was no recurrence.

Case 192.—Mrs. H. F., aged 49; no pregnancy. Complained of swelling of the abdomen (one year), loss of flesh (eighteen months), errors of micturition, and constipation. This was a case of moderate-sized fibroid complicated with a large ovarian cyst which contained about $7\frac{1}{2}$ pints of brownish opaque fluid. The interest of the case consisted in the fact that growing into the cavity of the cyst from its lower end was a mass of cauliflower-like growth, very friable, having papillary processes, some of which measured 3 in. in length.

The pathological report was: The uterus contains several interstitial fibroids varying in size from that of an orange to that of a pea; they were ordinary fibro-miomata, not degenerated. The sections of the wall of the ovarian cyst showed the structure of adeno-carcinoma.

VARICOSE VEINS OF PELVIS.

Case 176.—F. N., aged 52. Patient's symptoms were simply those of an abdominal tumour the size of a full-term pregnancy, together with severe abdominal pain and pain and numbness in the legs.

On opening the abdomen the tumour presented a pearly white appearance which suggested an ovarian cyst, but really was a soft oedematous fibroid. It had stripped up the broad ligament on each side, and between their folds were bunches of varicose veins, the size of each vein being that of a little finger. On the left side the tumour was intimately adherent to the sigmoid flexure, to the ovary and tube. Except for the difficulty of separating these adhesions and of securing the huge veins, the operation presented no difficulty, and the wound healed by first intention.

PAPILLOMATA.

Case 122.—F. R. F., aged 42. Operation December 9, 1903. Complained of very little except acute abdominal pain and abdominal tumour. The abdomen was enlarged to the size of a six months' pregnancy by a firm smooth tumour. *Per vaginam* a mass could be felt behind the uterus filling both fornices. At the time of operation a very severe matting of all the pelvic viscera was found, and at first the individual

viscera could not be recognized. Eventually it was found that there was a subperitoneal fibroid and two ovarian cysts. The operation was serious owing to the denseness of the adhesions. The patient recovered well.

Pathological report: Parts removed consisted of both ovaries and a pedunculated fibroid. The ovarian cysts were thin walled and studded with papillomata. Under the microscope they showed branching processes of fibrous tissue covered with columnar epithelium. In many places the columnar epithelium had broken through the basement membrane to form irregular clusters and buds of cells. The cyst fluid contained many cells which had been shed from the papillomata of the cysts. The patient did well at the time.

I am informed that early in 1905 the left mamma was removed for scirrhus, and subsequently on two or more occasions the abdomen was reopened by various surgeons, and papillomatous masses were removed. The patient survived until the summer of 1908.

PYOSALPINX.

Case 102.—Mrs. J. W., aged 39; no children. Complained of profuse and prolonged menstruation for one year, periods frequently lasting for a fortnight. On October 22 patient had a sudden flooding, with passage of large clots. She had from time to time retention of urine and much abdominal pain. Pulse about 108 as a rule. A hard, irregular abdominal swelling reached up to the umbilicus—it was very tender on palpation. A very soft and tender mass was felt *per vaginam* in Douglas's pouch; it appeared to be separable from the uterus and was thought to be an inflamed tube and ovary. On opening the abdomen a multi-nodular fibroid presented, adherent to most of the surrounding viscera, especially on the right side. There was a pyosalpinx of the right side standing head downwards in Douglas's fossa. After the separation of adhesions and the safe delivery of the distended tube, the operation had no further interest. Patient made an excellent recovery.

PREGNANCY.

It is noticeable that in the two cases in which myomectomy was performed during pregnancy (Case 118 at four months and Case 142 at three and a half months) the natural condition was not interrupted, and the patients were delivered at term of living children.

ULCER OF RECTUM.

Case 138.—R., aged 48; unmarried. Complained of incontinence of urine, menorrhagia, severe abdominal pain and very bad constipation. The operation presented no difficulty except that involved in dislodging a lobe of the tumour which was jammed into the pelvis. Within a few hours of the operation $\frac{1}{2}$ pint of bright blood was passed *per anum*, and after a time blood and faeces escaped *per vaginam*. The temperature rose and pleurisy of the right side developed about one month after the operation. This was accompanied by severe rigors, and the exploring needle showed that the right thorax was full of pus. A portion of a rib was resected and the patient did well. Microscopical examination of the pus showed streptococci and *Bacillus coli communis*. The recto-vaginal fistula healed within a month of operation, the empyema wound some months later.

As the operation was otherwise an easy one it is fair to suppose that no injury was inflicted on the rectum, especially as no faecal smell was perceived during operation. Probably the nutrition of the rectum had been impaired by long-continued pressure between the sacral promontory and the firm fibroid mass incarcerated in the pelvis.

CARCINOMATOUS INVASION.

Case 103.—C. S., aged 68; married, no pregnancy. The patient complained of moderate vaginal hæmorrhage which began some eighteen years after the menopause. On examination, the uterus was found to be enlarged and irregular, not freely movable. It was dilated and curetted, and the scrapings, on examination, proved to be malignant.

The pathological report was: The scrapings show uterine fibromuscular tissue invaded by adenomatous new growth.

Sir John Williams saw the patient in consultation, and examined her under anæsthesia; he was of opinion that there was a large, imperfectly movable uterus; he feared implication of the left broad ligament, and considered it somewhat doubtful whether the disease was entirely removable. The operation was done for the sake of the malignancy, but at the time of operation it was found that the uterus was full of fibroids, over which the peritoneum had undergone inflammation, causing innumerable adhesions. The bladder, which

reached very high on the front of the uterus, had contracted intimate adhesions with the small intestine, and the task of freeing the organ was very difficult. The patient made a good recovery, except for an attack of renal colic on the twelfth day.

A microscope section of the whole uterus was made by Mr. Lenthal Cheatele; it showed a glandular carcinoma commencing in the endometrium, fairly extensive in distribution, especially towards the lower part of the body of the uterus, but in no part did the malignant growth reach the peritoneal surface. Microscopically the new growth showed proliferating glandular epithelioma forming much convoluted tubular processes arranged in richly cellular, rather scanty stroma. The cancer-cells were columnar with oval nuclei, often showing karyokinetic changes, sometimes arranged in one layer, in other places heaped up or consisting of barely differentiated masses of protoplasm bestrewn with nuclei. The stroma consisted of spindle-cells with elongated nuclei, and in places there was a certain amount of small cell-infiltration. The rest of the body showed multiple fibro-myomata. In June, 1903, the patient had an attack of pericarditis and pneumonia, followed by cerebral symptoms, aphasia, and paralysis of the right side. The cerebral symptoms progressed, and she died without recovery of speech or movement on October 31, 1903, eleven and a half months after the operation. There was no evidence of local recurrence.

Case 152.—D. W., aged 43, single. Operation November, 1905. Sent to me by a colleague on account of sudden and severe uterine hæmorrhages. On examination, the uterus was found to be about the size of a three and a half months' pregnancy, full of small fibroids, from the size of a walnut downwards. The length of the uterine cavity was $4\frac{1}{2}$ in., the os, cervix, and vagina were virginal and normal in all respects. The uterus was removed, all except the lower portion of the cervix; the ovaries and tubes were left, as they were quite healthy. There was no difficulty in the operation, and her recovery was perfect. She was seen in good health in October, 1908. No suspicion of malignant invasion arose until the uterus was examined after the operation was finished.

The pathologist's report was: Specimen consists of the body and about three-quarters of the cervix uteri. The body of the uterus was about the size of a three and a half months' pregnancy, and contained numerous interstitial fibroids. The endometrium showed growth of another kind; nearly the whole of it, except at the extreme fundus and around the internal openings of the Fallopian tubes, was infiltrated with new growth. The surface was raised, irregular, and whitish yellow

in colour, with here and there marked, almost polypoid, elevations. There was no breaking down or ulceration, but near the lower part of the body on the left side there was a pedunculated oval growth the size of a Spanish olive with a tracery of vessels on its surface. On section it showed a greyish white homogeneous brain-like tissue. The tip of this growth projected through the internal os; it was infiltrated with blood and was evidently the source of the recent hæmorrhage.

Dr. Cuthbert Lockyer's report on the microscopic examination was: The section shows important changes in the endometrium. The uterine tubules are actively proliferating and the epithelium in one area has burst through its basement membrane, forming irregular masses of malignant-looking cells; the latter are lying amidst fragments of tubules, the whole presenting a very complex and irregular appearance. It is noteworthy that the uterine muscle is not invaded by gland tissue, either malignant or benign. The above signs of malignancy apply to a circumscribed area of the mucous membrane. The bulk of the mucosa shows a benign hypertrophy of glands and stroma.

This paper does not profess to discuss many interesting questions relating to fibroids, such as remote complications in distant organs, *e.g.*, the heart or the kidneys. It makes no reference to the sterility or fertility of the patients, and does not examine into their ages and their civil status. The one aim and object of this paper is to put on record the number and kinds of degenerations and complications met with in my second hundred of operations for fibroids.

It has been ably maintained and is still held by many authorities that fibroids are innocent tumours, that they never destroy life, and that operations for their removal are seldom justifiable. These authorities further deprecate operations for the removal of fibroids on the ground of the high rate of mortality involved.

The object of this paper is to show the frequency and the gravity of the degenerations and the complications of fibroids, and the very low percentage of deaths after operation at the present time.

With regard to the method of operating, the vaginal route has not been used in this series, but it was in thirteen cases out of the first hundred. It is, of course, the route to use in dealing with fibroid polypi of all sizes, and is also the method of election for the removal of sloughing and infected fibroids of moderate size when the vagina is of sufficient calibre to permit of the necessary manipulations.

With regard to the question of abdominal panhysterectomy versus

abdominal supravaginal amputation, the former is the ideal operation, and will hold the field in the future.

The cervix uteri is very vulnerable and prone to malignant disease. It is often impossible to define the nature of a uterine growth until it has been properly prepared and microscopically examined. Therefore it is surely wise to remove the whole uterus rather than to leave the vulnerable and possibly malignant cervix.

It sometimes happens that a tumour which, to the naked eye, appears to be a fibro-myoma, is shown on further examination to be a sarcoma, and in other cases the subsequent history suggests the same lesson.

Thus, in Case 163 of this series, malignant disease of the ovaries must have developed very soon after the removal of the uterus for what appeared to be an ordinary fibroid. The first operation was done on July 24, 1906; it was noted that the ovaries were healthy, and they were not removed. At the time of the second operation (December, 1907), the patient said that she had noticed a large tumour for six months, and that her doctor had told her she had two ovarian cysts. The abdomen was filled with a tumour resembling a jelly-fish, which sprang from both ovaries. It had invaded the small intestines, and new growth was felt in the liver. On section and microscope examination these growths were found to be fasciculated sarcoma.

Analysis of 100 Consecutive Cases of Fibro-myomata Uteri.

By Mrs. SCHARLIEB, M.S.

No.	Name and age	Diagnosis	Operation	Complications	Degenerations	Date	Symptoms	Remarks	Result
101	Mrs. N. M., 37	Uni-nodular fibroid	Supravaginal hysterectomy	Cystic ovaries	—	Oct. 18, 1902	Menorrhagia, rapid growth, faintness	—	Recovery
102	Mrs. J. W., 39	Multiple fibro-myomata	Supravaginal hysterectomy	Tubo-ovarian abscess and pyosalpinx	—	Nov. 12, 1902	Menorrhagia and metrorrhagia, retention of urine	—	Recovery
103	Mrs. C. S., 68	Multiple fibro-myomata	Panhysterectomy	Carcinoma; severe adhesions	—	Nov. 13, 1902	Hæmorrhage	Operation performed for carcinoma, fibroids small	Recovery
104	Mrs. B., 50	Fibroid	Supravaginal hysterectomy	—	Endothelioma or sarcoma	Nov. 28, 1902	Flooding, pressure on heart and lungs, inability to eat and walk	Duration of tumour eleven years, no recurrence up to August, 1908	Recovery
105	Mrs. L. N., 40	Fibroids	Panhysterectomy	Double pyosalpinx; very bad adhesions	—	Jan. 2, 1903	Pain, collapse; temperature 102° F.	—	Recovery
106	Mrs. A. H., 46	Sloughing fibroid	Supravaginal hysterectomy	—	Sloughing fibroid	Jan. 6, 1903	Menorrhagia and metrorrhagia, sepsis	Sloughing mass passed per vaginam December 25, 1902; attempt at enucleation, December 26, 1902	Recovery
107	Mrs. T., 53	Fibroids	Panhysterectomy	—	—	Jan. 7, 1903	Hæmorrhage, pain, dismemberment	Died during operation for enterovaginal fistula some months later	Recovery
108	Mrs. B.	Fibroids	Supravaginal hysterectomy	Multilocular cyst, right ovary	—	Feb. 21, 1903	Constant offensive hæmorrhage since menopause two years before	—	Recovery
109	Miss J. K., 51	Multi-nodular fibro-myomata	Panhysterectomy	Fibroid of broad ligament	—	Mar. 17, 1903	Hæmorrhage, pain, dismemberment	—	Recovery
110	A. B., 44	Multi-nodular fibro-myomata	Supravaginal hysterectomy	—	—	Mar. 20, 1903	Menorrhagia, metrorrhagia, emaciation, pain and difficulty in micturition	—	Recovery

No.	Name and age	Diagnosis	Operation	Complications	Degenerations	Date	Symptoms	Remarks	Result
111	A. L., 40	Multi-nodular fibro-myomata	Supravaginal hysterectomy	—	Cystic	May 12, 1903	Menorrhagia, tumour	—	Recovery
112	K. K., 48	Fibro myomata	Pan hysterectomy, removal of appendages	—	—	May 29, 1903	Menorrhagia six years, menorrhagia and tumour two years	—	Recovery
113	P., 47	Multiple fibro-myomata	Supravaginal hysterectomy and right salpingo-oophorectomy	Both ovaries showed perioophoritis	—	June 29, 1903	Backache, nausea, vomiting	—	Recovery
114	Mrs. M. G., 47	Fibro-myomata	Supravaginal hysterectomy and oophorectomy	—	—	July 10, 1903	Tumour four years, frequency of micturition and vaginal discharge	—	Recovery
115	E. J. A., 46	Fibro-myomata and cystic ovaries	Pan hysterectomy and oophorectomy	Cystic ovaries and bad adhesions	Calcification	Sept. 14, 1903	Severe menorrhagia and menorrhagia, extreme anemia	—	Recovery
116	Mrs. H. T., 37	Fibroid myoma and ovarian cyst	Supravaginal hysterectomy and right ovariectomy	—	—	Sept. 18, 1903	Hemorrhage seven years, dysmenorrhoea	—	Recovery
117	H. J. M., 46	Multi-nodular fibroid	Pan hysterectomy	Right ovarian cyst	Benign adenomata	Sept. 18, 1903	Intermittent severe hemorrhage seven years, hemorrhages lasting nine weeks to five months	—	Recovery
118	Mrs. H. H., 37	Uni-nodular fibroid	Myomectomy and ovariectomy	Right dermoid with twisted pedicle; pregnancy, four months' duration	—	Oct. 27, 1903	Severe colicky pain, with rapid growth of tumours and vomiting	Pregnancy recognized; rapid growth of tumour necessitated operation; pregnancy continued uninterrupted, large boy born at full term	Recovery
119	L. P., 42	Multiple fibro-myomata	Supravaginal hysterectomy	—	—	Nov. 7, 1903	Pain, rapid growth of tumour	—	Recovery
120	A. D., 37	Uni-nodular fibroid	Supravaginal hysterectomy	Cystic left ovary and general fibrosis of uterus	—	Nov. 23, 1903	Marked sense of illness, frequency of micturition, frequent and painful periods	—	Recovery

121	A. B., 41	Multi-nodular fibro-myomata	Supravaginal hysterectomy	Cystic right ovary	Mucoid softening	Dec. 1, 1903	Tumour, pain down right leg, incontinence of urine	—	Recovery
122	F. R. F., 42	Fibro-myomata	Myomectomy, double ovariectomy	Malignant papillomatous disease of both ovaries, scirrhous left mamma	—	Dec. 9, 1903	Acute abdominal pain, tumour	Abdomen and both ovaries full of new growth, and mass in meso-colon	Recovery
123	Mrs. M. F., 40	Sloughing fibroid after confinement	Panhysterectomy	Parturition	Sloughing	Dec. 15, 1903	Fibroid recognized at confinement; two months later severe pain and flooding, extrusion of mass into vagina	Sloughing mass removed by Miss Vaughan, December 12, 1903, patient remaining very ill; pulse 120, temperature 101.4° F.; abdominal panhysterectomy, December 15, 1903, followed by suppuration of wound and pelvic cellular tissue; vesico-vaginal fistula, and bronchitis	Recovery
124	M. C., 37	Multi-nodular fibro-myomata	Supravaginal hysterectomy	—	—	Dec. 15, 1903	Ill three years; menorrhagia, dyspnoea, palpitation, neuralgia, tumour	—	Recovery
125	E. G., 25	Multiple fibro-myomata	Supravaginal hysterectomy	—	—	July 5, 1904	Tumour, menorrhagia, dyspepsia, nausea, vomiting	Attempt at enucleation disclosed innumerable small fibroids, necessitating hysterectomy	Recovery
126	E. R., 46	Multiple fibro-myomata	Supravaginal hysterectomy and double oophorectomy	CEdema of right leg	—	July 5, 1904	Tumour, pain and numbness of right leg, frequency and difficulty of micturition, severe constipation, menorrhagia	—	Recovery
127	Mrs. W., 50	Multiple fibro-myomata	Supravaginal hysterectomy and right oophorectomy	Both ovaries unhealthy	—	July 20, 1904	Menorrhagia, metrorrhagia, severe anaemia	—	Recovery
128	E. J., 38	Fibro-myomata	Supravaginal hysterectomy and double oophorectomy	Cystomata both ovaries, many adhesions	—	Sept. 13, 1904	Six years tumour and pain, menorrhagia, micturition painful and frequent	—	Recovery
129	E. W., 37	Multiple fibro-myomata	Supravaginal hysterectomy and oophorectomy	Cystic degeneration of ovaries	—	Oct. 22, 1904	Headache, faintness, menorrhagia, constipation, tumour ($\frac{7}{8}$)	—	Recovery

No.	Name and age	Diagnosis	Operation	Complications	Degenerations	Date	Symptoms	Remarks	Result
130	M. N., 40	Retroflexion, ? ovarian cyst	Supravaginal hysterectomy and left sal- pingo-ophor- ectomy	Adhesions	—	Nov. 10, 1904	Pain, bearing down	The case was diagnosed as ovarian cyst, but proved to be a soft fibroid in pelvis together with multiple small fibro-myoma	Recovery
131	Mrs. E. J., 34	Multiple fibro- myoma	Supravaginal hysterectomy and oophorec- tomy	Cystic disease of ovaries	Cystic	Nov. 15, 1904	Pain in abdomen and back seven years, tumour two years constipation, menor- rhagia	On opening abdomen dark blood welled up out of pelvis due to recently ruptured vein on pos- terior aspect of tumour	Recovery
132	Mrs. K. S., 43	Fibro-myoma	Supravaginal hysterectomy	—	—	Nov. 25, 1904	Frequent micturition, reten- tion of urine, menorrhagia, intense vertical headache	—	Recovery
133	M. C., 56	Fibro-myoma	Panhysterec- tomy, double oophorectomy	—	Necrobiosis	Dec. 14, 1904	Tumour eleven years, some blood-stained vaginal dis- charge	Tumour completely filled the uterus, which was stretched over it like a shell; it had not perforated the musculature at any point; patient remains well (August, 1906)	Recovery
134	D. L., 52	Multiple fibro- myoma	Panhysterec- tomy, double oophorectomy	—	Cystic and necrotic	Jan. 8, 1905	Tumour sixteen years, pain necessitating use of mor- phia, menorrhagia	Particularly severe operation, left hospital twenty-second day	Recovery
135	Mrs. I. S., 35	Multi-nodular fibro-myoma	Supravaginal hysterectomy and double oophorectomy	Matting of ap- pendages	Small area	Jan. 13, 1905	Pain, nausea, vomiting, difficulty in micturition	—	Recovery
136	G. S., 41	Small fibroid	Supravaginal hysterectomy, right oophorec- tomy and appen- dicectomy	Colitis, disease of right ovary and appendi- citis	—	Jan. 31, 1905	Menorrhagia, metrorrhagia, small tumour, disability	—	Recovery
137	Mrs. K., 42	Fibro-myoma and polypus	Panhysterectomy	Some adhesions	—	Feb. 15, 1905	Persistent menorrhagia and metrorrhagia	Double oophorectomy had been performed February 14, 1903, for tuberculous tubo-ovarian abscess, from which there was good recovery, but hemorrhage persisted	Recovery

138	R., 48	Multiple fibro-myoma	Pan-hysterectomy, right salpingo-oophorectomy	Ulcer of rectum	—	Feb. 22, 1906	Incontinence of urine, menorrhagia, severe pain, severe constipation	A lobe of tumour jammed in pelvis had interfered with nutrition of rectum opposite sacral promontory; ½ pint bright blood passed <i>per anum</i> a few hours after operation, next day blood and faeces <i>per vaginam</i> ; temperature rose, right pleurisy developed; severe rigors March 25 and 26; resection of rib, right thorax filled with pus; microscopic report—streptococci and <i>Bacillus coli communis</i> ; recto-vaginal fistula healed before end of March, empyema wound later	Recovery
139	C. B., 46	Multiple fibro-myoma	Pan-hysterectomy, right salpingo-oophorectomy	Cysts of right ovary	Calcification	Mar. 3, 1905	Pain, constipation, frequent micturition	—	Recovery
140	A. H., 49	Supposed malignant adenoma uteri, small multiple fibroids	Supravaginal hysterectomy, removal of both appendages and appendiceotomy	Appendicitis, double hæmatosalpinx and pelvic adhesions	—	Mar. 17, 1905	Great sense of illness and menorrhagia three years, unable to work two months	Operation not particularly severe but she did not rally, remained with blue lips, died of cardiac syncope; post mortem, operation area satisfactory	Died four days after operation
141	Mrs. D., 43	Multiple fibro-myoma	Supravaginal hysterectomy, left salpingo-oophorectomy and appendiceotomy	Pneumonia commencing at time of operation, chronic appendicitis	—	Mar. 26, 1905	Tumour size, 8", dyspepsia, alternating diarrhoea and constipation, periods bi-monthly	The patient was complaining of cold at time of operation, pneumonia developed in few hours, ended by crisis fourth day; surgical condition remained satisfactory, patient discharged twenty-first day	Recovery
142	Mrs. S. K., 39	Pregnancy three and a half months with necrosed fibroid	Myomectomy	Pregnancy	Necrobiosis	Mar. 28, 1905	Amenorrhoea three and a half months, tumour felt to right of uterus, probable tubal gestation	Appendages healthy; tumour, attached to right cornu size of large hen's egg, adherent to sigmoid flexure, removed; uterine wound closed, pregnancy continued to full term	Recovery

No.	Name and age	Diagnosis	Operation	Complications	Degenerations	Date	Symptoms	Remarks	Result
143	Mrs. S. T., 46	Fibromata plus carcinoma cervicis	Panhysterectomy, double oophorectomy	Carcinoma cervicis	—	June 16, 1905	Hæmorrhage and clots three months, yellow offensive discharge, dyspnea and anemia	The carcinomatous change was limited to the cervix, was squamous-celled, fibroid size of five months' pregnancy; intra-mural and submucous	Recovery
144	Mrs. M. A. H., 46	Fibro-myomata	Panhysterectomy, left oophorectomy	—	Calcification of fibroids	July 24, 1905	Severe abdominal pain and flooding, progressive emaciation and weakness, dropsy, aortic disease, renal insufficiency	Operation was undertaken to relieve patient of hæmorrhages and pressure symptoms; her condition was very poor before operation, pneumonia and septic peritonitis supervened; died fifth day	Died on fifth day
145	Mrs. G. B., 40	Abdominal and impacted fibroids	Panhysterectomy (bi-section uteri), double salpingo-oophorectomy	Severe adhesions	—	Aug. 9, 1905	Frequent and profuse hæmorrhage eight months, tumour	Multiple fibro-myomata, one of which was so jammed in pelvis that the promontory of the sacrum made on it a deep atrophic groove, consequently uterus bisected before ablation	Recovery
146	Mrs. F. S., 40	—	Supravaginal hysterectomy, left oophorectomy, ovariectomy	Ovarian cyst, hæmato-salpinx, adhesions	—	Aug. 9, 1905	Backache, severe abdominal pain, continuous blood-stained vaginal discharge, tumour	Abdominal examination suggested multi-follicular ovarian cysts—these were found; in addition several fibroids and left hæmato-salpinx; adhesions very troublesome	Recovery
147	Mrs. M. J., 50	Fibro-myoma	Panhysterectomy, right salpingo-oophorectomy	Right salpingo-oophoritis and papillomatous cyst	Purulent, infected	Sept. 28, 1905	Retention of urine, flooding, January, 1905; abundant constant yellow discharge, tumour	Adhesions very tiresome	Recovery
148	Mrs. A. S., 43	Fibro-myoma	Panhysterectomy, left salpingo-oophorectomy	—	—	Oct. 6, 1905	Prolapsus uteri, menorrhagia, dysmenorrhœa, offensive discharge	The cervix and lower segment of uteri forced out of vagina by the pelvic fibroid	Recovery
149	Mrs. E., 36	Multiple fibro-myoma	Supravaginal hysterectomy, salpingo-oophorectomy, appendectomy	Adhesions	—	Oct. 30, 1905	Persistent hæmorrhage, several years' duration, tumour, incapacitated for missionary work	—	Recovery

150	E. C., 55	Fibro-myoma	Panhysterectomy	—	—	Nov. 14, 1906	Pain, tumour, severe constipation	Uterus and tumour were so wedged into pelvis that they could not be drawn up; bisection, enucleation, ablation of uterus	Recovery
151	H. C., 45	Multiple fibro-myoma	Panhysterectomy, double oophorectomy, appendectomy	—	Mucoid	Nov. 22, 1905	Failing health, tumour	Very feeble patient	Recovery
152	D. W., 43	Small fibroid, ? plus polypus	Supravaginal hysterectomy	Early carcinoma of endometrium	—	Nov. 25, 1905	Sudden severe flooding	A case of coexistence of fibroids with malignant disease of endometrium, not degeneration of fibroid; carcinoma discovered after operation on microscopic examination	Recovery
153	D. P., 33	Fibro-myoma	Myomectomy	—	—	Jan. 19, 1906	Abdominal tumour some years, disability for work	The tumour and uterus were extraordinarily vascular; every needle-prick bled profusely, checked by injection of hemisine	Recovery
154	Mrs. J., 42	Fibro-myoma	Panhysterectomy	—	—	Jan. 20, 1906	Profuse menorrhagia and metrorrhagia, backache	Peculiar fibrosis of whole uterus, probably due to precedent metritis	Recovery
155	Mrs. K. S., 46	Multiple fibro-myoma	Panhysterectomy	—	—	Jan. 26, 1906	Extreme constipation, varicose veins plus phlebitis, tumour	—	Recovery
156	E. M., 43	Multiple fibro-myoma	Panhysterectomy	Cystic disease right ovary	—	Feb. 13, 1906	Weakness, pain, frequent micturition, tumour	—	Recovery
157	Mrs. F. M., 42	Fibro-myoma	Panhysterectomy	—	—	Feb. 27, 1906	Severe pain, partly bed-ridden, yellow discharge	—	Recovery
158	C. P. S., 39	Fibroid uterus	Panhysterectomy	—	—	May 9, 1906	Continuous hemorrhage five years, constipation, pain on defecation	Fibrosis of uterus; remarkably swollen and edematous mucous membrane	Recovery
159	A. C., 42	Fibroid	Panhysterectomy	Diseased right ovary	—	June 7, 1906	Menorrhagia, dysmenorrhea twelve years, metrorrhagia, spasmodic pain	—	Recovery
160	Mrs. A. S.	Uterine fibroids	Panhysterectomy; salpingo-oophorectomy	Diseased ovaries and tubes	—	July 20, 1906	Backache six years, headache, uterine hemorrhage	—	Recovery
161	H. S. C., 49	Fibroid tumour	Panhysterectomy	Phlebitis, swollen legs, apical systolic murmur	—	July 21, 1906	Tumour, exhausting menorrhagia, bladder irritable	—	Recovery

No.	Name and age	Diagnosis	Operation	Complications	Degenerations	Date	Symptoms	Remarks	Result
162	Mrs. M. T., 44	Multiple fibroids	Panhysterectomy	—	—	July 24, 1906	Profuse periods, prolapsus and pain	—	Recovery
163	A. W., 54½	Fibroid myoma	Panhysterectomy	Constipation amounting to obstruction, and cystitis	?	July 28, 1906	Obstruction, acute dysuria, bedridden, severe dysmenorrhea	—	Recovery
164	L. B., 38	Ovarian cyst	Ovariectomy and supravaginal amputation of uterus	Ovarian cyst	—	Sept. 21, 1906	Abdominal tumour, sharp pain left side of abdomen	Fibroids were not degenerated, but the wall of the ovarian cyst showed the structure of adeno-carcinoma; in perfect health, June, 1907	Recovery
165	Mrs. V. H., 47	Multi-nodular fibroids	Panhysterectomy	—	—	Sept. 27, 1906	Piles, severe constipation, involuntary passage of flatus	Ovaries and tubes normal, the fibroids absolutely jammed in pelvis	Recovery
166	M. S. S., 43	Uterine fibro-myomata	Supravaginal hysterectomy	—	—	Oct. 2, 1906	Constipation, difficult micturition, profuse menorrhagia and metrorrhagia	The constipation and difficult micturition were probably due to the jamming of the tumour in the pelvis	Recovery
167	J. C., 40	Fibro-myomata uteri	Supravaginal hysterectomy	—	—	Oct. 5, 1906	Hypogastric pain eighteen months, tumour, menorrhagia	Several fibroids, one of which involved lower anterior wall of uterus; endometrium swollen, oedematous	Recovery
168	M. R., 39½	Fibromata	Supravaginal hysterectomy	—	—	Oct. 24, 1906	Excessive loss and anemia	Patient's condition was very bad, chiefly due to anemia and pressure of tumour in pelvis	Recovery
169	E. R., 44	Fibro-myomata	Supravaginal hysterectomy	—	—	Nov. 23, 1906	Repeated floodings during two years	—	Recovery
170	V. F., 36	Fibro-myomata	Supravaginal hysterectomy	—	—	Nov. 28, 1906	Severe dysmenorrhœa, menorrhagia	Operation was performed on account of rapid deterioration of general health	Recovery
171	D. H., 37	Fibro-myomata	Supravaginal hysterectomy	—	—	Dec. 31, 1906	Inveterate constipation	The operation appeared to be justified by the constipation, which was unrelieved by drugs, diet, exercises, and massage	Recovery
172	M. B., 36	Fibroids or carcinoma	Panhysterectomy	Cystic left ovary	—	Feb. 12, 1907	Severe pain in back, dysmenorrhœa and irregular periods, loss of flesh	The patient was suspected of carcinoma uteri, but after removal the uterus was found to be full of fibroids, which had not undergone any degeneration	Recovery

173	C. H., 47	Fibro-myomata	Supravaginal hysterectomy and ovariectomy	Ovarian cyst, containing 2 pints of fluid	Calcification	Feb. 15, 1907	Abdominal pain and tumour	—	Recovery
174	Mrs. B. A., 41	Fibro-myomata	Supravaginal hysterectomy	—	—	Feb. 15, 1907	Menorrhagia, pain re- sembling that of parturi- tion, retention of urine and aphonia	—	Recovery
175	F. B., 38	Fibro-myomata	Myomectomy	Right ovary cystic	—	Mar. 1, 1907	Pain and rapidly growing fibroids, great sense of illness	Three fibroids removed by myo- nectomy; the raw areas were sewn over to avoid adhesions	Recovery
176	F. N., 52	Uninodular fibroid	Supravaginal hysterectomy	Adhesions to bowel, enor- mous bunches of varicose veins in broad ligament	—	Mar. 13, 1907	Large tumour, severe pain, numbness in legs	The tumour had opened up the broad ligaments on either side, and thus was really retro- peritoneal	Recovery
177	M. S., 45	Fibroids	Supravaginal hysterectomy	Severe polyoid endometritis, diseased left appendages	—	April 30, 1907	Pain, profuse periods	—	Recovery
178	Mrs. L. D., 26	Multilocular ovarian cyst	Panhysterectomy	Cystic ovaries; Myxomatous dilated lymphatics in broad liga- ments re- sembling bunches of white grapes	—	May 8, 1907	Gradually increasing size, emaciation, and general ill-health	This tumour was remarkable for being covered with five or six distinct capsules, which were probably laminated peri- tonium; the peritoneum had been stripped off the lateral and posterior aspects of the abdomen	Recovery
179	E. S., 36	Fibroids	Panhysterectomy	Varicose veins of broad liga- ments	—	May 10, 1907	Abdominal tumour, urgency and frequency of micturi- tion, apical systolic murmur	—	Recovery
180	F. C., 40	Fibro-myomata	Supravaginal hysterectomy	—	—	May 29, 1907	Pain in abdomen and rise of temperature	—	Recovery
181	Mrs. M. D., 36	Multiple fibroids	Panhysterectomy and removal of appendages	Diseased ap- pendages	Necrobiotic cervical fibroid	July 2, 1907	Irregular and profuse periods, pain extending down legs	Dilatation and curettement thir- teen years ago; small fibroid noted at time	Recovery
182	A. T.	Multiple fibroids and polypus	Supravaginal hysterectomy	Cyst of right broad liga- ment	—	Aug. 6, 1907	Abdominal tumour, pain, incontinence, dysuria, constipation, piles	—	Recovery

No.	Name and age	Diagnosis	Operation	Complications	Degenerations	Date	Symptoms	Remarks	Result
183	Mrs. S. W. 39	Multiple fibroids	Supravaginal hysterectomy and removal of left appendages	Diseased left appendages	—	Sept. 16, 1907	Very profuse losses with short intervals for two years	Curet November, 1906, but was not benefited, and drugs were useless	Recovery
184	Mrs. R. D., 39	Fibroids	Panhysterectomy; removal of left ovary and tube, also appendix	Three concretions in appendix, diseased left ovary and tube	—	Sept. 21, 1907	Profuse, frequent, and exhausting losses, continuing sometimes for two months, and enlargement of uterus	Patient looked so ill that malignant disease was feared	Recovery
185	B. G., 42	? Carcinoma of uterus or sloughing fibroid	Panhysterectomy	—	Sloughing	Oct. 4, 1907	Profuse watery offensive discharge; flooding; severe pain	Patient looked very ill and had suffered much; convalescence delayed by infection of abdominal wall by the sloughing fibroid, which protruded through the os uteri	Recovery
186	Mrs. M. C., 45	Fibromyomata	Supravaginal hysterectomy	—	—	Oct. 28, 1907	Abdominal tumour, frequency of micturition, severe piles	Bladder much drawn up on the tumour, which so pressed into the brim as to greatly limit the cubic capacity of that viscus	Recovery
187	Mrs. M. K., 47	Fibroids	Supravaginal hysterectomy and right salpingo-oophorectomy	—	—	Nov. 1, 1907	Menorrhagia and metrorrhagia, dysuria, dyspepsia, anemia	—	Recovery
188	K. P., 48	Fibroids	Supravaginal hysterectomy and left salpingo-oophorectomy	Disease of left appendages	—	Dec. 30, 1907	Increased frequency and profuseness of menstruation, frequency of micturition and constipation	—	Recovery
189	Mrs. E. C., 39	Fibroids	Supravaginal hysterectomy	Salpingitis	—	Jan. 10, 1908	Abdominal tumour; retention of urine, with great distress	—	Recovery
190	Mrs. M., 49	Fibroids	Panhysterectomy	Adhesions between uterus and intestines	—	Jan. 11, 1908	Almost continuous uterine hemorrhage, very severe at intervals, constipation, dyspepsia, frequency of micturition	Endometrium markedly hypertrophic and hyperplastic	Recovery

191	B. C., 49	Fibroids	Panhysterectomy and salpingo-oophorectomy	—	Jan. 29, 1908	Menorrhagia, extreme anemia, abdominal tumour, vomiting	—	Recovery
192	Mrs. H. F., 49	Fibroids and ovarian cyst	Panhysterectomy and ovariectomy	Ovarian cyst	May 8, 1908	Abdominal tumour, emaciation, dysuria, constipation	Microscopic section—wall of cyst showed structure of adenocarcinoma; there was no degeneration of the fibroid	Recovery
193	E. A. B., 37	Multiple fibroids in uterus	Supravaginal hysterectomy	—	June 3, 1908	Pain and discomfort preventing earning of livelihood	—	Recovery
194	M. H., 48	Fibroids	Supravaginal hysterectomy	Symptoms of gastric ulcer	June 16, 1908	Excessive metrorrhagia with pain, also frequency of micturition and dysuria, constipation	It was thought that patient had also gastric ulcer; she had much pain in epigastrium and vomiting after every meal; no haematemeses	Recovery
195	Mrs. J. M., 44	Fibroids and umbilical hernia	Panhysterectomy	—	July 14, 1908	Tumour, vomiting, dysuria, constipation	Patient suffering from umbilical hernia, which was cured at time of operation	Recovery
196	Mrs. S. M., 55	Uterine fibroid	Myomectomy	—	Myomatous July 24, 1908	Tumour, dysuria	Tumour situated in left broad ligament, close to uterus; was it extended from uterus or developed in broad ligament?	Recovery
197	J. B.	Fibroid	Supravaginal hysterectomy	—	July 27, 1908	Menorrhagia, tumour, dyspepsia	—	Recovery
198	Mrs. E. T.	Fibroid	Supravaginal hysterectomy	Cyst of left ovary	July 31, 1908	Large tumour; frequency and profuseness of periods, pain	Absolutely incapacitated from work	Recovery
199	Mrs. M. H., 46	Fibroid	Panhysterectomy	Left hydro-salpinx	Aug. 14, 1908	Large abdominal tumour, dyspnoea, extreme frequency of micturition	—	Recovery
200	Mrs. R. S.	Fibroid	Panhysterectomy	Right ovary and tube very adherent; large varicose veins of broad ligaments; pyosalpinx	Oct. 23, 1908	Abdominal tumour size of five months' gestation, menorrhagia, retention of urine, constipation	—	Recovery

Panhysterectomy	...	43
Supravaginal amputation	...	51
Myomectomy	...	6
		100

DISCUSSION.

Mr. ALBAN DORAN considered that this brilliant series showed that a patient might be relieved, at very little risk, of the mental distress caused by the presence of a uterine fibroid. At the same time he thought that many of the cases included in the tables were much too recent. Removal of a fibroid uterus was quite different from ovariectomy; an after-history of at least two years was necessary before the full benefits of the hysterectomy could be determined. He also regretted that the tables did not include a column noting the treatment of the ovaries as well as a column for the after-history, as in his own series brought before the Obstetrical Society just three years ago. His experience was identical with Lucas-Championnière's, who recently declared that an artificial menopause was a very grave matter, and that year by year he spared more and more ovaries when performing hysterectomy.

Dr. MACNAUGHTON-JONES said that it was difficult to pass over in silence such a paper as that of Mrs. Scharlieb. Such a complete record of operations on cases which had to be taken as they came was evidence of the most brilliant surgery. Looking through the list of cases, seeing the large number of grave complications that existed and the unpropitious circumstances in which the operations were frequently performed, a mortality of 2 per cent. was truly wonderful; the more so when they realized that 40 per cent. of the cases required panhysterectomy. Looking through the list, he observed that in only one instance was there any reference to cardiac complications, and as the association of cardiac troubles with uterine myomata was not infrequent, and now and then made the question of operation and anæsthetization a serious one, it was important that such cardiac conditions should be noted. Also, he noticed that there was no reference to any renal complication in any of the cases. Dysuria and local pelvic troubles, the result of pressure, were present in several, but renal complications were not alluded to in any. The presence of such was often an important factor in deciding operation. A noteworthy feature of the record was the large number of women over 50 years of age who were operated upon, and the fact that some two-thirds of the cases in all were over 40 years of age was an important point in the etiology of myoma.

Dr. HEYWOOD SMITH wished to ask Mrs. Scharlieb what was the proportion among the married women in her list of cases of those who had had children and of those who were sterile, as thereby perhaps some light might be thrown on the question whether the existence of fibroids was the cause of sterility or sterility the cause of the fibroids.

The PRESIDENT (Dr. Herbert Spencer) supposed that this was a consecutive series of *operations* for uterine fibroids and not of cases seen. He inquired if Mrs. Scharlieb could give an idea of the proportion of operations to cases seen, and also as to the method of examining the specimens; obviously these factors would to a large extent influence the frequency of degenerations and complications met with. Amongst fifty fibro-myomatous uteri recently examined he had

found degenerations (hyaline, mucous, and cystic) in no less than half the cases. He thought Mrs. Scharlieb was to be congratulated on the low rate of mortality. He was surprised to hear that no fibroid had been removed by her through the vagina for several years past. Surely fibroid polypi were so removed, and he thought all submucous fibroids not larger than a foetal head and many infected fibroids should be removed by the vaginal route. The danger of operating by the abdominal route for infected tumours was well illustrated by one of the cases. He agreed with Mrs. Scharlieb in preferring total hysterectomy to supravaginal amputation, and was surprised that she had not entirely given up the latter operation. He asked on what grounds Mrs. Scharlieb asserted that the bladder was more endangered by the total operation; the statement was evidently not based on her own experience, and he was of the opinion that, if Doyen's method were employed, the bladder could only be injured by careless operating.

Mrs. SCHARLIEB, in reply to Mr. Alban Doran's question with regard to the retention of ovaries, stated that she retained both ovaries if both were healthy, one ovary if only one were healthy, and a portion of one if even a minute portion appeared to be healthy; that she had followed up her cases so far as was possible, and that in the great majority of instances the women enjoyed good health and did not suffer from the nervous troubles of an exaggerated menopause. Replying to Dr. Macnaughton-Jones's questions as to the condition of heart and kidneys in cases of operation for fibroid, Mrs. Scharlieb admitted that many cases gave rise to considerable anxiety on these scores, but said she had not alluded to them inasmuch as they could not be included in degenerations or local complications of fibroids. For the same reason she had not entered into any estimate of the different ages of her patients nor their sterility or fertility. The latter consideration was also an answer to the question of Dr. Heywood Smith, who asked how many women out of the 200 were fertile. In conclusion, Mrs. Scharlieb thanked the President for his remarks on her paper; she said that there were twenty-three cases of degeneration, that the operation of panhysterectomy was done in 42 per cent., and that she habitually removed fibrous polypi of various sizes *per vaginam*.

**Cæsarean Section and Total Abdominal Hysterectomy for
Retroflexion of the Uterus at Term by Fibro-myoma
adherent in Douglas's Pouch.**

By HERBERT R. SPENCER, M.D.

M. A. N., a primigravida, aged 38, was admitted late on Sunday night, March 29, 1908, to University College Hospital. The patient last menstruated on June 31, 1906. She was a healthy woman, and had never had any trouble with micturition or defæcation, either before or during her pregnancy, but since February, 1907, she used to get occasionally a sharp pain in the hypogastric region. Menstruation had always been normal in duration, frequency, and amount. Slight pains and a little red discharge had occurred on March 26, 27, and 28, on which day the membranes ruptured at 4.30 p.m. At midnight the patient had two sharp pains, which then ceased. On March 29 the red discharge became more pronounced, and later in the day pains occurred, and the medical attendant, Mr. Harold Dane, was summoned and, finding the tumour in the pelvis, sent the patient to the hospital. On examination at 10.30 p.m. the patient was found to be in good condition; the labour pains recurred about every seven minutes. The abdomen was of unusual shape, the uterus being much wider above than normal, and in its anterior wall two small flattened fibroids could be felt. The lower part of the uterus bulged forwards considerably, and was very thin. It looked and felt something like a distended bladder, but there was no sharp upper limit as seen in the case of the distended bladder, and after the bladder had been emptied by catheter the bulging remained. Owing to the rigidity of the parts the head could not be clearly defined, but it appeared to lie in the right hypochondrium. On vaginal examination, a hard fixed tumour of the size of the fist occupied the pelvis. The finger could be passed between it and the pubes, but could only just touch the anterior lip, which was high up above the pubes. The lip felt quite soft. The tumour was evidently a fibroid adherent in Douglas's pouch. It was not quite clear what the bulging of the lower segment was due to. It was not due to placenta, for when the patient was under an anæsthetic the lower limbs of the child, which presented by the breech, could be felt through the thin lower segment with great distinctness. The true explanation was found

at the operation to be the stretching and thinning of the cervix and lower segment, owing to the fixation of the fundus by the adherent fibroid, which prevented the posterior wall from elongating and so caused the enlargement of the uterine cavity to take place at the expense of the anterior wall. A dirty brown discharge escaped from the uterus after the examination. The patient had not felt the movements of the child for some hours, and a prolonged examination failed to give evidence that the child was alive.

The patient was prepared for abdominal section and was operated on just after midnight. An incision 6 in. long exposed the uterus, which was covered to an unusual extent with movable vascular peritoneum. This was due to the great stretching of the cervix and lower segment, and the consequent extension of the area covered with loose peritoneum. Another peculiar feature was that the round ligaments could not be exposed, this again being due to the great transverse distension of the anterior wall. The uterus was incised longitudinally through the distended lower segment and upper cervix, and the child was removed in three minutes from the beginning of the operation, there being some delay owing to the anæsthetic. The child was extracted by the feet; but though quite fresh it had evidently been dead some hours, its cord being pulseless and discoloured. It was a well-developed female, weighing 5 lb. 11½ oz., and measuring 20½ in. in length. The uterus was then lifted, and was found to be held down by adhesions of the fibroid tumour in Douglas's pouch. The adhesions were easily separated. After the upper abdomen had been protected with gauze and the Cæsarean section wound had been temporarily stitched up, the whole uterus containing the placenta was removed by Doyen's method, and the peritoneum was completely closed by a silk purse-string suture. The tubes and ovaries were left behind. The abdomen was then flushed with salt solution, and the wound closed by through-and-through stitches of silkworm-gut and buried fascial stitches of silk. The cervix was found to be dilated to about the size of a shilling, and the tissues were so soft and gelatinous that the vaginal portion tore away while it was being pulled up by the vulsella. The uterus was removed in eleven minutes from the beginning of the operation. The whole operation lasted fifty-eight minutes; not much blood had been lost, and the patient's condition was good. The wound healed by first intention and the recovery was simple, the temperature only once reaching 100° F. during her stay in the hospital, which she left on the twenty-sixth day.

The uterus with the placenta weighed 5 lb. 12 oz. It was in the position of retroflexion caused by a myoma 12 cm. by 8 cm. by 9 cm., which was adherent in Douglas's pouch. This tumour was attached to the back of the upper part of the body of the uterus by a short pedicle 5 cm. broad by $3\frac{1}{2}$ cm. thick. There were numerous small myomata on the left side of the fundus and on the anterior wall. A median sagittal section was made after hardening with formalin (*see fig.*). The uterus is seen to be retroflexed almost to a right angle, the angle of the canal being a sharp spur formed by the posterior wall at the internal os. On the front wall is seen the Cæsarean section wound, now contracted to $7\frac{1}{2}$ cm. The peritoneum on the anterior wall is loosely attached as high as the upper extremity of the wound, which is 13 cm. above the external os, its lower extremity being $5\frac{1}{2}$ cm. above the external os and 1 cm. below the internal os. The distance from the external os to the middle of the fundus, measured over the surface of the *anterior* wall, is 34 cm.; the distance similarly measured over the *posterior* surface is 18 cm. This difference in the length of the anterior and posterior walls was, of course, enormously greater when the uterus was distended by the foetus and the liquor amnii. The anterior wall of the lower segment has been much stretched, being only 7 mm. thick. The anterior wall of the body also varies in thickness between 12 mm. and 15 mm.; the posterior wall is about 2 cm. thick. The anterior wall of the cervix is about 1 cm. thick, while the posterior wall is at its upper part more than 3 cm. thick. The placental site is on the posterior wall from 1 cm. above the internal os to just above the middle point of the fundus. The upper two-thirds of the placenta are detached, and a retroplacental clot fills up the space between the separated surface of the placenta and the uterine wall. The decidua had a greenish tinge.

Retroflexion of the uterus at term produced by an adherent fibromyoma is rare. Cases have been published by Herman¹ and Varnier and Delbet.² In Herman's case the patient was aged 36. There was an immovable tumour of the size of a cocoanut in the pelvis. The membranes had ruptured nine and a half hours before operation. The child was decomposing. Porro's operation was performed with the elastic ligature; the patient died in four hours. The thinness of the anterior wall was noticed and the spur at the angle of flexion. The elastic ligature was applied below the spur but above the tumour, which was left in the pelvis. The case was published fifteen years ago, in 1893.

¹ *New York Journ. of Gyn. and Obstet.*, 1893, iii, p. 484.

² *Ann. de Gyn., Par.*, 1897, xlvii, p. 102.

In Varnier and Delbet's case the patient was aged 32. There was an adherent fibro-myoma in the pelvis of the size of an orange. The child was alive when the patient was seen; but while she was waiting for the doctors to decide on the treatment to be adopted, labour came on and the child died before the operation (Cæsarean section followed by total abdominal hysterectomy). The mother recovered.

With regard to the diagnosis between adherent fibro-myoma and ovarian tumour, the presence of other tumours in the wall of the uterus (as in my case) will aid in the diagnosis; but I am disposed to attach considerable value to the bulging and thinning of the lower segment, which was noticed in all three of the above cases, as evidence that the tumour is a uterine fibroid and that the uterus is retroflexed. With regard to treatment, there should be no delay; as the result of labours having set in the child succumbed in all three cases before operation, although in Varnier and Delbet's case it was alive when the patient was first seen. Cæsarean section is necessary in these cases owing to the adhesions around the tumour, and when the child has been dead some hours and the membranes have ruptured, it will usually be wise to remove the uterus, especially when it is the seat of multiple tumours. In cases where the membranes were unruptured and there was no suspicion of infection, the tumour might be removed, leaving the uterus. Total abdominal hysterectomy appears to be the best operation for the removal of the uterus, having amongst other advantages a lower mortality than the supravaginal amputation.

The latest statistics with which I am acquainted dealing with the relative advantage of total hysterectomy and of supravaginal amputation as a complement of Cæsarean section in the case of fibroids complicating advanced pregnancy are those of Olshausen, in Veit's "*Handbuch*," 1907. They show fifty-two cases with five deaths for amputation, against twenty cases with one death for total hysterectomy. To these twenty cases may be added the present case and a case of mine published in 1906;¹ also a case recently published by Bucurra,² all three without mortality, giving a total of twenty-three cases with one death. Cases in labour and with the uterus retroflexed by an adherent fibroid offer features specially suitable for the total operation; so that, although the observed instances are few in number, we may infer that in their case, too, the total operation is to be preferred.

¹ *Trans. Obstet. Soc., Lond.*, (1906), 1907, xlviii, p. 240.

² *Centralbl. f. Gyn.*, Leipz., 1908, p. 1194.

DESCRIPTION OF FIGURE.

A SAGITTAL SECTION THROUGH THE UTERUS AND TUMOUR.

($\frac{2}{3}$ natural size.)

An arrow has been placed opposite the origin of the Fallopian tube, showing the position of the middle of the fundus and that the cavity of the uterus has been developed almost entirely by distension of the anterior wall.

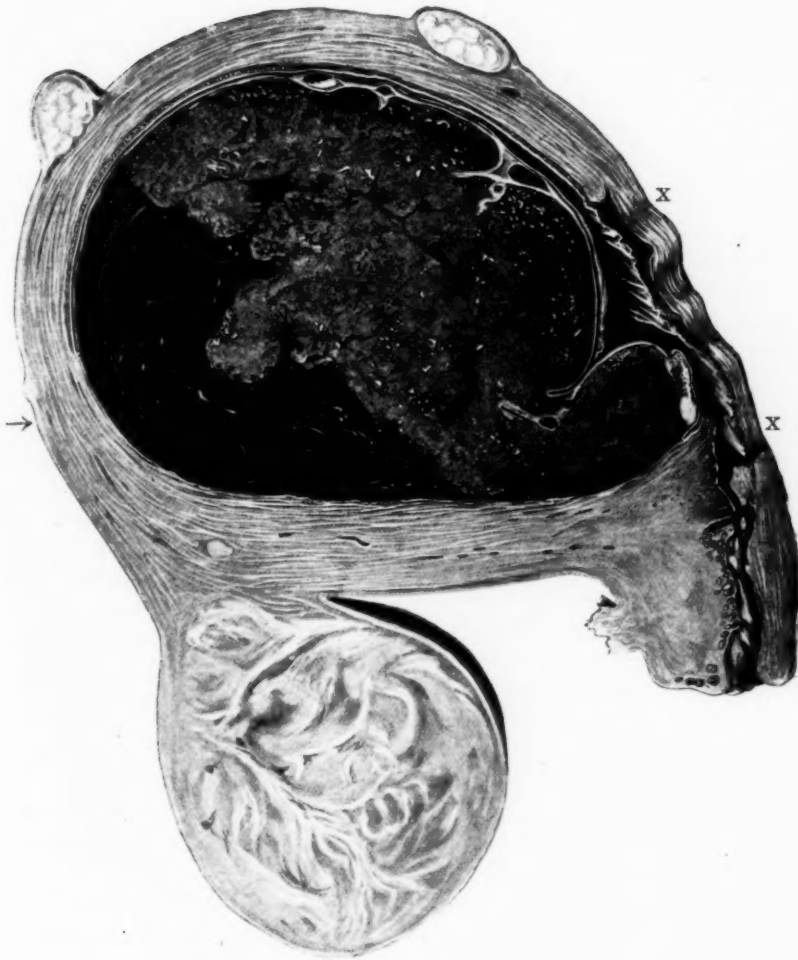
The uterus is retroflexed nearly to a right angle; during life, owing to the approximation of the cervix to the tumour produced by the vaginal attachment, the retroflexion angle was probably smaller; it is marked by a sharp spur formed by the posterior wall at the internal os.

The posterior wall is much thicker than the anterior; the lower segment of the anterior wall is especially thin, and was extremely thin and prominent when the uterus was distended by the fœtus and liquor amnii.

The Cæsarean section wound (X to X) has been made through the lower segment and the upper centimetre of the cervix.

The placental site is on the posterior wall to within 1 cm. of the internal os. The upper two-thirds of the placenta have been detached and the space between the detached portion of the organ and the uterine wall is occupied by clotted blood.

Attached to the posterior wall of the fundus is the fibroid, which was adherent in Douglas's pouch.



DR. HERBERT SPENCER'S CASE.

Sagittal Section through the Uterus and Tumour.

DISCUSSION.

Dr. AMAND ROUTH congratulated Dr. Spencer on his treatment of this case. He drew attention to the excellent result of the operation even after the patient had been so long in labour and had had to journey up from the country, and though the natural labour would have seemed hopeless if seen in the middle months of pregnancy, he thought it pointed to the advisability of avoiding operation till nearly or quite full term, when the foetus was viable. He thought Dr. Spencer's remarks on the diagnosis valuable. The case was clearly one of "spontaneous partial reposition" of a retroverted gravid uterus, as described by Barnes, complete reposition having been prevented by adhesions of the posterior fibroid. The thinning out of the anterior wall during this process of partial reposition was diagnostic of the condition, but was not easy to recognize before the abdomen was opened.

Dr. RIVERS POLLOCK said that he could add another successful case of Caesareo-hysterectomy in which the uterus was removed by supravaginal amputation. In his case the patient was aged 42, pregnant for the first time, and with a large subperitoneal fibroid filling up the pelvis and fixed by adhesions. He anticipated labour by about ten days, and delivered the patient of an 8 lb. male child. The mother and child both did well. The tumour was removed with the uterus.

**Notes of a Case of Sarcoma of the Cervix Uteri (? Epithelioma),
with Unusual Microscopical Appearances, in a Patient
aged 21.**

By C. E. PURSLOW, M.D.

THE patient, M. G., was born on October 12, 1885. She was sent to see me at the Queen's Hospital by Dr. Rowlands, of Bromsgrove, and was admitted on August 30, 1907. There was no history of cancer on either father's or mother's side. She was unmarried, but there had been parturition four years previously. Menstruation began when aged 12, was regular and not attended by pain. The patient had felt quite well until nine weeks before admission; since then she had noticed a pinkish discharge from the vagina. She had also felt occasional pain across the back and had complained that the stomach "had felt heavy, as if she had a lot of trouble on her mind."

On vaginal examination there was found to be a fungating growth extending round more than half the vaginal portion of the cervix on the right side. The uterus was freely movable and there was no sign of involvement of the vaginal walls or broad ligaments. The growth was friable and bled freely on digital examination. I diagnosed cancer of the cervix and performed vaginal hysterectomy two days later, first removing the growth with a Volkmann's spoon, as shown in the photograph. The patient made a good recovery.

The growth was submitted to Dr. Mackay, pathologist to the hospital, and he reported as follows: "The growth is an epithelioma. It is very vascular and appears to be of very rapid growth. An extraordinary feature is the large number of giant-cells."

Some of the sections were of so unusual an appearance that there was a difference of opinion as to the nature of the growth, and one pathologist expressed the opinion that the tissue was inflammatory.

Dr. Hewetson, Honorary Curator of the Gynæcological Department of the University Museum, very kindly offered to make a further investigation, and he drew up a very full report, which I will now read, and prepared the fine series of photomicrographs which I have the honour to bring before you this evening.

DR. HEWETSON'S REPORT.

Macroscopic Appearances.—The uterus is 3 in. by $1\frac{1}{4}$ in. by $1\frac{1}{4}$ in. The Fallopian tubes have been divided opposite the isthmic interstitial junction. The sound passes $2\frac{1}{2}$ in. into the uterus. The uterine body is normal in shape and appearance. The lips of the external os are everted, showing the rugæ of the cervical canal exposed. The fragments of the growth have been curetted away from the right half of the portio vaginalis, as indicated by a raw surface which exists on this part of the vaginal cervix. The remaining part of the cervix is covered with smooth vaginal mucous membrane. There is no growth visible to the naked eye upon the raw surface above mentioned, and when it is sectioned there appears to be no growth infiltrating the cervical tissue deep to it, and it is not friable. The fragments of growth which have been curetted from the right half of the cervix consist of soft, friable, cauliflower-like masses, varying in size from that of a pea to that of a large hazel-nut. They consist of a yellowish white centre or core from which numerous radiating low papillæ project. The surface layer of these papillæ is of a dull reddish brown colour suggestive of congestion or hæmorrhage. A mesial sagittal section of the uterus reveals a smooth endometrium of a dark reddish brown colour. The cavity of the uterus is quite empty and shows no evidence of retained gestation products. The uterine musculature is $\frac{5}{8}$ in. thick, it is firm and to all appearance normal.

Microscopical Appearances (Curetted Fragments).—These consist of solid masses of cells separated by a delicate stroma. There is an entire absence of columns, or acini, or cell-nests. The stroma consists of a fine fibro-cellular tissue running between the cells and richly supplied with blood-sinuses and capillaries, consisting of an endothelial lining, which often is placed upon the cellular growth which forms the tumour. These solid masses of tumour-cells show extensive areas of round-celled (polynuclear) infiltration, evidently migrated leucocytes (fig. 1). The surface of the curetted fragments is devoid of any epithelial covering and is composed of low papillary projections, composed of tumour-cells and inflammatory cells, amongst which is a network of engorged capillaries, many of which have given rise to hæmorrhages on the surface and into the tissue just below the surface of the growth. The tumour-cells are for the most part large rounded or oval cells containing a large vesicular nucleus, which retains nuclear stains very readily. Scattered in large numbers amongst these round or oval cells are large protoplasmic masses which contain many nuclei. These

resemble very closely the myeloid cells of a myeloma. There are, however, all stages between the mononuclear round-cell of the tumour and the multinuclear protoplasmic masses (fig. 2). Many cells contain two nuclei, others four, six, and eight; there is the same gradation in size of cells, so that it would appear evident that the protoplasmic multi-nucleated masses have merely arisen from division of the nuclei of



FIG. 1.

Showing cellular structure of growth—small round-cells and giant-cells, with areas of inflammatory round-celled infiltration (low power).

mononuclear round-cells without division of the protoplasm. There are numerous karyokinetic figures present, showing rapid division of cells. The deeper surface of these curetted fragments shows muscular tissue which has been invaded by growths, proving its malignancy. This growing edge is very abrupt—there is no infiltration by columns. One

peculiarity about it, however, is that the tumour-cells appear to be ranging themselves round capillaries, and there are many vessels which show one or two layers of cells around their lumina which are identical with the tumour-cells of the growth. When the raw surface of the portio vaginalis from which the growth has been curetted away is examined, very little growth has been left behind, showing that the tumour must have had very little infiltrative properties.

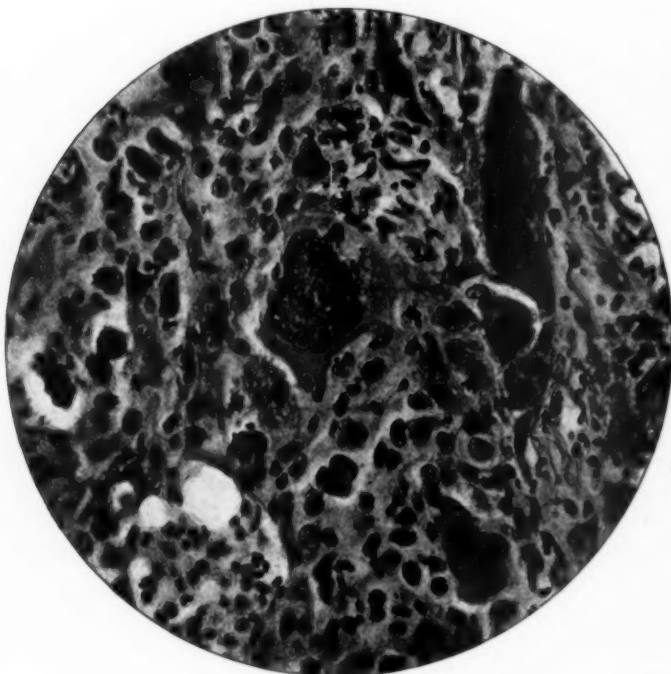


FIG. 2.

Showing types of multi-nucleated protoplasmic masses like myeloid cells (high power).

The stratified squamous epithelium of the vaginal cervix at the margin of the growth stops abruptly at the edge of the tumour and shows absolutely no evidence of infiltration (fig. 3). Similarly the epithelial covering of the cervical canal and of the deeper cervical glands is perfectly normal, and where it joins on to the tumour shows no evidence

of proliferation. The endometrium of the uterine body has been examined in several places and is everywhere perfectly normal.

Nature of the Tumour.—This is by no means an easy question to settle. I have only once seen a tumour which resembled it, and this proved to be a mixed-cell sarcoma of the suprarenal gland. The special feature about this tumour is the large size of the tumour-cells and the enormous number of protoplasmic masses. I have occasionally seen



FIG. 3.

Showing how the stratified squamous epithelium stops abruptly at the margin of the growth without proliferation (low power).

giant-cells in squamous-celled epitheliomata, but in these there were typical epithelial columns and cell-nests and no vascular stroma between the cells. The general arrangement of this growth, with its delicate stroma running between the tumour-cells, and its marked vascularity are all in favour of sarcoma and not of epithelioma. The absence of cell-nests,

infiltrating columns, and the lack of proliferation of the stratified squamous epithelium at the margin of the growths all tend to confirm this view. For similar reasons the absence of acini and the normal state of the cervical mucosa would go to exclude malignant adenoma of the cervical canal. In some respects the microscopical appearances of the tumour resemble those of chorionepithelioma. I have never, however, seen such perfect preservation of growth tissue, such masses of healthy tumour-cells aggregated together, without a large admixture of extravasated blood in chorionepithelioma. There is also little or no invasion of vessels with tumour-cells, such as is common in chorionepithelioma. There is also no evidence of syncytial deposits in relation to the endometrium. The growth appears to have originated in the portio vaginalis part of the cervix, and probably just below the stratified squamous epithelium of the right half of it, and to have fungated through the mucosa. The growing deeper margin suggests that it is progressing by a proliferation of cells in the endothelium of blood-vessels. It is probably, then, a form of endothelioma which is very malignant, and assuming the type of a mixed-celled sarcoma containing many myeloid cells.

The further history of the case is disappointing. I found on inquiry from Dr. Rowlands that the patient remained perfectly well and gained weight for three months after the operation. She then commenced to lose strength and weight and gradually got weaker, finally dying from exhaustion on September 28 of this year, a fortnight before her twenty-third birthday. Dr. Rowlands examined her on several occasions and found that there was no sign of recurrence in the scar or in the vaginal wall, but there were masses of growth to be felt in the pelvis, and, on palpation, above the pubes; there was no sign of involvement of the liver or any other viscera. A post-mortem could not be obtained.

Report of the Pathology Committee.—We have examined the specimen, together with microscopical sections, submitted by Dr. Purslow, and are of opinion that the growth is a giant-celled sarcoma arising in the cervix. Mr. Targett was of opinion that the reticulated mode of growth at the base of the neoplasm indicated an endothelial origin.

Uterine Fibroids.

Exhibited by W. D. SPANTON, F.R.C.S.

MRS. R., aged 33, a healthy woman, whom I was asked to see on October 7 on account of intestinal obstruction. I found she had been married about four months, was supposed to be about two and a half months pregnant, and occupying the whole of the lower abdomen and pelvis was a large multiple uterine myoma. There had been *complete* obstruction, no passage of flatus for eight days, with incessant vomiting, a rising temperature, and very rapid pulse. The abdomen was tense and tender to touch.



Uterine fibroids causing intestinal obstruction, the intestine being gripped in the sulcus between the two large tumours.

Enemata and aperients had been rather freely tried without any result; the rectum was empty. I therefore advised immediate operation. We found a large mass of myomata monopolizing the uterus, so that the uterine cavity was thin; it contained a two and half months

fœtus, which bulged the moment the base of the uterus was divided. The two principal tumours had evidently grown rather rapidly after marriage, though there was a history of their existence four years before, and in a sulcus between them (*see fig.*) the lower portion of the ileum was gripped firmly, the distal portion being empty, the proximal much distended and of a deep purple colour, but not gangrenous. There were flakes of lymph and a considerable quantity of turbid fluid in the peritoneal cavity. The uterus was removed, leaving only the cervix; but the pulse never picked up, and she seemed gradually to become weaker and died about thirty-six hours afterwards.

It is an instructive case as showing one grave source of danger during a period of what might appear otherwise a justifiable awaiting of events.

Parovarian Cyst with Intracystic Hæmorrhage from Torsion of the Fallopian Tube.

By ALBAN DORAN, F.R.C.S.

M. S., AGED 47, a laundress, applied to Dr. Drummond Maxwell, in the out-patient department of the Samaritan Free Hospital on June 19, 1908, on account of attacks of acute pain in the left iliac fossa to which she had been subject for some years. They were associated with bearing-down pains in the rectum. The last and worst attack began on June 6 and lasted for five days. A mass was detected in the left fornix, and she was admitted at once into my wards in the hospital. The patient had been married twenty-nine years and had borne fourteen children. The last confinement occurred in November, 1904. The periods were becoming irregular, the intervals during the past two years ranging from two weeks to six months. A scanty show had been seen a few weeks before admission. There was no history of any severe illness or bad puerperium. The child delivered in 1904 was born dead, but the labour and puerperium were uncomplicated. The temperature was normal, the pulse 84, regular, and of small volume. There were varicose veins in both legs extending upwards to the thighs. The patient was a strong, muscular woman. The abdominal walls were lax; no tumour could be detected above the pelvic brim. The uterus was retroverted, but easily reducible; its cavity measured $3\frac{1}{2}$ in. A mass occupied the left fornix, encroaching on Douglas's pouch. It was tense

and felt as though it were fixed to the adjacent structures; on rectal examination it was found to lie anterior to the bowel.

I operated on June 24, with the assistance of Dr. Vernon Monckton, Dr. Belfrage administering the anæsthetic. There were no adhesions above the brim of the pelvis. At first sight the uterus and appendages appeared normal in position and condition. I raised the right Fallopian tube and ovary, and found them free from adhesions. On raising the left appendages I saw that the mass lay below, connected with them. I passed my hand under the mass and it came up easily, being but lightly adherent to the peritoneum forming Douglas's pouch. Such is often the case with tumours and diseased appendages which feel fixed when examined before abdominal section. Some old blood issued from what I took at the time for a breach in its surface. Its pedicle was the inner part of the Fallopian tube, twisted three turns from left to right. The tube ran on to its surface, and the outer extremity of the left ovary, not involved in the torsion, abutted on the inner aspect of the mass. The tumour was a thick-walled cyst hardly as big as a tennis-ball; its surface was ochreous in colour. Owing to the relations, as above described, I had no difficulty in removing it, with the tube and ovary. The patient made a speedy recovery.

The parts removed were sent to the Royal College of Surgeons, and were examined by Mr. Shattock.

DESCRIPTION OF THE SPECIMEN.

The parts removed consisted of the tumour with the greater part of the left Fallopian tube and ovary. The tumour was made up of two oval cysts, very unequal in size, and separated from each other by a marked constriction. The larger and more external measured 3 in. in length by about $2\frac{1}{2}$ in. vertically and antero-posteriorly. The surface was perfectly smooth and of a dull yellow colour. The outer part of the Fallopian tube was stretched over the upper portion of the cyst; the ostium opened externally in the manner usually seen in "parovarian" cysts, and from the ostium the ovarian fimbria ran along the inferior aspect of the cyst towards the ovary. The inferior part of the cyst wall has been cut away, with a great portion of the ovarian fimbria, in order to expose the interior. The smaller and more internal cyst measured $\frac{3}{4}$ in. in length by $\frac{1}{2}$ in. in thickness. Its inner extremity touched the ovary; the Fallopian tube ran over its upper, and the ovarian extremity of the ovarian fimbria over its lower aspect.

On section, the wall of the larger cyst was found to consist of greatly thickened and intimately adherent mesosalpinx, of a deep red lamina, which was the proper wall thickened and infiltrated with blood, and of a distinct, perfectly smooth and transparent inner lining. The cavity of the cyst contained a recent blood-clot, mostly deep red, with paler parts, evidently fibrin from which the coloured corpuscles had disappeared. The clot did not adhere very firmly to the smooth lining membrane. The wall of the smaller cyst and its contents precisely resembled the wall and contents of the larger cyst. Their cavities did not communicate. The ovary was small and showed signs of atrophy. It was torn during removal, as its substance was friable. The middle portion of the Fallopian tube was greatly elongated and twisted, but its cavity was neither obstructed nor dilated.

The blood, which at the operation seemed to issue from the cyst, really came from the ostium. The cyst and the lumen of the tube, however, did not communicate.

REMARKS.

The swelling in the left fornix seemed as though firmly fixed when examined bimanually, yet in reality it was but lightly adherent. The same was the case in three instances where I recently removed a diseased tube and ovary. This fallacious appearance of apparent fixation of a tumour hardly adherent at all may lead us to suspect tubal pregnancy, as a tubal cyst soon becomes fixed. The irregular menstruation was in this case the forerunner of the menopause. When I raised the tumour, blood issued, as I thought, from a breach in its surface, but really from the ostium of the tube, so that until I had leisure to examine the parts removed very carefully, I fancied that the tumour might be a tubal sac, rupturing into the mesosalpinx, after all.

As a small cyst in the mesosalpinx is relatively heavy and very movable it is strange that a specimen like that which I exhibit is rather uncommon. A case was recently reported by Fabricius, of Vienna.¹ The tube was twisted and reduced to a thin string, the ovary was not involved and lay close to the uterus, which bore a fibro-myoma and was removed with the appendages. Pseudo-ileus followed; Fabricius suspected that he had carried infection from another case of axial rotation, where the cyst was ovarian and filled with putrid bloody fluid. The abdominal wound was opened again and the distended intestine

¹ *Zentralbl. f. Gyn.*, 1908, xxxii, p. 1192.

punctured. The patient ultimately recovered. This report ought to be borne in mind, as in most cases of operation for the removal of small parovarian cysts the patient fares well after the operation.

Several instructive reports of axial rotation of broad ligament cysts are to be found in the *Transactions of the Obstetrical Society of London*, one by Leith Napier (xxxiv, p. 124), a second by Arnold Lea (xxxix, p. 8), and a third by F. J. McCann (xlviii, p. 179). In the third, hæmorrhage occurred into the broad ligament. Freeland Barbour¹ two years ago read notes of a fatal case where blood was extravasated into the broad ligament and along the left ovarian, close up to the renal vein; the operation was performed when it was too late to save life. Axial rotation of a pedunculated ovarian cyst is rarely associated with so grave a complication. I may note that in my specimen, exhibited this evening, the ovary was not involved in the rotation. Twisting of the Fallopian tube alone, as described by Hamilton Bell, Malcolm, and others, is a complication of a different class.

Case of Very Early Ectopic Gestation with some Unusual Symptoms.

By H. MACNAUGHTON-JONES, M.D.

THE patient, who had been married three years, sought advice in October, 1897, for the relief of severe dysmenorrhœa, at the same time expressing her disappointment that she had not become pregnant. Some stenosis of the cervix uteri and a slight uterine discharge were the only detectable abnormalities. The uterine canal was dilated and the uterus curetted. Menstruation came on regularly and painlessly until March 6, 1908. A catamenial period was then due; for a week she had constant pain, but no menstrual discharge. The catamenia then appeared and the pain ceased. After a few days the flow almost disappeared, but for the following month there was a slight, painless, and nearly continuous discharge. On April 9 the patient passed a small body, which was destroyed by her husband (to save her anxiety). From his description it appears to have been a cast of the uterus. On examination a comparatively small tumour could be felt on the left side, quite distinguishable from the ovary.

¹ "Broad Ligament Cyst with Twisted Pedicle," *Journ. Obst. and Gyn. Brit. Emp.*, 1906, x p. 91.

After the passage of the body referred to, all discharge ceased for about ten days. It then reappeared, and continued on and off until May 9, when she again sought advice, complaining of pains in the groins and down the legs. For forty-eight hours she remained in bed, and all pain and discharge ceased. On May 20 she wished to leave London, as she was free from pain and discomfort. She was, however, advised not to travel.

Two days later she was seized with violent pain in the region of the gall-bladder and in the right shoulder and side, simulating an acute



FIG. 1.

Anterior view of sac, opened, with mesosalpinx and ovary. The early mole is seen in the centre of the sac surrounded by a darker area (natural size).

pleuritic attack or an attack of biliary colic. The pain was somewhat intermittent. There was no tenderness over the region to which the pain was referred, nor did pressure bring on a paroxysm; but the slightest manipulation internally at the seat of the tumour brought on violent pain referred to the shoulder and hypochondriac region. There were considerable restlessness and pallor, quite disproportionate to any symptoms of collapse which were present. A diagnosis of internal

hæmorrhage was made. I saw her for the second time and agreed with this view. On opening the abdomen a quantity of blood escaped, and I found the pelvis full of blood. I removed the tumour shown (*see* figs. 1 and 2). The patient had no untoward symptom in her recovery. The hæmatocele appeared to me to be associated with a very early gestation—possibly not more than one week to ten days. The body which was passed was probably a decidual cast. The symptoms were somewhat erratic and misleading. The ultimate symptoms were curious.



FIG. 2.

Posterior surface of sac, showing the small rent, the source of the pelvic hæmorrhage (natural size).

The reflex pain referred to the right hypochondrium and extending to the abdomen, at once excited or exaggerated by pressure on the *adnexal* swelling, was very unusual.

**Dermoid Cyst of the Ovary complicating Extreme
Displacement of the Kidney.**

Exhibited by H. MACNAUGHTON-JONES, M.D.

THIS specimen is exhibited not so much for its pathological significance as to show how a pelvic tumour may be overlooked through some other remote affection or abnormality being present which explains all the symptoms from which the patient suffers.

A lady, aged 39, consulted me; she had been an invalid for twenty years. She had been married several years and had never conceived. The more prominent symptoms she complained of were periodical attacks of severe dysmenorrhœa, chronic constipation, abdominal and, of late, suprarenal pain, with occasional dysuria and frequent micturition. She was emaciated and in wretched health, her weight being only 5 st. 2 lb. On palpation of the right renal region I failed to find the right kidney. The left was easily felt. On examination of the abdomen I at once came upon a freely movable tumour in the right inguinal region, the lower plane of which was level with Poupart. This proved to be the missing kidney. There being no symptoms pointing to the pelvis, and nothing discernible from an external examination, I came to the conclusion that the displaced kidney was the prime cause of her condition and recommended its anchorage, with a subsequent Weir-Mitchell course. Having anchored the enlarged kidney by the lumbar incision in the usual manner, and seeing how very bad her appearance was before operation, I was prompted to make a pelvic examination, and to my unpleasant surprise I discovered a small mass occupying the pouch of Douglas. I at once opened the abdomen and removed the tumour, which proved to be a benign sebaceous dermoid cyst of the left ovary containing hairs and greasy material. The two operations were completed in fifty-three minutes. She is now in excellent health and has put on some 16 lb. in weight since the operation in July last. My position would not have been a pleasant one had I not discovered the tumour at the time.

Case of Fibroids in Both Halves of a Bicornate Uterus.

By J. BLAND-SUTTON, F.R.C.S.

FIBROIDS arise in malformed uteri as well as in those of normal shape. The specimen, the subject of this communication, I removed from a spinster aged 32. She was admitted into the Chelsea Hospital for Women on a diagnosis of pelvic tumour, and it was quite easy to feel a lump in the hypogastrium. Vaginal examination was difficult on account of the extreme narrowness of what eventually proved to be the right vaginal passage. The mass was regarded as a cervix



FIG. 1.

The cornua of a bicornate uterus shown in section; each cornu contains an interstitial fibroid. A small transparent cyst hangs in the mesosalpinx near the oöomic ostium of the tube.

fibroid. As soon as the abdomen was opened, in the course of the operation, I recognized the bicornate condition of the uterus. Total hysterectomy was performed with conservation of the right ovary and Fallopian tube. Each cornu had a cervix and a corresponding vagina. The vaginae were separated from each other by a complete longitudinal septum. Each cornu is occupied by an interstitial fibroid (fig. 1).

The operation did not present any difficulty, but there is one point connected with it which might puzzle a surgeon should he stumble upon this malformation unexpectedly in the course of hysterectomy.

When the body of the uterus is bicornate the rectum lies in the middle line of the pelvis, and a median vertical fold of peritoneum, the *ligamentum vesico-rectale*, passes from its anterior aspect through the gap between the uterine cornua to become continuous with the peritoneum covering the posterior surface of the bladder. That portion of the vesico-rectal ligament which lies between the rectum and the neck of the uterus divides the recto-vaginal fossa into a right and a left half. This peritoneal ligament requires careful treatment, or the surgeon may accidentally open the rectum or the bladder. In closing the

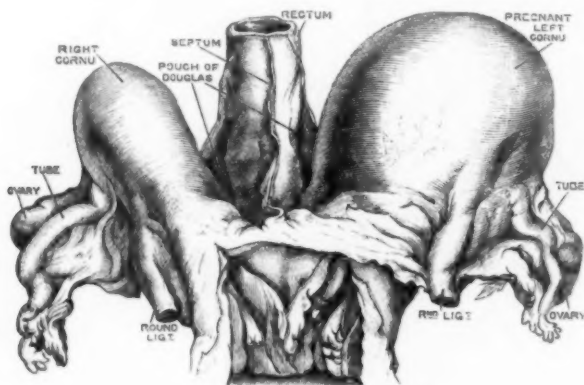


FIG. 2.

A bicornate uterus shortly after delivery; the *ligamentum vesico-rectale* is well shown.

peritoneum over the cervical stump it is sometimes necessary to bring the edges of the abnormal fold into apposition vertically by a continuous suture.

There is a matter concerning the clinical aspect of this specimen worth consideration. The patient's friends earnestly asked for her admission into the hospital because she had been seized suddenly with acute pain in the abdomen. I examined her shortly after admission and satisfied myself that the pelvic mass was a uterine fibroid. Since the operation I have come to the conclusion that the sudden acute pain

of which the patient complained was in all probability due to axial rotation of one cornu, for at the time of the operation we noticed that these twin cornua were as movable in the pelvis as two ovarian tumours with long and slender pedicles.

The *ligamentum vesico-rectale* has an interest apart from questions relating to technique, for it very probably stands in some embryological relationship to, and may even be responsible for, the production of a bicornate uterus. In addition to the specimen which forms the subject of this communication, I have removed a bicornate uterus by the abdominal route for cancer of the cervix. I attempted to extirpate it by the vaginal route, but in the early stages of the operation I recognized its bifid character, and, knowing the impossibility of removing it safely by this route, amputated the cervix. A year later there was some evidence of recurrence, so I removed the cornua by the abdominal method. Experience teaches that bicornate uteri cause more difficulties in diagnosis than in technique, but I think the presence of the vesico-rectal ligament would bar the removal of the uterus by the vaginal route. English literature contains few references to fibroids in malformed uteri; such as I have found are recorded in the subjoined list.

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Mr. ALBAN DORAN referred to a case published in the *British Medical Journal*, 1899, i, p. 1389, where he removed the undeveloped cornu of a uterus unicornis which was the seat of a small fibroid. There had already been two pregnancies in the well-developed cornu, which bore a child to term over one year after the operation. All forms of duplicity of the uterus had been ascribed by embryologists to premature development of the round ligaments, which interfered with the union of Müller's ducts. In the case he had mentioned, the round ligament connected with the undeveloped cornu formed a wide thick strap.

**Case of Unilateral Carcinoma of the Ovary, associated with
a Papillary Tumour of the Endometrium.**

By T. W. EDEN, M.D.

IN July, 1908, I was consulted by an unmarried lady, aged 56, who complained of her feet swelling and of being easily fatigued, and of some enlargement of the abdomen. She had been hill-climbing in the Tyrol until a fortnight previously, and had only come home because she felt unable to do any more climbing. She had had very little pain. On examining her abdomen I found it was entirely occupied by an elastic tumour, which extended in the middle line from the pubes up to a point about 2 in. below the ensiform cartilage. The upper part of the epigastrium and the outlying portions of the flanks were the only resonant areas. On internal examination I found an enlarged, softish, retroverted uterus, about the size of the closed fist, and quite independent of the tumour, the lower part of which could barely be felt. Her menstrual history was that for many months (she could not remember exactly how long) she had had continuous slight bleeding, which she regarded as due to the change of life. Previous to this the periods had been irregular and rather profuse, but she had never missed for more than two or three months.

At the operation the abdominal tumour proved to be a large semi-solid growth of the left ovary with a half-twist of the pedicle. I turned it out of the abdomen through a long incision, clamped the pedicle, and cut it away. On exploring the other organs I found the right ovary atrophied and the uterus enlarged symmetrically and of softish consistence. Fearing that it might contain a malignant growth I removed it along with the remaining appendages, making as free as possible a removal of the broad ligaments on both sides.

The ovarian tumour was a semi-solid growth containing many small, smooth, epithelium-lined cavities; it was unfortunately destroyed without having been weighed, but it was of large size and completely filled a wash-hand basin. On microscopic examination it proved to be carcinoma of the alveolar type, with an unusually large amount of stroma, which was oedematous and showed interstitial hæmorrhages and a good

deal of degeneration, indicated by feeble nuclear staining. The ovary and tube of the other side were examined microscopically, and the only abnormality found was a tubal diverticulum in the ampullary portion; there was no cancer. The carcinoma was therefore unilateral, and I am inclined to think that it was primary in the ovary; no trace of new growth could be found on palpation of the large intestine and the viscera in the upper abdominal zone during the operation.

The uterus was hardened in bulk, and then laid open in coronal section, so as to divide it into anterior and posterior halves. The cavity was then found to be full of a softish, pale-coloured new growth, arising from the endometrium. This growth consists of an aggregation of polypoid or villous masses, of varying size and irregular shape, arising from almost the whole endometrium. The cervical canal is dilated, and a mass of villous growth is found in it down to the level of the external os. The uterine wall is unusually thick for a nulliparous woman.

On microscopic examination the uterine growth consists for the most part of compound villous or papillary processes composed of a connective tissue stroma with an epithelial covering. The deeper parts consist of adenomatous tissue closely resembling that characteristic of glandular endometritis, and the development from this portion of branching papillary processes can be readily traced. The gland tubules in the deepest portion of the growth appear to penetrate the muscular layer to a somewhat greater extent than is normal, but there is nothing at all resembling malignant destructive infiltration. I therefore regard the uterine growth as innocent.

Report of Pathology Committee.—We have examined the specimen and sections exhibited by Dr. Eden, and are of opinion that the growth is a columnar-celled carcinoma of papillary type. Dr. Eden was of opinion that there was no definite microscopical evidence of malignancy in this growth.

Dr. AMAND ROUTH thought the intra-uterine growth was probably malignant, and referred to a case reported by him to the Obstetrical Society in 1897.¹ The patient, aged 57, suffered from metrorrhagia. The uterus was dilated and curetted on three occasions, the scrapings being reported by Dr. Tate and Mr. Targett to be benign villous papilloma. Eventually, two and a half years after the first curetting, he removed the uterus by vaginal hysterectomy, and the

¹ *Trans. Obstet. Soc., Lond.* (1897), 1898, xxxix, p. 5.

specimen was reported upon by Mr. Targett, who stated that the papillomatous growth had deeply invaded the uterine muscle, at one spot having almost reached its peritoneal covering. This proved that the papilloma, benign in its microscopical structure and retaining its papillomatous type, had developed malignant characters in its methods of growth, and would almost certainly have spread over the peritoneum, as happened in cases of ovarian papilloma, if the operation had been long delayed. The patient was still alive and fairly well.

Obstetrical and Gynæcological Section.

December 10, 1908.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

Hysterectomy for Large Cervical Fibroid during Pregnancy.

By J. H. DAUBER, M.B.

THE occurrence of pregnancy in a large fibroid uterus is at all times an anxious complication. Especially is this the case when the fibroid is of the cervical variety, firmly fixed in the pelvis, practically blocking it. Then escape from the uterus by the normal route, should the fœtus attain to any size, is entirely cut off. Cæsarean section is the only alternative. If, however, the fibroid fills a large portion of the abdominal cavity as well as the entire pelvis, space is wanting for the fœtus to grow. Generally, in such cases, alarming symptoms supervene before very long, necessitating surgical interference.

In cases of large cervical fibroid with pregnancy the expectant treatment has nothing to recommend it. Cæsarean section at full term offers no hope of saving both mother and child, for the fœtus has not room in which to expand; every week's delay adds to the difficulties and dangers. Exploratory cœliotomy becomes imperative. If myomectomy—the ideal treatment for such cases—can be performed, and the uterus with its living contents saved, one of the greatest of modern surgical triumphs has been won, and the successful operator has every right to feel proud of his achievement. But this happy solution of the difficulty is not always obtainable.

It is not possible to lay down any hard and fast rules of procedure for these cases; no two are identical—each must be carefully considered in all its aspects—but it may be taken as a guiding principle that where the prospect of saving a living and livable fœtus is remote it is not good surgery to defer for a moment longer than is necessary the taking of measures for preserving the life of the mother by timely operation.

The case now shown is illustrative of this view, and it will be contrasted with a somewhat similar but less urgent case that occurred some time previously. H., aged 43, single, a housekeeper, was brought to the out-patient department at the Hospital for Women, Soho, by her doctor on July 20. She had been attended by him for a few days previously at her own home, during which time she had suffered from acute abdominal pain and tenderness with constant vomiting. The temperature meanwhile ranged as high as 104° F., the pulse 120. She had complained of abdominal pain for a year previously. Her symptoms had been much accentuated during the past month, until at last she felt so ill that she was compelled to seek medical advice. The catamenia had been irregular for twelve months, the loss excessive with clots. The last menstruation was in March. The breasts contained secretion, and she stated that she had become larger of late. Bimanual examination revealed a large irregular tumour, immovably fixed in the pelvis and rising into the abdomen to above the umbilicus. There was a softish area in the upper part of the tumour on the left. A diagnosis of fibroid disease was made, and in view of the four months' amenorrhoea, the accompaniment of pregnancy was strongly suspected, although it was thought possible that some secondary circulatory or other degenerative or infective change might be present by reason of the abdominal pain, vomiting, and high temperature. Dr. Dauber detained the patient in hospital and operated within forty-eight hours.

The fact that the same doctor had some time previously brought to him at the out-patient department a patient in the early months of pregnancy with a fibroid uterus, in whom he had advocated the expectant line of treatment, with unfortunate results, determined him to run no risks of delay in this instance. In the former case, as the uterus was not fixed and the fibroids did not entirely block the pelvis, and in the absence of constitutional symptoms, he had suggested that the patient should merely be kept under close observation. This advice, however, proved disastrous for her, for about two months afterwards she was suddenly seized with violent abdominal pain, rise of temperature, and symptoms of acute peritonitis. As she lived adjacent to a general hospital she was quickly transferred there and operated upon without loss of time, but soon afterwards succumbed. This unfortunate occurrence deeply impressed both the patient's doctor and himself.

The operation itself in Miss H.'s case calls for little comment. The tumour was of the cervical type, burrowing into the broad ligaments, with its lower segment firmly wedged in the pelvis; the hæmorrhage

from the capsule was profuse, as is usual where pregnancy accompanies fibroid disease. Subtotal hysterectomy was performed. Recovery was uninterrupted and complete, and the patient left the hospital within twenty-eight days. At the present time she is walking about and performing her duties, and states that she is in perfect health. The other patient, where the expectant line of treatment was adopted, is in her grave. Such different results compel reflection.

The PRESIDENT (Dr. Herbert Spencer) said no one would doubt that operation was required in this case; whether it should have been done at once or when the child was viable depended upon the symptoms present.

Necrobiotic Interstitial Fibroid Removed from a Patient Three Months Pregnant without interruption of Gestation.

Shown by JOHN S. FAIRBAIRN, M.B.

THE chief interest in this specimen lies in the fact that it occupied the entire anterior wall of the uterus, all of which, except a thin layer, was removed with the tumour without disturbing the pregnancy, though the patient at the time of operation had signs of threatened abortion.

The history is as follows: The patient was admitted to St. Thomas's Hospital on April 29, 1908. She was a married woman aged 34, who had had one child seventeen years ago and no other pregnancy till the present one. Menstruation had always been regular; the last period began on January 26 and ended on February 2, 1908. She had had no symptoms up to within six days of her admission. On Thursday, April 23, whilst at work, she had an attack of pain in the abdomen and back with slight uterine hæmorrhage, but not severe enough to make her give up work. That night, however, severe attacks of pain occurred, colicky in character, radiating all over the abdomen and back, but most severe, and apparently originating, in the left iliac fossa. The uterine hæmorrhage continued, though it was never profuse. As the pain, though varying in severity, continued and quite prevented her from working, she came up to the hospital for advice and was admitted. She was a stout woman and did not appear ill, though she could not move in bed or lie on her left side without pain. A rounded tumour was felt in the abdomen, reaching to just above the level of the umbilicus; it was movable, elastic, and very tender, especially on the left side.

Vaginal examination showed that the tumour was uterine and that the posterior and inferior part was softer and more cystic than the upper part. Some bleeding from the uterus was noted during examination; it was slight in amount and dark in colour. The tumour was thought to be a uterine fibroid undergoing degenerative change, and its removal was advised. As the patient had not had a child for seventeen years and was anxious to go to term, myomectomy was considered the operation of choice, and this was kept in view when the abdomen was opened on April 30. When the tumour was brought outside the abdominal incision, it was found that the fibroid formed by far the larger part of the mass, the pregnancy being the soft cystic portion felt behind and below on vaginal examination. At a first glance it did not seem that myomectomy was likely to be successful, as the tumour and the uterus were very closely incorporated, and one or two other small fibroid nodules could be felt. It was, however, decided to make the attempt, so a circular incision was made round the tumour and the fibroid enucleated without opening into the sac of the ovum, though the finger was working so close that it felt as if it might go through any moment. During this part of the operation there was very profuse venous bleeding, which was controlled by pressure with gauze, while two other small fibroids were enucleated. The bed of the tumour which formed the whole anterior surface of the pregnancy was then under-run with catgut and the raw surface closed by bringing together the edges of the uterine incision in the middle line, care being taken not to pass the sutures deep enough to wound the ovum. The cavities where the small tumours had been shelled out were closed in a like manner. A third small pedunculated fibroid was removed by ligaturing the pedicle and sewing over the raw surface.

The tumour weighed 1.150 kilos.; the area of anterior uterine wall removed measured $4\frac{1}{2}$ in. by 5 in.; its thickness was difficult to measure accurately owing to the oblique section, but was about $\frac{3}{8}$ in. On section the tumour had the characteristic raw-meat appearance and stale odour of a necrobiotic fibroid.

During the first twelve hours after the operation the patient had three hypodermic injections of morphia, $\frac{1}{4}$ gr., in order to minimize the risk of abortion. Convalescence was without incident; the uterine hæmorrhage persisted for about a week, but gradually diminished. There was no pain. As a precaution, potassium bromide in 10 gr. doses was given three times a day in a mixture. The patient was kept in bed a little longer than usual, and when she left the hospital on May 24 an

increase in the size of the uterus was evident. The pregnancy ran a normal course, and she was delivered of a healthy boy of 9 lb. 3 oz. weight in the General Lying-in Hospital on November 9. The labour was somewhat protracted (sixteen hours), as the occiput was posterior. The uterine action was good, and only low forceps was required after manual rotation of the head. Otherwise there was nothing unusual or noteworthy in the labour. The puerperium was normal.

There are several points of interest in this case. First of all the tumour shows the typical appearance of fleshy necrobiotic degeneration which occurs frequently in association with pregnancy. The acute nature of the process is also shown by the fact that the patient had no symptoms and was unaware of the presence of the tumour until, in the third month of her pregnancy, she was seized somewhat suddenly with abdominal pain, which soon became quite incapacitating in its severity. Bland-Sutton has pointed out that the pain which often accompanies this change may be so severe and so sudden in onset as to give rise to the wrong diagnosis of ovarian cyst with axial rotation of the pedicle.¹ In the nineteen cases of this form of degeneration which I investigated² pain was present in sixteen cases, and in eleven of these was severe and was the chief reason for the patients seeking advice. The chief point of interest seems to me to be in the presence of premonitory symptoms of abortion in the shape of uterine hæmorrhage and in that, although the tumour was interstitial and very closely incorporated with the uterus, the result of its removal and the consequent complete rest in bed was the gradual disappearance of the symptoms of abortion. Further, the removal of a large area of the anterior surface of the uterus and nearly the whole thickness of the anterior wall did not interfere in any way with the development of the pregnancy or with labour. Bland-Sutton, in the paper already mentioned, calls attention to the way in which the pregnant uterus will tolerate surgical operations, and gives a table of eight cases of myomectomy during pregnancy in which the seven patients who recovered all went to term. These were, however, cases of subserous fibroids. In Thumin's series³ twenty-one aborted out of ninety-two non-fatal cases. No more striking case of the kind could be quoted than one of Mackenrodt's,⁴ in which the uterine wall was so far torn through that the decidua was exposed and yet the

¹ *Lancet*, 1901, i, p. 452.

² *Journ. of Obstet. and Gyn. of the Brit. Emp.*, 1903, iv, p. 118.

³ *Archiv f. Gyn.*, lxiv, p. 457.

⁴ *Zeitschr. f. Geburts. u. Gyn.*, xxxi, p. 452.

pregnancy went to term. The result in the case here recorded and the figures quoted are certainly very encouraging for the conservative treatment of fibroids in pregnancy.

DISCUSSION.

Mrs. BOYD congratulated Dr. Fairbairn on the courage and success of his treatment. She inquired how far pain and tenderness appearing in a fibroid complicating pregnancy could be regarded as diagnostic of necrobiotic change. She had recently under her care a case of pregnancy in a uterus with fibroids, where marked pain and tenderness in the tumours recurred on several distinct occasions, giving rise to a suspicion of this degeneration, but the pregnancy terminated satisfactorily at term, and the uterus and fibroids appeared to be undergoing normal involution.

Dr. COMYNS BERKELEY said necrobiotic changes in fibro-myomata complicating pregnancy could be diagnosed by the symptoms of pain and tenderness. At any rate this had been his experience in five cases of this nature upon which he had operated.

Mr. BLAND-SUTTON remarked that the painfulness and tenderness of uterine fibroids in a state of red degeneration when complicating pregnancy were so characteristic that the condition could be diagnosed with the same amount of accuracy as other pathological conditions in the pelvis. He had assured himself that in cases where these symptoms were of a mild or moderate type they would disappear if the patient were kept at rest in bed. Red degeneration, even in an extreme degree, in a fibroid growing in a non-gravid uterus was rarely associated with pain, and it was important to remember that a fibroid in a state of red degeneration, even in a gravid uterus, was often neither tender nor painful. It was also curious to see a gravid uterus containing four or five fibroids the size of large potatoes, and only one would exhibit this remarkable red degeneration and cause great pain, whilst the remainder would be as insensitive as apples.

Dr. MACNAUGHTON-JONES said that he had seen several cases of necrobiotic degeneration of myomata in which pain was not a characteristic symptom. He did not think that the presence of pain could be looked upon as of special diagnostic value.

Dr. DRUMMOND ROBINSON asked why so large a portion of the anterior uterine wall had been removed, and suggested that the tumour might have been enucleated through a simple linear incision. With reference to the remarks of some speakers that pain and tenderness together were almost certain proof of degenerative changes in a fibroid situated in the walls of a gravid uterus, he pointed out that these symptoms (pain and tenderness) were sometimes marked in a gravid uterus that was perfectly healthy. He had not long ago carefully watched such a case for some time.

Dr. AMAND ROUTH had been accustomed to rely upon the temperature being raised in all true cases of necrobiosis of fibroids as a help to the diagnosis of that form of degeneration.

Dr. FAIRBAIRN, in reply to Dr. Drummond Robinson, said that no doubt it would have been possible to have enucleated the tumour by simply incising the anterior wall, but by making an incision round the tumour the amount of enucleation was lessened, and therefore also the manipulation of the pregnant uterus. It is not easy to calculate exactly the retraction of the uterine wall, but if the tumour is examined it is seen that only the outermost quarter is covered with uterine wall, and the remaining three-quarters from the surface of the growth as enucleated. As the pregnancy was only three months advanced, this is about the proportion of redundant wall which would be left in the case of a tumour of that size occupying one wall of the uterus. In regard to what Mrs. Stanley Boyd and Mr. Bland-Sutton had said about pain in necrobiotic fibroids during pregnancy, he would refer them to the statistics he had quoted from a former paper on this degenerative change. He considered Mr. Bland-Sutton's statement a little too sweeping, for though pain was frequent and often the marked symptom in such cases, it was sometimes absent, and also it might occur in fibroids during pregnancy without any degeneration being present. At the same time, he would agree that the diagnosis of necrobiotic change would prove correct in the majority of cases where pain, tenderness, and softening of the tumour were present in fibroids in a pregnant uterus.

**A Fatal Case of Accidental Hæmorrhage, partly concealed,
with Intra-peritoneal Bleeding from both Fallopian Tubes.**

By W. D. KEYWORTH, B.C.

THE patient, M. B., aged 34, had had seven normal labours, the last one being three years ago. On September 23, at 3.30 p.m., when about thirty-eight weeks pregnant, she was engaged with the ordinary housework—which was not of an arduous nature—when she found that she was passing blood *per vaginam*. There was a definite history of a fright three days before, but otherwise she could suggest nothing to account for the hæmorrhage. There was no evidence of a hæmophilic tendency. After two hours' hæmorrhage, feeling faint, she lay down and sent for the midwife. The latter saw the patient and at once communicated with me. During this time there had been no pain whatever, and on my arrival, at 6.30 p.m., I found that probably 20 oz. of blood had been lost externally, and the condition of the patient was as follows: She was blanched, but not extremely so, and the pulse was 120, regular and of fair quality. Blood was trickling steadily from the vagina. The

uterus was the size of full term and was hard, tense, and prominent. The foetal parts could not be made out, except that just above the pelvic brim the head could be felt on firm pressure. The foetal heart was well heard in the left hypogastrium, and the rate was 130 per minute. There was no abdominal pain and only slight tenderness. On vaginal examination the cervical canal was found to be taken up, the os to be about the size of a shilling, and its edge rigid. The head was well engaged, and no placental tissue could be reached.

The case was recognized as one of accidental hæmorrhage, partly concealed, partly evident, and the patient removed to the York Road Lying-in Hospital as quickly as possible. As a preliminary measure the vagina was plugged with gauze as thoroughly as could be done without an anæsthetic, and a tight binder was applied. Shortly after arrival at hospital fresh bleeding appeared past the plug, which had become somewhat loosened. Accordingly the remainder of the gauze was withdrawn and, under chloroform, the membranes were ruptured digitally, about 25 oz. of liquor amnii escaping with a few clots, which were probably washed out of the vagina. Otherwise the liquor was only tinged a pink colour. The tension of the uterus was somewhat but not greatly relieved. The condition of the patient had now definitely changed for the worse and was typical of that found in severe hæmorrhage. Dr. Fairbairn was sent for, and on his arrival he firmly repacked the vagina under anæsthesia with 11 yards of 6 in. sterile gauze and had a tight binder with perineal straps applied. During the manipulations a few abrasions were produced on the inner aspect of the labia by the pressure of the speculum and by the introduction of the gauze. Aseptic ergot 20 minims and morphia $\frac{1}{3}$ gr. were given hypodermically before the patient had recovered from the anæsthetic. The condition of the patient was now unchanged. The pulse-rate was 132, and there was complete absence of pains. This was at 9.30 p.m. Nothing of importance happened for five hours. During that time the patient had had a dose of ext. ergot. liq. by mouth, strychnine $\frac{1}{20}$ gr. hypodermically, and 1 pint normal salt solution *per rectum*. There was no evidence of further hæmorrhage, and no labour pains had appeared. At 2.40 a.m. strong bearing-down pains came on. These succeeded each other every two minutes; the gauze was driven down and partially expelled, and the head soon showed at the vulva. The hæmorrhage at this stage was slight and probably did not exceed 3 oz.; there was certainly no great loss such as would suggest the escape of a large concealed hæmorrhage. At 3.10 a.m. (*i.e.*, half an hour after the first pain) the patient was delivered of a stillborn child, and ten minutes

later the placenta was born. A vaginal douche was administered, but as the uterus remained bulky, with some hæmorrhage, after an injection of ergot aseptic, an intra-uterine douche at 118° F. was given; good contraction followed, and the post-partum hæmorrhage, which had amounted in all to 18 oz., now ceased; 1 pint of normal salt solution was also given *per rectum*. The placenta was circular with lateral cord insertion, and divided sharply on its maternal surface into two almost equal portions, the one firm and having the characteristic pale and anæmic appearance of a normally separated placenta, the other thinner, more flabby, and covered with old blood-clots. This latter was clearly the portion from which the ante-partum hæmorrhage had taken place, but there was no evidence to show whether it had been situated above or below the other portion when in situ or that it had been attached to the lower segment of the uterus. The patient had been collapsed after delivery, but her condition was never such as to give rise to great anxiety. The intra-uterine douche had been given rather as a prophylactic against further hæmorrhage, as we knew she could not afford to lose much. She gradually improved under the above treatment and appeared to be doing well. Three hours later her condition was found to have changed for the worse, the change apparently extending over the last half-hour. The pulse had become 150 and was very irregular and of poor quality. The patient was restless and was again losing *per vaginam*, but only slightly. The uterus was well contracted, and there did not appear to be sufficient external hæmorrhage to account for the change in her condition. The right median basilic vein was therefore opened and infusion of normal saline solution practised. As, after 3 pints had been administered in about one hour, there was little improvement, attention was again directed to the uterus. It was bulky and fairly well contracted; a slow but steady stream was found, however, to be issuing from the vagina. The abrasions previously mentioned on the labia minora were oozing a pale watery stream, the blood apparently showing no tendency to clot. These were picked up with stitches after hot water applications had been tried. As blood was still flowing from above and no lacerations of cervix or vagina could be found, it was decided to plug the uterus. This was done with as little disturbance as possible, the patient still lying on her back in bed with the thighs separated. The intravenous infusion was meanwhile continued, 5 pints in all being administered. As far as could be seen the bleeding was checked, but in spite of free stimulation the condition gradually became worse, and the patient died at 10 a.m., *i.e.*, eighteen and a half hours after the first external bleeding had been

noticed, about seven hours after delivery, and one and a half hours after the uterine plugging.

At the post-mortem, thirty hours after death, the condition was as follows: Body of well-nourished adult woman; marked universal pallor; rigor mortis present; no post-mortem staining. Examination of thorax negative, except for one small hæmorrhage on the convexity of the diaphragm, and a second on the visceral pericardium; heart substance pale and friable. Abdomen: 15 oz. blood, partly free, partly clotted, were sponged out of the peritoneal cavity about the uterus, mostly on the left side; some of the blood lay in the pelvis, some in the iliac fossæ; as regards colour, it was not noticed to be particularly pale; the significance of this will be seen later. Uterus: External appearance—length 8 in., weight 42 oz. (with vagina); irregular subperitoneal “bruising,” chiefly about the cornua, and a larger extravasation of blood into the tissues of the left broad ligament, mainly visible on the anterior aspect. In no case did this “bruising” appear to form a hæmorrhage of any magnitude. Nowhere could any solution of continuity of the peritoneal surface be found. Appendages: Both tubes evidently contained blood, but except for the ampullæ could not be said to be distended with it. The diameter was uniformly about $\frac{1}{2}$ in. The ampulla of the left tube was distended with blood and formed a swelling the size of a walnut, that on the right, smaller, size of filbert. On both sides small fragments of blood-clot adhered to the fimbriated extremity, and blood could be expressed from the left ampulla by gentle pressure. Neither tube was opened. A section taken from about the middle of the right tube shows hæmorrhage in the wall of the tube, but none in the lumen. An incision was now made through the anterior uterine and vaginal walls showing the uterine plug in situ; $3\frac{1}{2}$ ft. of wide gauze were lying well up to the fundus and stained only to a pink colour. There was no free blood in the uterus and only one or two small clots. The uterus appeared to be well contracted on the gauze, which had left its pattern on the uterine wall where it had been in contact with it. The placental site was seen to correspond to the anterior surface, extending up to the fundus, and did not involve the lower segment; no undue patency of the uterine ostia was observed.

The interest of the case of the specimen I show lies in the occurrence of intra-peritoneal bleeding from the Fallopian tubes in the course of a case of accidental hæmorrhage. It is not easy to determine exactly when the bleeding took place, but judging from the clinical signs it is most likely that it occurred shortly before death and accounted for the final collapse.

There had been no serious flooding after delivery, but the patient had been extremely exhausted. This was natural considering the amount of ante-partum hæmorrhage, estimated at 40 oz. Further, she improved during the two or three hours following delivery. I suggest that it was with this improvement in her pulse, which was part of her general improvement, that the intra-peritoneal bleeding occurred. The external hæmorrhage was not, I think, sufficient to account for the change for the worse which then took place; though this internal hæmorrhage was only about 15 oz., it was sufficient to turn the scale.

It might be suggested that the blood was in some way driven back through the tubes as the result of the gauze plugging in the uterus. Against this we have the following facts: the uterus was not plugged until about 3 pints of saline solution had been infused. The blood which was oozing from the labial lacerations was extremely pale and watery; presumably this watery condition was due largely to the free infusion, and was some index of the condition of the blood throughout the body; it has been already stated, in the post-mortem examination, that the intra-peritoneal blood was not particularly pale, so I think we may take it as probable that the internal bleeding took place before the patient had been infused to any extent.

An examination of the specimen will, I think, show that the bleeding came from the Fallopian tubes. Unfortunately, in the hardened state the tubes have shrunk and there is little to be seen except the distension of the ampulla of the left tube, in which, as I have said, the changes were most marked. In their present state no one would say that the tubes contained blood along their whole length, either in the wall or in the lumen. The uterus was intact, and there was no solution of continuity of the peritoneal coat. Blood dripped from the open ends of both tubes, and the ampullæ were distended, more especially the left. The distribution of blood agreed with these findings, being for the most part on the left side.

The most difficult problem of all is to explain where the blood in the peritoneal cavity came from—whether it was (1) primarily from the tubes, or (2) primarily from the uterus, and reaching the peritoneal cavity *viâ* the tubes. The tubes have not yet been cut open, so no attempt has been made to trace the course of the blood. A small section has, however, been removed from the right tube for microscopic examination. Against tubal bleeding we have the fact that there was no history of hæmophilia or other cause of spontaneous bleeding; moreover, the source of the external bleeding is known—the appearance of

the placenta is sufficient evidence of this, if any were necessary—and it seems unnecessary to assume a second source for the internal bleeding, except with adequate cause. It is true that at the one spot examined the blood is in the wall of the tube. This is in favour of the tube being the source of bleeding. Against the uterine bleeding via the tubes we have the physical difficulty. Why should blood pass by what is practically a capillary channel, while the natural exit is widely patent? I have already pointed out why, apart from the then serious condition of the patient, it is unlikely that the bleeding occurred internally, after the patency of that exit had been interfered with by the gauze plug *in utero*.

I need hardly say, after labour had been satisfactorily terminated, I thought I had good reason to expect complete recovery. The case is extremely unusual and very difficult, and at Dr. Fairbairn's suggestion I am willing to submit it to the Pathology Committee for future investigation.

I cannot close without making the obvious suggestion as to treatment. I suggest that the possibility of this intra-peritoneal complication after plugging is an argument in favour of some form of Cæsarean section in cases of concealed accidental hæmorrhage. I am indebted to Dr. Fairbairn for permission to use the notes of this case.

Report of the Pathology Committee.—On dissection, very extensive extravasation of blood was found in both broad ligaments, especially in the left, extending from the level of the internal os up to the fundus, through the meso-salpinx and around the tube, the only part free being the central portion of the anterior and posterior uterine walls and of the fundus. Sections of the tubes show that the extravasation is entirely outside the lumen, the lumen being free from blood-discs, and undilated. We have not found any breach of surface or the actual source of the hæmorrhage, but are of opinion that the whole of the hæmorrhage was due to the pressure exerted on the uterus whilst the vagina was tightly packed.

Dr. FAIRBAIRN said he was anxious this specimen should be shown at the Section, as he would like to hear if any Fellows had met with or knew of the occurrence of intra-peritoneal bleeding in the course of accidental hæmorrhage. He had not been able to find any record of a case of the kind. The cause of it was very difficult to explain. He could not believe it came from the uterus, as it was difficult to see why blood should take this track when the much easier exit through the open cervical canal was available. The plugging of the uterus might be thought to have blocked this, but it must be remembered that

the plugging was done after the signs of collapse had shown themselves, and therefore after the internal bleeding had occurred. The blood was dripping from the open ends of both Fallopian tubes, and a clot projected from one of them, so it evidently came from there; but the microscope seemed to show that the bleeding began in the wall of the tubes. He would be glad if the Pathology Committee would investigate this very unusual case.

On a Case of Diffuse Adeno-myoma of the Uterus.

By J. BLAND-SUTTON, F.R.C.S.

A SPINSTER, aged 43, came under my care in the Chelsea Hospital for Women on account of profuse and long-continued menorrhagia associated with an enlarged uterus, the fundus of which reached well above the pubes. The menorrhagia and the increase in the size of the uterus were attributed to the presence of a submucous fibroid, and on this diagnosis hysterectomy was undertaken. The uterus completely filled the pelvis, and at the time of the operation measured 40 cm. in circumference. When the organ was exposed through the abdominal incision in the course of the operation I was struck by its peculiar vivid redness, and the fundus presented a number of short villous tufts of organized lymph.

As soon as the operation was completed I divided the uterus, and on exposing its cut surface at once realized that the enlargement of the organ depended on changes in the endometrium and in all probability on what is known as diffuse adeno-myomatous disease. This view was confirmed by a subsequent microscopic examination of the carefully hardened tissues. A complete sagittal section of the uterus is represented in the accompanying figure drawn to scale, from which it will be seen that the walls of the uterus are thickened in a fairly uniform manner, but when the cut surface is critically examined the new material can be distinguished from the strictly parietal or muscular tissue of the uterus, and although it is unencapsuled there is nevertheless such a marked distinction between this adventitious and the true tissues of the uterus that the naked eye can fairly well define its limits. The whole of this adventitious mass is made up of dense strands of myomatous and connective tissue, which is permeated by chinks filled in with the well-known cytogenic connective tissue matrix of the endometrium in which

glands are embedded, lined with columnar epithelium identical with, and indeed derived from, the tubular glands which normally occupy the endometrium lining the cavity of the uterus.

I have met with fifteen examples of diffuse adeno-myomatous disease of the uterus in the last five years, but this is the largest specimen which has come under my notice. This uterus has an average diameter of 12.5 cm. (5 in.). The disease occurs with fair frequency, but it is commonly overlooked because careful microscopic examination is necessary for its identification. Fortunately the disease gives good results immediate and remote to hysterectomy. When the uterus is enlarged,



Uterus in sagittal section showing diffuse adenomatous disease. The polypoid process projecting into the uterine cavity contains glands. From a spinster aged 43.

as in the specimen the subject of these remarks, it is necessary to remove it by the abdominal route; and then subtotal hysterectomy, with conservation of one ovary, meets all the requirements of the case. In regard to the present case the operation was performed in twelve minutes, and the patient left the hospital convalescent twelve days after the operation.

DISCUSSION.

Dr. MACNAUGHTON-JONES said that he regarded diffuse adenoma as one of the most interesting of uterine tumours. It was so from its histological, pathological, and clinical aspects. Histologically, from the doubtful embryological relations of its structure; pathologically, from its relation to the muscular elements, the uterine mucosa, and its origin in the endometrium; and clinically, through the severe hæmorrhage which invariably attends on its presence. He was surprised to find that Mr. Bland-Sutton had met with so many cases in comparatively so few years. Cullen, who had most thoroughly investigated its histology, up to 1904 had only found adenomatous change in 19 out of 700 cases of uterine myoma that he had examined. Very few instances of diffuse adenoma had been reported upon in the United Kingdom. In England the most complete pathological investigation had been made by Dr. Murdoch Cameron and Dr. F. E. Taylor in the case they recorded.¹ He (Dr. Macnaughton-Jones) had met with only two cases of true diffuse adenoma; one was some years ago, and was reported on for him by Mr. Targett; the other by Dr. Cuthbert Lockyer. The first of these was particularly interesting. The patient, a young married woman, had the right adnexa removed for a cystic ovary; two years subsequently the left adnexa were affected, and then with the ovary an intra-ligamentary cyst was also taken from the broad ligament. After two years she came back complaining of severe uterine hæmorrhage. She was then curetted, but without benefit. Dr. Cullingworth saw her with him in consultation, and it was agreed to perform hysterectomy. He accordingly performed the supra-vaginal operation, and it was found at the time that another broad ligament cyst had developed at the left side. She had no pelvic symptoms afterwards, though two years subsequently she had her fourth cœliotomy for appendical trouble. The appendix, which was removed, was bound down by adhesions. Since then, some four years ago, she has been in perfect health. In the other case the uterus was also removed by the supra-vaginal method. The patient, a widow, was approaching the menopause, and was blanched by repeated and almost continuous hæmorrhages, for which she had been curetted. She had had no trouble since the operation. It was worthy of notice how often the adnexa were involved in these cases. With regard to the supra-vaginal method of operation, he did not join in the President's condemnation of it at their last meeting. He believed that in the great majority of cases of benign uterine tumour, such as ordinary adeno-myoma, it was applicable. It was more rapidly performed, involved less risk than the pure hysterectomy, and in those cases in which secondary infection was not to be feared, he thought it was the operation of election.

Dr. COMYNS BERKELEY agreed with Mr. Bland-Sutton that supra-vaginal hysterectomy was the best operation for most cases of fibro-myomata. As a rule it was a simple, safe, and easy operation, and could generally be performed within half an hour. Mr. Bland-Sutton had just mentioned that the specimen

¹ *Journ. Obstet. and Gyn. Brit. Emp.*, 1904 v, p. 248.

he showed was removed in twelve minutes. The speaker had performed the operation in thirteen minutes and Dr. Victor Bonney in ten minutes, in both cases the abdominal wound being closed in three layers.

Mr. ALBAN DORAN maintained that the subtotal operation was good surgery, as the sacrifice of the cervix was usually needless and entailed weakening of the pelvic floor. Its after-results, in his own experience, were relatively even more satisfactory than he believed them to be when he read a communication before the Obstetrical Society, in 1905, advocating the operation on evidence based on the study of sixty cases. He did not deny, however, that there was some risk of leaving behind malignant elements in the cervix, and therefore he trusted that Mr. Bland-Sutton would follow up the case which he had just reported and publish its after-history.

Dr. R. H. PARAMORE said he would like to ask Mr. Doran—if the cervix uteri formed part of the pelvic floor, as Mr. Doran had stated—how he explained the good results (*i.e.*, as regards visceral position) which persisted in spite of its removal. Thus in Wertheim's operation the cervix is removed, the upper part of the vagina is removed, and as much as possible of the pelvic visceral connective tissue is removed, yet prolapse (*e.g.*, of the scar and superimposed viscera, as so often occurs after hysterectomy for prolapsus uteri) does not result. This shows plainly enough that the actual pelvic floor is in no way impaired by removal of the cervix; and that therefore the cervix cannot be considered as forming part of the pelvic floor.

The PRESIDENT (Dr. Herbert Spencer) said that adeno-myoma of the uterus had been shown by Cullen to be a common condition, occurring in over 5 per cent. of the fibroid uteri examined. He was surprised to hear that the specimen exhibited was the largest Mr. Bland-Sutton has seen. He (the President) showed a drawing of a larger specimen; but he had met with adeno-myoma in an enormous uterus weighing approximately 70 lb. In the case of the patient of whose uterus he showed the drawing, cancer of the breast developed about two years after the total hysterectomy, and it was an interesting subject of inquiry whether patients with adeno-myoma were more prone to cancer than others. He had noticed that Cullen had found cancer of the cervix or the body of the uterus in a considerable proportion of his cases. Mr. Bland-Sutton's patient was discharged twelve days after the operation (much too early a date, in his opinion, for the discharge of a patient after abdominal section), and no subsequent history was given, so that the case threw no light on this interesting subject. The duration of an operation depended to a large extent upon its difficulty and the care with which it was performed, and upon the careful suturing of the abdominal wall (especially of the fascia), which might well occupy twelve minutes. By adopting the through-and-through method of suturing much time was saved, but the danger of hernia was increased. He asked Mr. Bland-Sutton to state how he stitched up the wound in his case.

Mr. BLAND-SUTTON, in reply, said that the chief object he had in showing the specimen of an adeno-myomatous uterus was to draw the attention of the Section to a pathological condition of great importance and interest. He thought

it somewhat remarkable that although this disease had been described in 1896 by von Recklinghausen and by Cullen, yet up to the present time specimens illustrating it had only on two occasions been brought forward at its meetings: two examples by Dr. Tate and three by Dr. Cuthbert Lockyer. He thought perhaps the chief reason why adeno-myomatous disease had been so persistently neglected in London was largely due to the fact that its identification required the careful microscopic examination of the implicated tissues, but he felt sure that such a reproach could easily be removed if some of the young and able gynæcological surgeons belonging to the Section would take some interest in this disease. Cullen, in his admirable monograph, had given the results gathered from an examination of 1,200 examples of fibroid uteri, and among this multitude he detected seventy examples of diffuse adenomata, so that the disease can in no sense be described as uncommon.

A Case of Extra-uterine Gestation: Operation during the Sixth Month of Pregnancy.

By HERBERT J. PATERSON, F.R.C.S.

THREE years ago I reported to the Obstetrical Society of London a case of operation during the sixth month of extra-uterine pregnancy.¹ In that case the placenta was removed and the hæmorrhage controlled by the ligation with silk of many vessels, and by packing the sac with gauze. The points of interest in that case were, first, that the gestation persisted in spite of two severe, and three slight, attacks of internal hæmorrhage. Secondly, the difficulty in determining whether the fœtus was alive or dead. Thirdly, the severity of the symptoms caused by intestinal toxæmia due to intestinal paresis, the result, as I then thought, mainly of injury of the peritoneal coat of the bowel produced by the separation of adhesions, but which I am now of opinion was due almost entirely to the gauze packing. The paresis was successfully treated by repeated doses of calomel. Fourthly, the prolonged convalescence of the patient, due to the persistence of a sinus for eighteen months, from which ligatures were discharged from time to time. For several months the patient suffered from abscesses in her neck, probably the result of septic absorption from the persistent sinus. Eventually, however, the patient made a complete recovery and is at the present time (six and a half years after the operation) enjoying robust health.

¹ *Trans. Obstet. Soc.*, 1905, xlvii, p. 326.

A few months back I had under my care a somewhat similar case, of which the following are brief details: M. S., aged 29, suffering from abdominal pain, was admitted into the London Temperance Hospital on March 9, 1908, under the care of my colleague Dr. Porter Parkinson. The patient had had ten children, of which five were premature and still-born. The catamenia had been perfectly regular, the last period having ceased on the day of admission. Condition on admission: Patient is extremely anæmic, but well nourished. Temperature 100° F., pulse 116. In right iliac region, reaching inwards beyond the middle line and to the level of the umbilicus, is a smooth, firm, rounded, fixed, tender swelling. *Per vaginam*—some fullness in vaginal roof, cervix to left texture firm, canal closed. Bimanually—uterus somewhat large; the swelling felt *per hypogastrium* cannot be felt in the pelvis, but tenderness prevents a satisfactory examination. I thought that the patient had a twisted ovarian cystoma. The question of an extra-uterine gestation was discussed, but the very definite statement by the patient as to the absolute regularity of the catamenia seemed against this hypothesis. On March 14 the patient was markedly jaundiced, but by March 16 the jaundice had almost disappeared. On March 18 I opened the abdomen in the middle line below the umbilicus. The intestines in the lower part of the abdomen were much matted together. On separating the adhesions much recent blood welled up and a dark purple cyst-like swelling was exposed, which was recognized as an extra-uterine gestation sac. On incising the sac there was copious bleeding. The foetus was rapidly extracted and the bleeding checked by the pressure of the hand inside the sac. Further examination showed that the sac was incomplete, the roof of the cavity being formed of matted intestine. The placenta was attached to the posterior wall of the uterus and to the intestine. It was very friable and readily separated, but the bleeding was profuse. The placental site was compressed by the hand, while the cavity was tightly packed with gauze. The foetation had apparently occurred in the right Fallopian tube, which was ruptured close to the uterus. The pouch of Douglas was filled with recent unclotted blood. There was some free recent blood in the flanks. After removal of the temporary packing a large rubber tube was placed in the cavity and packed tightly around with gauze. No ligatures were used. The foetal circulation was active at the time of operation, and the foetus measured just over 6½ in. and weighed 8½ oz., so that pregnancy was in the beginning of the sixth month. During the night following the operation the patient was very restless and vomited frequently; pulse 140.

Continuous saline was administered *per rectum*. The next day 3 gr. of calomel were given in $\frac{1}{2}$ gr. doses. On March 20 the patient was still vomiting frequently. Four grains of calomel were given, followed by a Seidlitz powder. At 2 p.m. the patient was worse; she was vomiting more frequently, sometimes as often as three times in ten minutes. Four more grains of calomel were given immediately after an attack of vomiting, and later on two more grains. During the afternoon she had very severe pain, so much so that I was asked to allow her to have morphia. At 7 p.m., however, her bowels were freely opened, the vomiting ceased, and, except that she had an attack of abdominal pain with temporary intestinal obstruction on April 19, her recovery was uneventful, and she left the hospital on June 5, with the sinus, from which during the ten days following the operation there was a considerable flow of blood and pus, firmly closed.

This case in several respects resembles the one I have previously reported. In both there was the same condition of intestinal paresis following operation, accompanied by such severe vomiting, rapid pulse, and abdominal distension as to suggest septic peritonitis. In both cases this was treated by repeated doses of calomel, which in the second case caused such severe pain that one had to steel one's heart against giving morphia, a mode of treatment which I am convinced would have ensured a permanent but undesirable relief. Further, notwithstanding secondary rupture of the gestation sac and free hæmorrhage, the fœtus was still living. Profiting by my experience in the first case, no ligatures were used inside the abdomen, with the result that complete restoration to health was much more speedy than in the first case, in which a sinus persisted for eighteen months.

In the first case the diagnosis was correct, in the second wrong. The regularity of the catamenia up to the very day of admission influenced me in excluding extra-uterine pregnancy. There was, however, one sign to which I did not attach sufficient importance. I allude to the fact that shortly after admission the patient became jaundiced. Reflecting on this circumstance, I think that this transient jaundice ought to have suggested internal hæmorrhage, and internal hæmorrhage ought to have influenced me in favour of extra-uterine gestation.

I believe that at the present time the balance of opinion is in favour of removing the placenta in such cases, but I hope that the recital of this case will elicit a discussion on the details of treatment in such cases. The important points appear to me to be: First, the

avoidance of the use of ligatures inside the sac, which, as they are certain to become infected, will greatly delay convalescence. Secondly, the arrest of hæmorrhage by gauze packing, which, although it leads, in my experience, to severe intestinal paresis, is preferable to the use of ligatures. Thirdly, the treatment of the paresis caused by the gauze packing by *early* and repeated doses of calomel. Fourthly, the withholding of morphia at all costs. Fifthly, the use of continuous injection of saline by the rectum—one of the many good things which have come from America—after the method advocated by Dr. J. B. Murphy.

Operation at End of Fifth Month for Extra-uterine Gestation with Living Fœtus ; Recovery without Recurrent or Secondary Hæmorrhage.

By ALBAN DORAN, F.R.C.S.

J. E., AGED 35, was admitted into my wards in the Samaritan Free Hospital on July 3, 1908 ; she was sent to me by Mr. Boodle, of Sittingbourne, suffering from an abdominal tumour of uncertain nature. She was fairly nourished, but her hair was turning grey and her cheeks were flushed. She had been married eleven years, and her only child was aged 10 ; since its birth until the present illness there had never been any sign of pregnancy. On Easter Day, April 19, when scrubbing her floor, she had a sudden attack of hypogastric pains and vomiting ; she at once took to her bed, and Mr. Boodle noted symptoms of peritonitis with tenderness, chiefly to the left of the hypogastrium. A day or two later a distinct lump was definable in the region of the appendix. He sent her into Rochester Hospital. The catamenia had been quite regular until three years previously, but since then they had become irregular, with intervals of seven or eight weeks ; they had also grown scanty and painful. I found, after careful inquiry, that there had been no show of any kind for four months before admission.

The patient was kept in Rochester Hospital for several weeks, and was ultimately discharged at her own request. Mr. Godfrey Taunton kindly informed me : "The physical signs when she was in the hospital were fullness in the left fornix, slightly impaired mobility of the uterus, which seemed enlarged, the sound passing $2\frac{3}{4}$ in. During the last week of her stay there was distension of Douglas's pouch with fluid. The

temperature ranged from 101° F. to normal. There was no distension of the bowels, but much constipation, requiring enemata; there were no acute symptoms pointing to rupture of a foetal sac; in fact, the only symptoms complained of were abdominal pain and occasional sickness. She gave a history of passing a decidual cast, but also stated that she frequently passed something of the kind. I could define no foetus or fluid swelling in the abdomen, and my diagnosis lay between salpingo-ovaritis and extra-uterine pregnancy." After her discharge the fullness increased until Mr. Boodle was able to define a circumscribed and fluctuating tumour.

The abdomen was distinctly distended, but not tense. A tumour occupied the hypogastrium, extending as high as the umbilicus. It lay mostly to the left of the middle line, and was hardly movable; palpation was painful and set up contraction of the muscles of the abdominal wall. The tumour was more definable on the left than on the right side; fluctuation was not marked; distinct gurgling was noted on palpation over the right iliac region. The cervix was thick and short and closely connected with the tumour in the abdomen. A soft mass occupied Douglas's pouch and the left fornix; it was tender to touch. The sound passed for 4½ in. upwards and forwards, there was no blood or sanies on its point when withdrawn, but some clear glairy mucus. The uterus and mass were almost fixed. The breasts were fairly developed, but flaccid; no milk could be pressed out of them. The areolæ were well marked; the patient's complexion was naturally dark. The tongue was pale and rather glossy; the bowels, previously regular, had been constipated all through the illness. The patient had suffered from dysentery in 1902 and 1904. The pulse was 84, small, compressible, and distinctly intermittent.

I kept the patient under observation for seven days. The temperature did not rise above normal, and, indeed, on the evening of admission, after a railway journey, it was as low as 98·4° F. On July 5 several attacks of pain occurred in the course of the afternoon, passing away before the night; no foetal heart-sounds could be heard on auscultation. Not a trace of blood or sanies came away before the operation, and their total absence was somewhat misleading. Although the history suggested ectopic gestation, I rather suspected axial rotation of an ovarian cyst, with subsequent adhesion of intestine. It is true that there had been absolute amenorrhœa for at least four months, if not five, but that symptom has been observed when the patient's health is impaired by the pain and the local peritonitis caused by twisted pedicle;

besides, the total absence of hæmorrhages is unusual in extra-uterine pregnancy. The condition of the breasts and areolæ did not absolutely favour the diagnosis of pregnancy; the outer limits of the areolæ were sharply defined, as in many dark-complexioned subjects when not pregnant. Still, diagnosis was very obscure, as the symptoms of several pelvic diseases, which seemed possible in this case, are known to be irregular.

I operated on July 10, assisted by Dr. Drummond Maxwell, Dr. Belfrage administering the anæsthetic. On opening the abdominal cavity I found much intestine adherent to the tumour. On setting the bowel free I noted that the vermiform appendix was quite healthy. The tumour was now seen to be a big cyst; its walls were dull and of a deep ochreous colour, much as is seen in an ovarian dermoid after chronic axial rotation. On tapping, a little turbid fluid came away. I then passed my hand behind the cyst, and could feel a fœtus lying in its lower part. I pushed the greater part of the cyst gently out of the abdominal wound, and carefully enlarged the tapping puncture with scissors till I was able to extract a fœtus of about the twenty-fourth week, which showed signs of life. The sigmoid flexure ran on the posterior wall of the sac.

Although I took pains to extract the fœtus as gently as possible, the greater portion of the placenta was torn away with it. The sac wall bled profusely. I packed the cavity with a long roll of gauze, and, as the cut edge of the sac bled freely, I made a purse-string suture round it, but I drew on it very little, as the least traction affected the sigmoid colon, which was incorporated with the wall of the sac. The bleeding from the edge was, however, checked by the suture without much traction. On the other hand, the cavity of the sac required repeated packing. I sewed the edge of the sac lightly to the parietal peritoneum and closed the abdominal wound except at its lower extremity, where a portion was left open on account of the gauze packing. There was much oozing for four hours after the operation, and a pad was twice applied to the lower part of the abdomen. For two days the patient suffered from nausea and vomiting, with high pulse.

At 12 p.m. on July 12, fifty hours after the operation, Dr. Belfrage administered chloroform and I prepared to dress the wound. I feared that the hæmorrhage might recur, as in cases recorded by Malcolm¹

¹ "An Operation between the Third and Fourth Months of Extra-uterine Gestation, with Removal of a Living Fœtus and much trouble from Hæmorrhage during Convalescence," *Trans. Obstet. Soc. Lond.*, xlv, p. 382.

and others. In drawing the gauze out of the wound I found that it was saturated with blood, which had become very foetid. When the sac was emptied I was greatly relieved, as I noted the total absence of oozing from any part of the sac. About two or three minutes later I passed some dry absorbent wool into the lowest part of the sac, and on retracting it I found that it was not stained with fresh blood. I packed the cavity of the sac lightly with a roll of gauze. Within seven hours rather free uterine show, the first for four months, was observed, but there was no more oozing from the sac. No traces of decidua came away from the uterine cavity then nor later.

Much foetid bloody fluid collected in the sac for several days after the dressing; it was washed out with antiseptic solutions. The dressing gave the patient great relief, flatus passed freely, and the bowels were easily relieved by enemata. The knot of the purse-string suture came away on July 17, and the remainder and the sutures fixing the sac to the parietal peritoneum were gradually eliminated. By July 23 the cavity of the sac had greatly contracted, so that it had become a sinus. A sound introduced into it passed 5 in. downwards and backwards. On pelvic exploration, the uterus was found to be distinctly movable. The end of the sound which had been passed into the sinus could be felt touching the anterior wall of the rectum behind the uterus.

On August 10 I left the patient under the charge of Dr. Drummond Maxwell. The sinus, as was expected, contracted very slowly, and there was some purulent discharge, free from faecal odour.

On September 9 the patient was discharged from hospital; I examined her on my return from a vacation, two days before she left my wards. She had gained flesh and was in very good general health. The sinus had become narrow and tortuous, discharging a little pus daily. The cervix had undergone very marked involution, having become short and thin, whilst when I examined the parts before the operation it was very thick. The sac seemed to have contracted on to the back and fundus of the uterus, the whole mass being to a slight extent movable. The right fornix and Douglas's pouch were free. A slightly tender body, feeling like the ovary, could be defined in the left fornix. There had been no period since the operation.

(On December 1 Mr. Boodle informed me, in reply to a letter, that the patient was very well in general health, and only complained of a feeling of soreness over the left inguinal region. The sinus was gradually getting shallower. Menstruation had recently occurred, but the show was scanty.)

The Fœtus.—The height of the fœtus was 23 cm., or nearly 9 in. The hairy scalp was distinct, the eyebrows well developed. The skin was almost opaque, not wrinkled, and bore no distinct lanugo. The limbs were well developed, with perfect hands and feet, bearing equally perfect fingers and toes; the nails were very short. The external organs were of the female type, distinct and without any visible anomaly, meconium escaped from the rectum, and there was no sign of spinal deformity, fissures of the lip and palate, or ectopia of the viscera. This fœtus moved its limbs and distinctly cried when extracted, and it showed signs of life for some minutes after delivery. At the end of the fifth month the fœtus of intra-uterine pregnancy measures about 25 cm. in height. Granting that an ectopic fœtus is less well developed, especially as regards nutrition, I think that the proportions in this specimen indicate that the pregnancy had advanced to the end of the fifth month.

Placenta and Cord.—The placenta, as I have already stated, was torn through during extraction. The greater part, very thick, appeared quite normal, but at the edge there was a large infarct, over 2 in. long by 1 in. broad and 1 in. thick; the laceration during extraction seemed to have begun in the brittle tissue of this infarct. The portion of the placenta removed, some 4 in. in width, included the umbilical cord, which was 8 in. long.

Remarks.—In this case pregnancy began in the left Fallopian tube. The last show, four months before the operation, was no doubt a uterine hæmorrhage caused by the abnormal gestation and not a menstrual period, as the fœtus was too well developed to represent only four months of pregnancy. The acute attack of pain on April 19 seemed to indicate rupture of the tubal sac into the left broad ligament, a "posterior tubo-ligamentary pregnancy" (Taylor), as I found it to be at the operation. The lower boundary of the sac in that case was therefore the rectum. The pressure on the bowel caused constipation, as Mr. Taunton reported, and as was the case after admission into my wards, until the removal of the gauze packing. The placenta was *not* attached to the bowel, else there might have been diarrhœa (Freund). The fœtus was living; therefore *Bacillus coli* infection, not rare in posterior tubo-ligamentary pregnancy, had not taken place. The most important question, however, in regard to this case is the treatment of the sac. I fail to see how I could have adopted any safer method than that which seemed to me the only way to save the patient's life. The tearing through of the placenta was accidental, whilst on my attempting to remove the remainder so much hæmorrhage occurred that I left it alone and packed

the sac; arterial blood spurted from the cut edges of the sac, but the purse-string suture checked it at once. The sac was secured to the lower edges of the abdominal wound, otherwise the packing would not have been effective. The packing was removed on the second day, and the hæmorrhage did not recur. On the other hand, the disadvantages of this method, inevitable under the circumstances in my case, were clearly manifest to me. In drawing on the purse-string suture, as the cyst wall consisted of the posterior layer of the left broad ligament and the adjacent peritoneum of Douglas's pouch, the large intestine was dragged down, forming a sharp angle, which would have meant obstruction had I not relaxed the ends of the suture considerably. This suture, as well as those which fixed the edges of the sac to the wound, did not come away until much local suppuration had taken place, and the pus mixed with the sanies from the remains of the placenta made a foetid mixture which necessitated repeated daily dressings of the sac. Lastly, I dreaded the spread of the suppuration, as in Mr. Herbert Paterson's case,¹ but I feared much more the recurrence of hæmorrhage, as in Mr. Malcolm's, to which I have already referred, and fortunately there was no more bleeding and the suppuration did not extend. Still, I understand that Mr. Paterson and others find that sutures and ligatures may be dispensed with in the treatment of the foetal sac late in pregnancy, and I desire to learn how far this method guarantees the patient against that essential and immanent danger, never absent when the foetus is alive, namely, hæmorrhage.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) thought that these cases were of much value, since cases of extra-uterine pregnancy with living children in the second half of pregnancy were rare. Of three such cases which he had seen, only the last recovered. In the first he left the placenta behind in accordance with the teaching of the time. In the second he removed the placenta and membranes and packed with gauze; but the patient died the same night of syncope. In the third he treated the case in the same way as the second and recovery ensued. Mr. Paterson estimated the pregnancy in his case as "in the sixth month," the foetus only measuring $6\frac{1}{2}$ in. The size usually given for an intra-uterine foetus five calendar months old was 10 in., and for one six calendar months old 12 in. He did not know of any tables giving the size of the ectopic pregnancy foetus; but it was important that the age of the pregnancy should be

¹ "A Case of Extra-uterine Gestation; Operation during the Sixth Month of Pregnancy," *Trans. Obstet. Soc. Lond.*, xlvii, p. 326.

correctly stated as the mortality was enormously increased in the second half of pregnancy.

Dr. AMAND ROUTH had no doubt that the vomiting was due to the irritation of the gauze packing. It came on as the anæsthesia passed off and persisted till the gauze was removed. He did not consider that vomiting coming on immediately after the operation should be treated by calomel, but rather by morphia, for so long as the gauze was firmly packed in the pelvis and the rectum thus mechanically obstructed, purgatives could not efficiently relieve. Drainage must be continued till the wound closed if gauze packing had once been used.

Dr. MACNAUGHTON-JONES wished to call attention to the value of cyllin as a preventive in those cases of vomiting in which there were toxic influences present through bowel complications. He gave it as a general rule in all cases in which such were to be apprehended in minim doses and in the palatinoid form. He should not like to give calomel in large and repeated doses when there was persistent vomiting, as in Mr. Paterson's case.

Mr. ALBAN DORAN, in reply, observed that the vomiting caused by the packing of the cyst, which in his own case did not cease until the gauze was removed, was only what he expected. He did not hurry over purgatives, and there was no difficulty in getting the bowels open in this instance directly the pressure of the packing was removed. The gauze packing checked hæmorrhage, always imminent under the circumstances, whilst there was relatively little peril from the vomiting and obstruction. He admitted, on the other hand, that the free discharge and suppuration made convalescence tedious, and congratulated Mr. Paterson on the speedy recovery in his case; but the sutures in his own case, not identical with Mr. Paterson's as to the anatomy of the sac, were absolutely necessary.

**The Bacteria of the Puerperal Uterus, with especial reference
to the Presence of Hæmolytic Streptococci.**

By ARNOLD W. W. LEA, M.D., and E. J. SIDEBOTHAM, M.B.

THE problems presented by the relation of the *Streptococcus pyogenes* to puerperal infection have been the subject of many investigations during the last few years. This organism is undoubtedly the cause of the great majority of cases of severe infection, and is usually found in abundance in the uterine discharges. It is, however, not infrequently present in the vagina and in the uterine secretion after delivery in cases pursuing a normal course. The streptococcus has also often been found in the slighter forms of infection which are clinically regarded as being of sapræmic origin. It is therefore of great importance to determine whether the streptococci found in severe forms of infection are essentially different from the saprophytic types of the organism or not. The remarkable variations in the infective power of streptococci have led to many attempts to classify the various types, with the object of bringing their morphological and cultural features into relation to their pathogenic power. Efforts have been made to detect specific differences between varieties of streptococci according to the length of their chains, their mode of growth and staining reactions, their power of agglutination, and their virulence for animals. None of these features, however, have been found to be constant. The results of recent experiments on immunization of animals carried out by Natvig, Zangemeister and Meissl appear to show that all facultative anaerobic streptococci are essentially of one stock. Scheib, who has closely investigated this question, states that it is impossible to distinguish between streptococci existing as saprophytes in the lochial discharge and those causing puerperal infection. During the last few years great stress has been laid upon the hæmolytic power of streptococci as an indication of their virulence, and it is to the solution of this question that our observations have been mainly directed. Before considering our results in detail it may be desirable to briefly review the present-day knowledge of the organisms found in the vaginal secretion and in the lochial discharge of healthy individuals.

The Vagina.—An enormous amount of research has been devoted to the investigation of the vaginal secretion in healthy women during

pregnancy, with the object of determining the presence or otherwise of organisms capable of producing infection. Microscopic examination of the secretion reveals the presence of many organisms, but accurate information can only be obtained by cultural methods, and for this purpose it is essential to remove the vaginal secretion without any contamination from the vulva. This may be carried out by separation of the labia and the introduction of a platinum wire loop into the vagina, great care being taken to avoid contact with the vulva. Many observers have used a speculum. This, however, appears to be inadvisable, as it almost inevitably results in the introduction of organisms from without. A method which is probably the least open to objection consists in the introduction of a tube as first adopted by Krönig and Menge.

The earlier observers, such as Gonner, Winter, and others, found many bacilli and cocci in the vaginal secretion; these were mainly anaerobic in type and were not pathogenic for animals. Döderlein, in 1892, published his classical paper, based upon the examination of the vaginal secretion of 195 pregnant women. He described two forms of secretion as existing in pregnancy.

In the normal condition the vaginal secretion is scanty, white in colour, and cheesy in consistency, containing epithelial cells and leucocytes; it is intensely acid in reaction and is characterized by the presence of long anaerobic bacilli. These organisms appear to produce lactic acid and to exert a bactericidal influence on pyogenic bacteria introduced from without. In the other type, considered by Döderlein to be pathological, the secretion is profuse, yellowish white in colour, containing many leucocytes and epithelial debris; it is only slightly acid in reaction, and contains a large number of micro-organisms of various kinds, including streptococci, which were present in 10 per cent. of the cases observed by him. Further observations have shown that whilst the acid vaginal secretion is unfavourable to the growth of organisms the bacillus of Döderlein itself only plays a subordinate part, and that although the two forms of secretion undoubtedly exist, it is of little practical value to distinguish between them.

The researches of Stroganoff, Dubendorfer, Bergholm, Burghardt, Vahle, Stolz, and others have in the main confirmed these conclusions. All these observers have found streptococci to be present in the vaginal secretion in a proportion varying from 10 to 15 per cent. of cases. Bumm and Sigwart, in 1904, reported that they were able to cultivate streptococci in 30 to 50 per cent. of women examined during pregnancy, the secretion being obtained by the use of a speculum.

On the other hand, Krönig, in a series of 167 cases, was unable to demonstrate the presence of streptococci, and concluded that the vaginal secretion should be regarded as free from pyogenic organisms with the exception of the gonococcus, which was occasionally present. He was able to cultivate, however, a large number of anaerobic organisms, including an anaerobic streptococcus; none of these, however, possessed any pathogenic power. Whitridge Williams and Bergland have also published a series of observations which tend to show that if sufficient precautions are taken to avoid vulvar contamination no pyogenic organisms will be found in the vagina during pregnancy.

It seems probable that these widely different results are attributable partly to the absence of any uniform method of removing the secretion and partly also to the different culture media which have been adopted. Thus, those observers who have used alkaline agar only have rarely discovered streptococci; if, however, the agar is combined with a reducing substance, such as grape sugar, streptococci are found in a considerable proportion of cases, the secretion being obtained without any probability of vulval contamination (Bergholm, Bohne, Stähler, Winkler). We must therefore conclude that organisms indistinguishable from pyogenic streptococci, together with diplococci and the *Bacterium coli*, are not unfrequently present in healthy women during pregnancy, especially in the lower part of the vagina.

Walthard gives the following list of organisms which have been found to exist in the vagina of pregnant women, and which are indistinguishable from those present in puerperal infection:—

(1) Facultative anaerobic streptococci of the type of *Streptococcus pyogenes puerperalis* (Döderlein, Bohne, Stähler, Winkler, Bumm).

(2) Facultative anaerobic diplo-streptococci (Natvig).

(3) Anaerobic streptococci (Krönig and Menge).

(4) Staphylococci of the type of *Staphylococcus albus*.

(5) Bacteria of the coli group (Bensis, Kottmann, Walthard).

The following organisms are also found in rare instances and usually associated with offensive secretion:—

(6) *Bacillus funduliformis*.

(7) *Pseudo-tetanus bacilli*.

(8) *Bacillus aerogenes capsulatus*.

The Cervix.—The cervical canal during pregnancy is occupied by a plug of firm clear mucus, which effectually closes the uterine cavity. The secretion of the cervical glands appears to be an unsuitable culture medium for bacterial growth and, moreover, frequently contains phagocytes which

exert a definite bactericidal influence. The vaginal portion of the cervix, and even the lower part of the cervical canal, contains many organisms, and these are much increased in number if erosions or lacerations are present.

Varaldo has recently investigated the cervical secretion in thirty-two pregnant women in whom these conditions existed, and was able to detect streptococci, gonococci, and bacteria of the coli group in a considerable proportion of cases. Fullarton and Bonney also, in examining the cervical secretion of thirty non-pregnant women, found staphylococci, diphtheroid bacilli, and various forms of diplococci. None of these organisms, however, appear to be able to penetrate the protective zone of the cervical secretion during pregnancy, but it is not improbable that after delivery they ascend into the uterus and may become a source of infection.

All observers agree that the cavity of the uterus is free from organisms during normal pregnancy.

BACTERIOLOGY OF THE GENERATIVE TRACT DURING THE NORMAL PUERPERIUM.

The Vulva.—After delivery the vulval orifice is bathed in lochial discharge, and a large number of organisms are almost constantly present. Investigations on the vestibular secretion by Natvig, Pilz, and others, have demonstrated the presence of streptococci, staphylococci, and the *Bacterium coli* in a large proportion of cases. These organisms usually appear to possess little virulence, but it is quite possible that if they are conveyed into the vagina they may develop some degree of infective power.

The Vagina.—The vaginal discharge during the first twenty-four hours after delivery contains relatively few organisms. This is apparently the result of the cleansing process which occurs during labour as a result of the passage of the child's head with the placenta and membranes; it is possible also that the bactericidal power of the fresh blood may exert some influence. After the second day, however, a large number of organisms may be cultivated from the vaginal lochia. The bacillus of Döderlein, if present, disappears, and the organisms existing in the vagina before delivery multiply rapidly in the alkaline secretion. According to the observations of Krönig and Natvig, organisms from the vulva frequently ascend into the vagina during the early days after delivery.

The Uterus after Delivery.—The bacteria present in the uterine lochia have been investigated by numerous observers with widely different results. The early observations of Döderlein, Winternitz, von Ott, and Czerinewski appeared to show that the lochial discharge was sterile in 70 per cent. to 80 per cent. of cases. Streptococci and pyogenic organisms were found in 7 per cent., and in the remainder anaerobic saprophytic bacteria were present. Franz, however, in 1898, was able to detect pyogenic organisms in every case in the later days of the puerperium. Subsequent observers, such as Burghardt, Wormser, Stähler, Vogel, and others, have been able to cultivate organisms in from 20 per cent. to 50 per cent. of cases after the third or fourth day. Krönig, in a series of afebrile women, found that streptococci and staphylococci were not unfrequently present in the uterine cavity by the end of the third day, usually disappearing after the seventh day, when the lochia became sterile. Organisms undoubtedly ascend from the vulva and vagina, even in women who have never been examined, and it has been demonstrated by Hellendahl that this takes place along the blood-coagula which are usually present in the upper portion of the vagina and in the cervical canal.

Special significance must be attached to the existence of streptococci in the normal lochial discharge. These organisms appear to be present in 10 to 20 per cent. of all cases in the vaginal secretion and often in the uterine cavity.

Bumm and Sigwart, in a careful series of observations, discovered streptococci in the lochial discharge in 50 to 75 per cent. of cases examined within a few days of delivery.

Leo examined the lochia of thirty-eight afebrile puerperal women and found streptococci in the vaginal secretion in 50 per cent. of cases, and in 17.6 per cent. these organisms were also present in the uterine lochia. Schenk and Scheib have also found these organisms in the uterine secretion after the seventh day in at least one-third of their cases.

Stolz investigated the contents of the uterus in a series of normal cases on the fourth day after delivery, and found that organisms were present in no less than 80 per cent. Even in women who had never been examined streptococci were found in the uterine secretion after the fourth day of the puerperium in 36.9 per cent. of cases. In some of these cases febrile symptoms developed which he believes may be attributed to slight traumatism inflicted in the removal of the secretion. He observed that the number of organisms in the uterine cavity

diminishes during the later days of the puerperium, a feature which points to the uterus possessing the faculty of "self-cleansing."

Mansfeld has recently examined the intra-uterine secretion of forty afebrile puerperal women on the fourth and fifth day after delivery. He was able to detect organisms in 60 per cent. of cases, streptococci being present in 22.5 per cent.

In all these observations the secretion was obtained by the introduction of a glass tube into the uterine cavity, and it is impossible in this way to avoid contact with the secretion of the lower part of the cervical canal. It is therefore probable that the cavity of the uterus itself is sterile in a greater proportion of cases than would appear from these figures.

Many attempts have been made to investigate the secretion in the upper portion of the uterus without contamination by the cervical discharge, and the results show a much greater relative proportion of sterile lochia. Thus Little found that the cavity of the uterus immediately after labour was sterile in 92 per cent. of cases, and on the third day in 62 per cent. of afebrile cases. In only one instance was he able to detect the presence of the *Streptococcus pyogenes*.

Nicholson and Evans believe that the positive results obtained by a large number of observers are due to faulty technique, and that the secretion in the uterus is sterile in normal cases throughout the puerperium. Their observations were made by the introduction of a speculum along the cervical canal, through which a tube was passed into the uterine cavity. In a few cases streptococci were observed, always, however, associated with symptoms of infection.

From this brief review of our knowledge, and in spite of much difference of opinion, it must be concluded that organisms closely resembling those present in puerperal infection often exist in the upper part of the vagina and in the cervical secretion soon after delivery, and that this is frequently followed by invasion of the uterine cavity as a result of the spontaneous ascent of organisms. Our observations were made in the early days of the puerperium after disinfection of the vaginal portion of the cervix. In a large proportion of cases a positive result was obtained, but we think it is probable that in many instances the organisms were only present in the cervical canal and lower portion of the uterus.

It is important to know how far examinations and manipulations promote the entrance of organisms into the uterine cavity during the puerperium. Stolz made a series of investigations on the fourth and

ninth days after delivery, and found that repeated examinations made with the sterile hand, and manipulations such as the application of forceps, or version, had little influence on the organisms present in the uterine cavity. The influence of the third stage of labour was, however, very definite. He found that if this was under one hour's duration the proportion of cases in which bacteria were present increased by 10 per cent. This is probably due to the fact that if the third stage is unduly hastened the liability to hæmorrhage and retention of portions of placenta or membranes is much increased. These results were also confirmed by Little, who found that lacerations of the perineum, vaginal examinations, and operative delivery, provided that complete asepsis was maintained, exerted little influence on the "germ content" of the uterus during the puerperium.

Hæmolysis of Streptococci.—The early observations of Bordet (1897) and Marmorek (1902) showed that many streptococci possess a hæmolytic power. Schlesinger, in 1903, conducted a series of experiments with the object of determining the significance of hæmolysis, but his results were not conclusive. Schottmüller was the first who claimed that it was possible to determine the virulence of an organism by its power of hæmolysing human blood. For this purpose he made use of blood-agar as a culture medium and recognized three varieties of streptococci:—

(1) *The Streptococcus pyogenes.*—This is the organism found in puerperal infection. Within a few hours of inoculation it forms on the plates definite white areas indicating destruction of the red blood-corpuscles by hæmolysis.

(2) *Streptococcus mitior or gracilis.*—These organisms produce areas of brown discoloration around the point of inoculation due to the formation of methæmoglobin, but without obvious destruction of the corpuscles. This organism is often found as a saprophyte in the buccal and nasal cavities, and has only slight pathogenic power. It is also the organism frequently present in the vulvar and vaginal secretion.

(3) *Streptococcus mucosus.*—This organism develops slowly in small colonies, but shows no hæmolysis. This type of streptococcus is observed rarely and does not appear to possess any pathogenic power.

Schottmüller, who has for several years made a large number of observations, claims that the hæmolytic power is proportionate to the virulence of the organism, and he regards the presence of a hæmolytic streptococcus as clearly indicating a pathogenic variety of the organism.

His investigations have been mainly confined to the organisms found in the blood in cases of septicaemia, which always appear to possess a high degree of hæmolysis.

These conclusions have not been altogether confirmed by recent work, since many observers have shown that hæmolysis may be produced by organisms of little virulence.

Natvig made a large number of experiments on the hæmolysis of streptococci, and found that this is largely dependent on the mode of culture, and that many organisms possessed of little virulence showed very decided hæmolytic power, and that, in general, variations in hæmolysis do not run parallel with the virulence of the organism. It has not been found possible by cultural methods to convert a non-hæmolytic streptococcus into a hæmolytic organism. If, however, streptococci are inoculated into animals, such as mice, their virulence becomes intensified, and this is often associated with the development of hæmolytic power. The question as to the significance of the presence of hæmolytic streptococci in the lochial discharge or in the blood can, however, only be decided by systematic clinical observations, and considerable evidence exists that the organisms present in severe puerperal infection always show a marked capacity for hæmolysis.

Fromme has published the results of his observations on the presence of hæmolytic streptococci in puerperal women.

In a series of 100 normal pregnant women streptococci were found in the vaginal secretion in no fewer than twenty-seven. None of these, however, were obviously hæmolytic, though in three cases a slight discoloration of the blood was observed around the colonies.

In thirty-six observations made on the lochial discharge of afebrile women after delivery he discovered streptococci in nineteen; none of these were, however, hæmolytic, and according to his observations the streptococci found in the lochia of afebrile puerperal women possess no hæmolytic power. They produce an area of brown discoloration on agar plates, thus belonging to the group of *Streptococcus mitior*, but no destruction of hæmoglobin.

In fourteen women who presented signs of severe infection, hæmolytic streptococci were found in the lochia, and in ten cases of general infection he was able to obtain from the blood hæmolytic streptococci in each instance.

Gonnet, of Lyons, has also published figures which appear to confirm the conclusions of Fromme. In the vaginal secretion of 100 pregnant women he found streptococci in 16 per cent., but in no case did they

possess any hæmolytic power, and the puerperium was afebrile in each case. He also observed the lochial secretion of 100 women after delivery from the fifth to the seventh day. In 90 per cent. of these no streptococci were present. In six cases a streptococcus was found. This showed no hæmolytic power, and in these cases also the puerperium was afebrile. In four instances a typical hæmolytic streptococcus was detected, and each patient developed acute infection. Gonnet also relates six cases of general septicæmia in which hæmolytic streptococci were cultivated from the blood in large numbers. All these patients died.

The results of these observers appear to show that hæmolytic streptococci are never found in the vaginal secretion during normal pregnancy or after delivery, and that if hæmolytic organisms are present puerperal infection has occurred.

Veit goes so far as to state that the presence of hæmolytic streptococci in the lochial discharge is to be regarded as a sign of the presence of grave infection, and that in these cases no local therapeutic measures must be undertaken on account of the danger of generalization.

Freymuth, who has recently made a contribution to this subject, entirely disputes the conclusions arrived at by Fromme. He isolated streptococci from the vaginal secretion of twenty pregnant women and compared these with a strain of streptococci derived from a fatal case of puerperal infection. He found no constant relation in the hæmolytic power, and regards hæmolysis as of no value in distinguishing pathogenic from saprophytic varieties of streptococci.

Heynemann has recently published some observations on the results of the examination of the lochial discharge in 125 afebrile puerperal women, following in detail the technique of Schottmüller. In only twenty-two of these cases was he able to detect hæmolytic streptococci. In four of these the organisms were also present in large numbers in the blood, and each case terminated fatally. In the remaining eighteen the patients suffered from symptoms of severe local infection of the uterus, but the blood remained sterile throughout. All of these cases made good recoveries with simple treatment. He regards the presence of hæmolytic streptococci in the lochial secretion as of great practical value, as in a doubtful case it may serve to complete the diagnosis. He further believes that it is not necessary to introduce a tube into the uterus, but that if these organisms are found in the secretion of the upper portion of the vagina a clinical diagnosis of the existence of infection may be made.

From these observations it is clear that in the great majority of

cases of severe infection the streptococci present possess well-marked hæmolytic power. The question, however, as to whether we really possess in this hæmolysis an absolute means of differentiating between saprophytic and pathogenic organisms is not yet settled.

PERSONAL OBSERVATIONS.

Method of Investigation.—We have examined the lochial discharge from the cavity of the uterus in a series of fifty-eight cases between the second and ninth days after delivery. The technique adopted in the removal of the secretion is important, as it is essential that it should be obtained without contamination by the vaginal discharge. The patient is placed in the lithotomy position and the vulva thoroughly cleansed. The labia are now held widely apart and a Sim's speculum is introduced along the posterior vaginal wall. The cervix is thus readily exposed and the anterior lip is seized by a fine vulsella. The vaginal portion is now thoroughly cleansed by sterilized absorbent wool. A curved glass tube (after the pattern introduced by Döderlein) is passed into the uterus, up to the fundus. Suction is made by means of a syringe, and as much lochial discharge as possible obtained from the uterine cavity.

Preparation of the Plates.—The blood was obtained from the median basilic vein of a healthy individual. This can be very simply carried out and causes a very small amount of discomfort. The arm is tightly bandaged below the shoulder and the skin around the elbow disinfected; a small incision is now made over the distended vein in order to avoid any possibility of contamination from the skin. For the removal of the blood a glass syringe after the pattern of Luer was used. It is essential that this should be previously washed out with a sterile solution of citrate of potash in order to prevent premature coagulation of the blood. The needle is introduced into the vein in a direction opposite to the blood-flow. The syringe rapidly fills, 10 c.c. of blood being usually removed. This is now immediately added to a solution of agar in a test tube in the proportion of 2 c.c. of blood to 5 c.c. of agar; after thoroughly mixing, the solution is poured on to the Pétri plates.

Mode of Culture.—The contents of the uterine tube are immediately smeared on to the surface of freshly prepared blood-agar by means of a spatula made of platinum foil. The plates were incubated at a temperature of 37° C. in an inverted position, in order to prevent condensation of water on the surface of the agar. The plates were examined at the end of twenty-four and forty-eight hours, and cover-glass preparations made for each variety of colony present on the plate. In

many instances sub-cultures were also made in order to identify the type of organism. Hæmolysis was usually observed at the end of twenty-four hours, but in some cases it was necessary to wait until the end of the second day. The appearances are quite characteristic, each hæmolytic colony being surrounded by a characteristic white zone of absorption. Microscopic examination of the hæmolytic area shows complete disappearance of the hæmoglobin. The anaerobic cultures were made by mixing the blood with recently boiled glucose-agar. The plates were then placed in a glass vessel from which all the air was exhausted by means of a water pump.



Hæmolysis of streptococci, from twenty-four hour culture on blood-agar plate.

Results.—The day after delivery on which the secretion was removed was as follows:—

Day	Cases	Sterile	Organisms found in
Second	15	2	13
Third	20	5	15
Fourth	6	1	5
Fifth	6	0	6
Sixth	4	2	2
Seventh	2	0	2
Eighth	3	2	1
Ninth	2	0	2
Total	58	12	46

The contents of the uterus were sterile in twelve cases only, *i.e.*, a proportion of 20 per cent., and it is noteworthy that in five of these cases no vaginal examination had been made throughout. In 80 per cent. of cases bacterial growth appeared on the blood-agar plates, usually within twenty-four hours of inoculation. The organisms found were as follow: *Streptococcus pyogenes*: hæmolytic, five cases; non-hæmolytic, seven cases. *Staphylococcus pyogenes*: hæmolytic, twenty-six cases; non-hæmolytic, twenty-seven cases. *Diplo-streptococcus*: this appears to be the organism described by Natvig as the "para-pneumococcus," fourteen cases. Bacilli of the *coli* group, ten cases. *Pseudo-diphtheritic* bacilli, eighteen cases. *Proteus* bacilli, three cases. The anaerobic bacteria present in the secretion were cultivated in ten cases. In two the plates remained sterile. The organisms present in the other cases were anaerobic staphylococci and lanceolate diplococci, which frequently showed some power of hæmolysis.

Staphylococci.—The *Staphylococcus albus* was the organism usually present, but in a few instances colonies of the *Staphylococcus aureus* were observed. Hæmolytic and non-hæmolytic colonies frequently occurred together, but the hæmolysis was often not visible until the end of forty-eight hours. It was noticed in making sub-cultures that the non-hæmolytic variety of staphylococcus liquefied gelatine much more rapidly than the hæmolytic form.

Streptococci.—The hæmolytic streptococci formed long chains and grew abundantly in peptonized broth, producing a thick flocculent deposit. The pathogenic power of two strains of hæmolytic streptococcus was tested by inoculation into rabbits. One of these, derived from a case which manifested signs of infection, produced no lesions; the inoculation was not, however, made until the streptococcus had been cultivated for three weeks. In another case the hæmolytic streptococcus from a normal puerperium was inoculated into the hind leg of a rabbit; this was followed by the formation of a large abscess with local lymphatic infection. No organisms were, however, found in the pus, and no growth was obtained on culture, thus showing that the organism was possessed of very slight pathogenic power.

Pseudo-diphtheria Bacilli.—In some cases these bacilli were very short and resembled the Hoffmann bacillus, but usually the organisms were microscopically indistinguishable from the Klebs-Löffler bacillus and showed granules which stained by Neisser's method.

Diplococci.—These organisms were present in 20 per cent. of cases and were of two distinct types. Both forms were facultative anaerobes

growing equally well aerobically and anaerobically. Many of the colonies showed well-marked hæmolysis. One of the organisms presented all the features of the para-pneumococcus described by Natvig. A similar organism has also been found in the urethral secretions of healthy men by Pfeiffer. The other type of organism met with was a lanceolate diplococcus. This appears to be the organism described as occurring in four cases of puerperal infection by Foulerton and Bonney, and is indistinguishable from the pneumococcus. These organisms are, according to the observation of Natvig, in their cultural and biological features closely allied to the *Streptococcus pyogenes*.

Bacillus coli.—Organisms of this type were present in ten cases, always associated with other organisms, especially staphylococci and diplococci.

Proteus Bacilli.—These organisms were present in three cases. They have been frequently found in the lochial secretion and do not appear to be of much importance.

Character of the Puerperium.—In fifty-three patients the course of the puerperium was afebrile throughout. In five instances a rise of temperature was observed. The notes of these are briefly as follows: In two cases (No. 27 and No. 52) the temperature rose to 101.4° F. and 101.6° F. respectively on the evening of the day on which the culture was taken. This was the only rise of temperature during the puerperium. In both cases hæmolytic staphylococci, and in one instance a non-hæmolytic streptococcus, were cultivated from the lochial secretion. The temperature in each case fell to normal within twenty-four hours, and it is probable that the slight fever was the result of absorption produced by the introduction of the uterine tube. In one instance (No. 48), a multipara, labour was natural, but the temperature rose to 103.8° F. forty-eight hours after delivery. The uterine cavity was immediately explored and douched, clots being removed; this was followed by a gradual fall of temperature, which became normal on the seventh day. Cultures taken on the sixth day showed the presence of *diplococci* and the *Bacterium coli*.

Case 4.—The patient (No. 4), a multipara, was admitted during labour on account of severe accidental hæmorrhage. The membranes were artificially ruptured and delivery ensued without further bleeding. The temperature reached 100° F. on the second day, and a culture of the uterine contents showed the presence of hæmolytic staphylococci. On the third day the temperature rose to 103° F. The uterus was now thoroughly disinfected and this was followed by marked improvement,

the temperature becoming normal on the seventh day. This appears to have been an example of staphylococcic endometritis of a relatively mild type.

Case 5.—This is of considerable interest, as it is the only instance of infection associated with the presence of hæmolytic streptococci in the uterine cavity. The patient (No. 50), secundipara, was admitted to hospital during labour. Delivery was effected by forceps, and this resulted in some degree of laceration of the perineum. The temperature rose to 103° F. on the third day. Cultures taken from the uterus showed the presence of streptococci possessing very marked hæmolytic power. On the fourth day the uterine cavity was explored and douched. The temperature fell the same evening to 101° F., becoming normal in three days. The lochial discharge continued to be profuse and purulent for seven days. This case must be regarded as one of localized streptococcic infection of the endometrium.

These results show that organisms are frequently found in the uterine cavity as early as the second or third day after delivery. It is, however, impossible by the ordinary methods of obtaining lochial secretion to avoid contact with the cervical canal, and it seems probable that in many of these cases the organisms had not reached the upper portion of the uterine cavity. Our figures show a much higher percentage of cases of bacterial invasion in the early days of the puerperium than those of many other observers; this is perhaps attributable to the facts that we have used for cultures agar mixed with human blood, which is an extremely favourable medium for all forms of bacterial growth, and that the plates were placed in the incubator within a very short time after inoculation. The organisms present are precisely those which have been frequently found in the vaginal secretion during pregnancy. It is, however, probable that these are reinforced by bacteria from the vulva ascending into the vagina either spontaneously or as the result of manipulations during delivery. In a considerable number of cases operative interference was required, and this probably exerts some influence in facilitating the early invasion of the uterine cavity. On the other hand, organisms were found with almost equal frequency after normal labour, and even in cases in which no vaginal examination had been carried out.

The course of the puerperium was in the great majority of cases entirely uninfluenced by the presence of the organisms. It is probable that in many instances they only exist in small numbers and possess little, if any, pathogenic power; moreover, the uterine cavity speedily

becomes protected by an epithelial cellular layer, which, according to the observations of Krönig, is often fully developed by the end of the fifth day.

The primary object of this investigation was to test the conclusions of those observers who maintain that the presence of hæmolytic streptococci in the vaginal or uterine secretion after delivery must be regarded as pathognomonic of the existence of infection. There can be no doubt from the clinical observations of Schottmüller, Gonnet, Fabre, Fromme, and others that the streptococci present in cases of severe infection are characterized by great hæmolytic activity. This is not only observed in the organisms obtained from the uterine cavity, but is a characteristic feature of the streptococci which may be cultivated from the blood. It has also been observed that the streptococci present in the peritoneal fluid in cases of infection are always hæmolytic. Our results show that hæmolytic streptococci may exist in the lochial secretion without any evidence of infection. Streptococci were present in twelve cases in all—i.e., in 20 per cent. of the total number. In seven instances the organisms showed no hæmolytic power and corresponded to the type of *Streptococcus mitior* of Schottmüller or the *Streptococcus gracilis* of other observers. In all of these cases the puerperium was normal except that in one instance the temperature rose to 101.6° F. a few hours after the intra-uterine culture was obtained, and this is probably to be regarded as directly due to the introduction of the tube. In five cases hæmolytic streptococci were cultivated. In four of these the puerperium was afebrile throughout, but in one case (No. 43) febrile symptoms developed; these, however, remained limited to a superficial infection of the endometrium. It must therefore be acknowledged that streptococci indistinguishable from those found in severe forms of infection may exist in the lochial discharge of puerperal women even in the early days of the puerperium without causing any rise of temperature. This may be explained either by the fact that hæmolysis is often produced by organisms possessing a very slight degree of virulence or we must assume that these patients possess considerable resisting power to infection. It is therefore not possible to claim that the hæmolysis forms an absolute distinction between the saprophytic and slightly virulent organisms and those which are pathogenic. If, however, hæmolytic streptococci are discovered in a case of fever after delivery it is probable that they are the causal agents of the infection.

The question as to whether the saprophytic streptococci existing in the vagina before delivery are capable of developing infective power is of

great importance and cannot be regarded as absolutely decided. It is possible that under favourable conditions this may occur, but there can be no doubt that in almost every instance the highly virulent hæmolytic streptococci are introduced from without.

Since these observations were completed it is interesting to note that Fromme has made a communication on this subject in which he states that, as a result of further investigations, he has repeatedly found hæmolytic streptococci in the lochial discharge of afebrile puerperal women and also in many cases of mild infection. He has therefore proposed a method of distinguishing between the highly virulent hæmolytic streptococci and those possessing little or no infective power, which essentially depends on the variability of their rate of growth in a solution of defibrinated blood. This method is, however, very complicated and cannot be generally adopted. We must therefore conclude that we do not possess in the hæmolysis of streptococci any absolute indication as to the virulence of the organism.

It is remarkable that in the literature on the subject we have not found any reference to the occurrence of hæmolytic staphylococci in the uterine lochia. These organisms were present in almost half of the total number of cases, but in only one instance was any sign of infection observed. This occurred in the form of a superficial endometritis from which recovery speedily ensued.

CONCLUSIONS.

(1) In a series of fifty-eight cases in which the lochial secretion was examined between the second and ninth day after delivery, organisms were found to exist in the cervical canal and cavity of the uterus in 80 per cent. of observations.

(2) The organisms were mainly those which have been shown to be present in the vaginal secretion during pregnancy. There is, however, considerable evidence to show that organisms also ascend from without during the early days of the puerperium.

(3) The course of the puerperium was in the great majority of cases entirely uninfluenced by the presence of organisms.

(4) Streptococci were cultivated in 20 per cent. of cases and frequently showed marked power of hæmolysis.

(5) The presence of hæmolytic streptococci in the vaginal or uterine secretion cannot be regarded as in itself an indication of the existence of infection.

We desire to state that the bacteriological part of the work was carried out in the Public Health Laboratory of Manchester University, through the courtesy of Professor Sheridan Delépine, to whom we express our thanks.

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Analysis of Cases showing Organisms present in the Uterine Secretion during the Puerperium.

No.	No. of children	Day after delivery on which bacteriological examination was made	STAPHYLOCOCCI		STREPTOCOCCI		Diplo-coccus	Bacillus coli type	Pseudo-diplo-staphylococcus bacillus	Other organisms	Remarks
			Non-hemo-lytic	Hemo-lytic	Non-hemo-lytic	Hemo-lytic					
1	7	Second	—	—	—	—	x	x	x	—	Normal
2	1	Second	—	—	—	x	x	x	—	—	Normal
3	2	Second	x	x	—	—	—	—	—	—	Normal
4	6	Second	x	x	—	—	—	—	—	—	Accidental hemorrhage; temperature 102.6° F. on second day
5	2	Second	—	x	x	—	—	—	—	—	Normal
6	1	Second	x	x	—	—	—	—	—	—	Normal
7	9	Second	x	x	—	—	—	—	—	Proteus bacilli	Normal
8	1	Second	x	x	—	—	—	x	—	—	Normal
9	1	Second	x	x	—	—	—	—	—	—	Normal
10	5	Second	x	x	x	—	—	—	—	Anaerobic staphylococci and diplo-cocci	Placenta previa; version Normal
11	7	Second	x	x	—	—	x	—	—	—	High forceps
12	7	Second	—	—	—	—	—	—	—	—	Normal
13	1	Second	—	x	—	—	—	—	—	Anaerobic staphylococci and diplo-cocci	Breech presentation; forceps to head
14	1	Second	—	x	—	—	—	—	—	—	Normal
15	1	Second	x	x	—	—	x	—	—	—	Breech presentation; normal delivery
16	2	Third	x	x	—	—	—	x	—	—	Normal
17	1	Third	x	—	—	—	—	—	—	Proteus bacilli	Normal
18	17	Third	—	x	—	x	x	—	—	—	Normal
19	1	Third	—	x	—	—	—	—	—	—	Forceps
20	2	Third	—	—	x	—	—	—	—	—	Normal
21	3	Third	—	x	—	—	x	—	—	—	Normal
22	3	Third	—	x	—	—	—	—	—	—	Normal
23	3	Third	—	x	—	—	—	—	—	—	Normal
24	1	Third	—	—	—	—	—	x	—	—	Normal

Dr. DRUMMOND ROBINSON considered that the Section was hardly competent to discuss this paper as so few of the members possessed sufficient bacteriological knowledge. He thought that the weak spot in the paper had been indicated by the authors themselves. It was undisputed that the cervix always contained micro-organisms, and it appeared to him that it would always be impossible to obtain material from the cavity of the uterus without contaminating such material with micro-organisms introduced on the speculum from the cervix. With regard to this matter his observations were quite in accord with those of Whitridge Williams and others.

Obstetrical and Gynæcological Section.

January 14, 1909.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

Carcinoma of the Ovary.

By E. W. HEY GROVES, F.R.C.S.

MR. HEY GROVES showed a solid ovarian tumour removed from a girl, aged 16, who had had several attacks of pain from a torsion of its pedicle. The growth was ovoid and measured 5 in. by 4 in. It was smooth and firm, and was taken to be a fibroid, as there was no ascites or any other abdominal growths. The left ovary was healthy. Sections showed a typical carcinomatous structure; the fibrous stroma appeared to be destroying the epithelial cells in many places. The patient was well six months after the operation.

He also showed some viscera from a case of chorion-epithelioma. The patient, aged 51, had passed a vesicular mole in August, 1908, and continued to suffer from metrorrhagia until October, 1908, when she was admitted to hospital. Exploration of the uterus removed a soft polypoid mass growing from its anterior wall about 1 in. in diameter, and several hæmorrhagic nodules from the vaginal wall. These all proved to have the structure of chorion-epithelioma. The patient had some crepitations at the bases of both lungs, and expectorated blood-stained mucus every day. But as there is good evidence that early metastasis in this disease sometimes disappears after removal of the primary growth, the uterus was excised by the vaginal route. The patient made a good recovery, but died six weeks later from pulmonary failure. The uterus was quite natural in appearance, but from the base of the polypoid growth a narrow track of new growth ran to a nodule situate on the anterior uterine wall between it and the bladder. The peritoneal cavity contained no free fluid, but there were about ten

minute hæmorrhagic nodules in various parts. One was at the tip of the appendix, five on the surface of the liver near its anterior border, one on the lower surface of the diaphragm, and the rest beneath the parietal peritoneum. The largest of these was not bigger than a pea, and most were much smaller. Both lungs were densely covered by nodules of growth, each of which, in its wedge shape and hæmorrhagic section, exactly resembled an infarct. The nodules were so thickly studded on the pleural surfaces of the lungs that there was scarcely a square inch free from growth. There were no growths in the parietal pleura, and there was no increased pleuritic fluid. The manner of metastasis shown by the growth was much more like that of a sarcoma than that of a carcinoma, and was clearly effected by vascular embolism and not by lymphatic permeation.

Report of Pathology Committee.—The Committee have examined the specimen and sections, and agree with Mr. Groves's description.

The PRESIDENT (Dr. Herbert Spencer) said that the most remarkable feature in the specimen was the small amount of disease at its primary seat in the uterine body.

Cystic Subperitoneal Fibroid with Unusual Relations.

Shown by ARTHUR H. N. LEWERS, M.D.

THE patient from whom the specimen shown was removed was a married woman aged 40. She had been married seventeen years, but had never been pregnant. She was admitted into the London Hospital on November 9, 1908. She had noticed "a lump" in the abdomen for ten months, which had gradually increased in size till three months prior to her admission. Since that time she thought it had diminished slightly. She had had pain in the abdomen, which she thought was due to indigestion, for two years. Menstruation had been quite regular and normal, but for the last few months she had had great pain of a bearing-down character during the first days of each period. On examination the greater part of the abdomen was found to be occupied by a fluctuating tumour, irregular somewhat in outline, the highest point of which reached the costal margin in the left nipple line. On vaginal examination the posterior fornix was found to be bulged forward and downwards by an elastic tumour, which pushed the cervix and uterus forwards.

The sound passed the normal distance. On the above data, as may be supposed, the diagnosis arrived at was that the patient had an ovarian cyst.

Operation, November 17, 1908: On opening the abdomen a large cyst came into view, which was evidently not ovarian, but sprang from the fundus uteri, and was a large subperitoneal fibroid, which had undergone cystic degeneration. It was tapped, and a large quantity of thin, clear yellow fluid escaped. The anterior half of the cyst was free, except for one adhesion to small intestine above the umbilicus. The relations of the posterior half of the cyst were very complex. Behind the transverse diameter of the brim of the pelvis the anterior surface of the cyst was continuous laterally with the peritoneum, covering the iliac fossæ on each side. After incising the peritoneum laterally near the brim of the pelvis it was possible to enucleate the posterior wall of the cyst from its position deep in the pelvis, partly by the use of the knife and scissors. When this had been done, and the tumour had been freed from its deep attachments, the condition of the posterior half of the pelvis was similar to what occurs after the enucleation of a large retroperitoneal cyst. The iliac vessels and the ureters were in view on both sides. Two smaller cysts extended deeply from the lowest part of the main cyst, and had to be enucleated from Douglas's pouch in front of the rectum. At the level of the internal os uteri there was a free communication from one side to the other along the posterior surface of the uterus, the opening described being lined with peritoneum. I began the operation by dividing the cervix near the internal os, and then gradually dissected the cyst from its posterior attachments as already described. As there was such a large raw surface in the pelvis, I thought it better to make an opening in the posterior fornix, and provide drainage through the vagina. The peritoneum was then sutured over the raw surface. It seems difficult to account for the relations of the posterior half of the cyst. As it was a cystic fibroid springing from the fundus, it cannot have grown up behind the peritoneum. The only explanation seems to be that the posterior surface of the tumour was originally free and covered by peritoneum, and that at some time it became universally adherent to the posterior parietal peritoneum, so that when it was dissected away from its attachments the posterior part of the pelvic cavity was left bare of any peritoneal covering.

The patient made an uneventful recovery.

Description of the Specimen.—The specimen consists of the body of the uterus with a small portion of the cervix, and of a large cyst

springing from the back of the upper half of the portion of uterus removed. The left uterine appendages are with the specimen (the right uterine appendages were not removed). The cyst now, after hardening, has the following measurements: Extreme depth vertically, 7 in.; transverse measurement, $5\frac{1}{2}$ in.; antero-posterior, $4\frac{1}{2}$ in. The cyst wall is about $\frac{1}{4}$ in. in thickness. The above are the dimensions of the large cyst, but at the lowest part are two separate cysts, each about equal in size to a walnut. A portion of the main cyst wall was sent to the Clinical Research Association for examination. The report is as follows: "Microscopically this tissue is composed of interlacing bundles of smooth muscle tissue in a stroma of fibrous tissue. One half of the section is almost entirely fibrous, whilst the other contains the muscle. There is practically no degeneration in the muscular portion. We find no trace of an epithelial covering on either side of the section, and it is therefore very difficult for us to say which side bounded the cystic cavity."

A Large Retroperitoneal Cervical Fibroid.

Shown by ARTHUR H. N. LEWERS, M.D.

THE patient from whom the specimen shown was removed was a single woman aged 40. She came to see me on July 16, 1908. She had noticed a progressive enlargement of the abdomen for three years. A masseuse had noticed a lump there, and advised her to see a doctor. She had experienced a general feeling of discomfort, and of late noticed one area specially tender low down on the right side. She had also had a constant desire to pass water for some considerable time, and occasionally there was incontinence. Menstruation had been regular every three weeks, the period lasted a week, and the amount lost was not more than formerly. Sometimes she had had bad pain at the period; at other times the period had been painless.

On examination a large hard tumour was felt occupying the greater part of the abdomen and reaching the epigastric region, the prominence of the abdomen being similar to that seen in a woman seven to eight months pregnant. On vaginal examination the os uteri could not be identified; the posterior fornix was pushed low down by a hard fixed tumour, apparently continuous with that felt in the abdomen.

Operation, July 22, 1908: On opening the abdomen the body of the uterus was seen, normal in size and shape, situated on the front

of the tumour high up in the abdomen, and it was evident that the tumour was a retroperitoneal fibroid springing from the back of the cervix. The coverings of the tumour were very tightly on the stretch, as also were the vessels in each broad ligament running downwards almost vertically along the sides of the tumour—so much so that it was very difficult to find a free space to pass the pedicle needle in order to tie those vessels. Some of these veins were as large as the thumb. After ligaturing the vessels of the broad ligament on each side, the bladder was stripped down from the front of the tumour. Even the superficial transverse incision necessary to do this, contrary to the usual rule, caused a good deal of bleeding. The bladder was stretched so tightly, and its walls were so thin, that in stripping it down a rent about $1\frac{1}{2}$ in. in length was made in the bladder wall. The tear was at once closed with two layers of fine silk sutures.

The peritoneal capsule of the tumour laterally and posteriorly was then incised, and the enucleation of the retroperitoneal part of the tumour, which extended deeply to the floor of the pelvis, was proceeded with and completed. Numerous vessels required ligatures, and, owing to the extensive raw surface made as the tumour was enucleated, a good deal of blood was inevitably lost. Though in similar cases I have generally seen the ureters in this stage of the operation, they did not come into view. When the tumour was freed, the cervix was cut across at about what seemed $\frac{1}{2}$ in. above the external os uteri. As there was so large an oozing surface, I thought it better to provide drainage by splitting the ring of cervical tissue sufficiently to allow of a gauze drain being passed into the vagina. The peritoneal flaps were then sutured together over the raw surface left by the enucleation, and the abdomen closed. The abdominal wound had necessarily been very long, and extended several inches above the umbilicus. The patient made a good recovery. I have seen her quite recently (January, 1909), and she is quite well.

Description of the Specimen.—The specimen consists of a large retroperitoneal cervical fibroid, with the uterus and both sets of uterine appendages. The measurements of the tumour are as follows: Extreme length, $8\frac{3}{4}$ in.; antero-posterior measurement, $3\frac{3}{8}$ in.; transverse measurement, 7 in. The uterus is seen on the front of the specimen; the fundus uteri lies 1 in. below the highest point of the tumour. There is a pouch of peritoneum $1\frac{3}{4}$ in. deep behind the uterus, and from the lowest point of this pouch the peritoneum is reflected backwards over the tumour. The tumour is attached to the

uterus by a line of tissue $1\frac{3}{4}$ in. deep from above downwards. This tissue is looser in texture than that forming the bulk of the tumour.

The attachment corresponds to the upper two-thirds of the cervix, and encroaches to the extent of $\frac{1}{2}$ in. on the body of the uterus. The tumour in the recent state weighed 6 lbs.

Tumour (? Sarcoma)¹ of the Fundus Uteri.

Shown by LOUISA GARRETT ANDERSON, M.D.

MISS ANDERSON showed a specimen consisting of the whole uterus and the right appendages which she had removed by abdominal pan-hysterectomy from a married woman, three-para, aged 42. History: The patient lived in the country, and opportunities for thorough pelvic examination only occurred twice, in February, 1908, and again in the following September, when radical treatment was advised on account of increasing and severe menorrhagia which did not yield to drugs, abdominal pain, and rapid enlargement of the uterus. In February the uterus was bulky, and a nodule, thought to be a fibroid, the size of a small chestnut, was felt in the anterior wall near the right cornu. In September the uterus was as large as a four months pregnancy, and a separate nodule could no longer be felt apart from the general swelling. Abdominal hysterectomy was performed as soon as the necessary arrangements could be made. The operation was uncomplicated and convalescence satisfactory. Miss Anderson expressed regret that the left ovary had not been removed, but at the time of operation the tumour was regarded as innocent, and as the ovaries appeared to be perfectly healthy, it was thought better not to remove them both.

Specimen: The uterus was opened from behind in order to show the tumour, which grew from a wide base of attachment extending from the fundus over the greater part of the anterior wall. It projected into and filled the cavity. After hardening, the uterus was further divided by sagittal section. It measured 6 in. in length, $4\frac{1}{2}$ in. in width, $2\frac{1}{2}$ in. in depth from before back. The tumour formed a single well-defined oval mass lying in the musculature of the uterine wall. It was not encapsuled, but its limits were sharply defined. In front it encroached on the

¹ Fibromyoma; see report of Pathology Committee (opposite).

muscle, which was narrowed at this point; the mucous membrane covering it was intact. When fresh the cut surface of the tumour had a greyish swollen appearance, and it was cystic in parts, the cysts containing a mucoid substance. Microscopic examination by Miss Hamilton, M.D., showed that the growth was highly cellular and consisted of spindle-shaped cells irregularly arranged. Part of the muscular wall lay internal to the growth; the endometrium was normal. Diagnosis: Spindle-cell sarcoma. The specimen illustrated the difficulty of distinguishing a rapidly growing, and therefore cellular, fibroid from a spindle-cell sarcoma. In the present case the diagnosis was made after much consideration and some hesitation.

The fact was recalled that sarcoma of the fundus, although less rare than of the cervix, was a rare disease. Primary sarcoma of the fundus appeared to be less common than sarcoma secondary to fibroids. Statistics were quoted from the records of the New Hospital for Women: Out of the total number of in-patients suffering from malignant disease of the uterus who were treated in the wards from 1895 to 1907, 216 were cervix cases, of whom two were sarcomata, while forty-eight were fundus cases, of whom ten were sarcomata. The after-histories of the sarcomata appeared distinctly less favourable than those of the carcinomata.

Report of the Pathology Committee.—The Committee have examined the specimen and microscopical sections, together with an additional one made through the junction of the tumour and uterine wall, and are of opinion that the tumour is a fibromyoma, and that the unusually cellular character of parts of it is due to the presence of young, rapidly-growing muscular tissue.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) asked that the specimen might be submitted to the Pathology Committee to decide whether it was a sarcoma or a submucous fibroid; in the case of the latter, it would have been possible to remove it through the cervix.

Dr. EDEN said he had in his possession a specimen very similar to this one. The patient was an elderly woman 60 years of age; the menopause had been completed at 52, and for three months preceding the operation there had been a good deal of irregular hæmorrhage, but no pain. The uterus was digitally explored under anæsthesia, and finding that the cavity contained a large friable growth it was decided to remove the uterus at once without waiting for a microscopic report, as it was considered that this would be the best treatment, whether the growth was malignant or not. The abdominal operation was performed, and the appendages of both sides removed along with the uterus. The growth had the general characters of a degenerating

submucous fibroid in process of extrusion. In the main portion of the growth the evidence of malignancy was indefinite, but at the edge invasion and separation of bundles of muscle fibres by columns of round cells clearly showed that the growth was a sarcoma. He had endeavoured to determine whether the sarcoma had developed in a pre-existing fibroid or in the normal uterine tissues. The body of the tumour was accordingly carefully searched for the thick-walled arterioles and the striated arrangement of fibres so constantly found in fibroids. But these features were not detected in any part, and the point therefore remained undecided.

Case of Tubo-ovarian Abscess with Mammary Sympathies and Congenital Malformation of the Uterus.

By H. MACNAUGHTON-JONES, M.D.

THE pathological interest of this specimen consists in its being a typical example of a true tubo-ovarian abscess in which the abdominal end of the tube opens into the ovarian abscess cavity. Placed on the table for the Fellows' inspection are two other specimens, one showing the type of cyst which is apt to be confounded with the tubo-ovarian variety, and the second showing another typical example of the tubo-ovarian cyst in which the remains of the fimbriæ exist on the inner surface of the wall of the cyst; the latter was taken from a patient by Professor John Taylor.

The following is Dr. Cuthbert Lockyer's report on the specimen now shown: "The specimen is a tubo-ovarian abscess. Attached to its posterior surface is a mass of adherent omentum, and also a few peritonitic cysts. The tube is thickened throughout; its uterine end will not admit a fine probe, whilst its distal end is dilated and opens into the upper angle of an irregularly oval ovarian abscess. Between its two extremities the lumen of the tube becomes very dilated and tortuous. The general outline of the Fallopian tube is a V-shaped loop, which lies upon the top of the ovarian cyst. Sections have been prepared from the tube at its junction with the ovarian abscess, and also of the wall of the latter. The former shows the wall of the tube to be thickened by dense fibrous tissue, and its fimbriæ are seen infiltrated with round cells and very oedematous. The sections of the ovary show the structure of the abscess wall—*i.e.*, a fibrotic structure lined by oedematous granulation tissue."

The patient's age was 35; she was unmarried. The clinical interest attaches itself to the fact that the patient had been operated upon by a vaginal incision for pelvic abscess ten months before I saw her, and the abscess cavity drained. She had septic infection after the operation. During convalescence she suffered from severe mammary pains, especially in the right breast; these continued, and it was for this that she sought my advice.

I found in the right breast a large and fairly circumscribed mass which was sensitive to pressure. There was some general hardening of the left gland, but not of any surgical significance, and there was no involvement of the glands. Her general health was otherwise good. I removed the tumour in the right breast. This was carefully examined by Dr. Cuthbert Lockyer, and proved to be "interstitial fibrosis with cystic degeneration (involution of the gland tissue)." At the same time I also discovered by pelvic examination this condition: Complete absence of the portio, the os uteri being flush with the vaginal roof. Bimanually, the uterus itself was found to be infantile in its contour and proportion, and annexed to it was a mass, about the size of an orange, at the right side. Stretching from left to right was what felt like a tubular cord with the left ovary attached and displaced towards the middle line.

On questioning the patient, I found that the absence of the cervix had been commented upon at the time the abscess was drained; so much so, that she was questioned by the surgeon as to its possible previous removal. I came to the conclusion that I had to deal with a right adherent pyosalpinx with some old ovarian trouble, and that the small uterus was adherent to it, while the left ovary and tube were dragged out of their position to the right. This was exactly what was found at the operation, which was performed a fortnight after the mammary tumour was removed; the tubo-ovarian mass was intimately adherent all round; the omentum was drawn down and mantled over the tumour; the bowel was free and the appendix healthy. After separating the omentum, the pelvic cavity was carefully walled off, as in endeavouring to detach the mass there was an escape of rather fetid pus. The tumour was ultimately isolated, clamped off from the uterus and removed. The whole operation lasted an hour and a quarter. The anæsthetic, given by the scopolamin-morphia method, was chloroform. She made a rapid recovery.

The clinical lesson to be derived from such a case is the danger of trusting to vaginal drainage in the majority of cases of pelvic abscess

or suppuration of the adnexa, and the far safer and more certain treatment by abdominal exploration and extirpation.

DISCUSSION.

Dr. AMAND ROUTH did not agree with the speaker that vaginal incision of a pyosalpinx was bad treatment. On the contrary, he considered that if a pyosalpinx presented towards the vagina, posteriorly to the cervix, the vagina and the tube should be freely incised, and the tube packed and drained with iodoform gauze, as advised by Kelly and others. He considered that such an operation was easy, safe, and almost certain to permanently cure. If an ovarian abscess was found to be also present, an abdominal operation would be required, but this complication was infrequent.

Dr. MACNAUGHTON-JONES, in reply, said that the pain in the breast had ceased after the operation on the right gland, but there had been recently some complaint of pain in the left one. With regard to operation by the vagina, his point was that, in cases where there was an adnexal abscess with pathological changes in the adnexa, abdominal operation was preferable to that by the vagina in view of the future consequences to the woman which might result from leaving a diseased tube. He (Dr. Macnaughton-Jones) had been asked what he considered to be the origin of the abscess. He believed that this was in the first instance a tubo-ovarian cyst of inflammatory origin, in which suppuration of the ovarian portion subsequently occurred.

Some Notes on the Histology of the Smaller Fibromyomata.

By FLORENCE E. WILLEY, M.D.

WITHIN the last thirty years most theories as to the origin of young fibromyomata have had reference to the vascular system of the uterus, and have concerned themselves either with the origin of the cells which proliferate in these tumours or with the cause of such proliferation.

Writers of the first group affirm that primary changes in the walls of arterioles or capillaries give rise secondarily to myomatous tumours. These observers describe a vessel in the centre of young growths, concentric arrangement of fibres, a spherical shape, &c. Some have found fibroid change most marked at the periphery, as farthest from the source of blood-supply, while others have observed degenerations beginning centrally, after obliteration by pressure of the vessel from the walls of which the growth originated.

Klebs,¹ writing in 1876, describes their origin by proliferation of the muscular and connective tissue of the walls of arterioles. He says: The smaller fibromyomata less than the size of a pea show a concentric arrangement in all sections. Sometimes one may recognize in the middle a section of an artery of considerable size; more often, however, the larger vessels are wholly wanting, and the place where the smaller vessels lay are marked by depressions due to the shrinking of the loose connective tissue coat of the vessels. He says, further, that the larger collections of these tumours do not often arise from the union of several; more often it happens that the same process of new growth by which the primary fibromyoma arose is repeated in the substance thereof. Every single vessel with the muscular and connective tissue coats pertaining thereto proliferates again, and produces a second generation of nodes, which become incorporated in the original tumour, and extend it.

Not seldom the tumour is arranged in such a manner round the central vascular stem that it forms a wedge, the wide base of which lies on the surface. In other cases the formation of secondary nodes goes on in separate parts of the tumour, and then quite irregular masses result.

Pilliet² (in 1894) says: Fibromyoma begins in the uterine capillaries. The endothelium remains normal; the adventitia gives origin to a zone of embryonic cells which multiply and develop into rows of concentrically placed smooth muscle fibres arranged round the vessel. He declares that the constant element in the tumour is neither unstriated muscle, which may entirely disappear, nor connective tissue, which is only secondary, but it is the central vessel, and the uterine fibroma may be regarded as a benign tumour of vascular origin, a localized instance of exaggerated development of blood-vessels involving secondarily that of muscle and connective tissue. Fibromyoma is, according to Pilliet, an angiofibroma composed of capillaries, and he claims that this explains the development in fibromata of perivascular sarcoma, since both are tumours of a vascular series.

Pilliet says: The centre of the little growth always consists of a capillary vessel, round which the smooth fibres and connective tissue are arranged. The fibrous layers arise by transformation of the most peripheral muscular layers which are furthest from the vessel, and which, therefore, do not receive sufficient nourishment to allow of their normal development. Meslay and Hyenne³ agree with this description

¹ *Handbuch d. Path. Anat.*, 1876, i, 2, p. 884.

² *Bull. Soc. Anat. de Paris*, 1894, lxi, p. 4.

³ "Les dégénérescences des fibromyomes de l'utérus," *Ann. de Gynéc. et d'Obstét.*, Par., 1908, i, pp. 1-31.

in their paper published in 1898. The theories dealing with the cause of the proliferation may be roughly divided into those which describe young muscle-cells or embryonic remains in all uteri awaiting some stimulus to development and those which hold the infective nature of new growths. These latter have found the source of infection in a microbic endometritis, an endarteritis or peri-arteritis, or in tubal infection.¹ The observations made by these competent observers must always be of value, whether the deductions drawn from them stand the test of time and further discovery or not. It seems, therefore, important that at the outset of any investigation into the histology of these growths, which have been so intimately related to the vascular system, we should remind ourselves of any features of the uterine circulation peculiar to that organ.

The important points for our purpose may be briefly summarized as follows: The larger arteries are surrounded by a thick sheath of connective tissue, which encircles each artery and each arterial group, and separates it from the surrounding muscular tissue.² This sheath gradually becomes less abundant, the smaller arteries contracting closer attachment to the uterine parenchyma, while their walls are reduced in thickness till in the smallest arterioles one single muscular layer remains. The diameter of the uterine veins is larger than that of the arteries, and their section is more irregular in form. Unlike the veins of other organs their walls are thicker than those of the arteries, but do not consist of two coats of smooth muscle-fibres. They are true connective tissue walls, into which a few muscle-fibres from the uterine parenchyma extend. This is characteristic of uterine veins. The capillary circulation is peculiar to the uterus. Its principal characteristics are as follows:—

(1) An extremely irregular form which appears to depend upon the form and direction of the muscle-bundles.

(2) A most varying diameter, from capillaries so narrow that they appear impermeable by blood-corpuscles, to sinuses so large that the diameter much exceeds that of the arterioles and venules.

(3) Its wall consists of a single layer of cells, which are capable of changing with contraction and dilatation of the uterus from a thick palisade epithelium to extremely flattened cells like a true endothelium.

¹ Leguen et Marien, *Ann. de Gynec. et d'Obstet.*, Par., 1897, xlvii, p. 136; *Bull. Soc. Anat. de Paris*, 1896, lxxi, p. 329; *Comptes Rendus Soc. de Biol.*, Par., 1896, xlviii, p. 247; Hyenne, "Thèse de Paris," 1898; Clairre, "Thèse de Paris," 1900.

² "Recherches sur la anatomie et la physiologie vasculaires de l'utérus humain," *Bull. de la Soc. belge de Gynec. et d'Obstet.*, 1905 6, xvi, pp. 15-28.

The uterine muscle and the connective tissue between the bundles are abundantly supplied by this system of vessels, which cannot be called either capillary or cavernous, but is a fissural system characteristic of the uterus. Many of these vessels apparently do not function at all when the uterus is at rest in the normal state, so that there is in uterine muscle and mucosa a functional circulation, collapsed during periods of rest, and only coming into play with the peristalsis and congestion which accompany menstruation and pregnancy, and some pathological conditions.

In order to investigate the histology of young fibromyomata, sections have been cut out of the smaller tumours varying from $\frac{1}{2}$ mm. to 1 cm. in diameter. The material was in most cases prepared by hardening in formalin and embedding in celloidin, and the sections have been stained by van Gieson's method, and with hæmatoxylin and eosin. In some cases complete serial sections have been cut of the tumours. The smallest areas of proliferation were found in uteri from women between 30 and 40 years of age, which were removed on account of hæmorrhage or other troubles due to fibroids. The sections illustrate the following points:—

(1) *The Proliferating Cells in Growing Tumours are Muscle-Cells.*—

This point is determined by staining with van Gieson's method. Examination of sections so prepared shows that in uteri removed for rapid enlargement or hæmorrhage from women between 30 and 40, the smallest tumours consist of muscle-fibres; that is to say, the areas of proliferation show less connective tissue in proportion to the muscle than is found in non-proliferating muscular bundles in the same uterine wall. The nuclei of the muscle-fibres are oval or rod-shaped, or intermediate between these. They stain deeply with hæmatoxylin and show well-marked chromatin network and nucleoli. On the other hand, sections of even the smallest tumours in uteri past the menopause (removed post mortem from patients dying of other causes) show a considerable proportion of fibrous tissue, and the nuclei of the muscle-cells are narrow and rod-shaped.

(2) *Relation to the Vascular System.*—These areas of proliferation are related to the vascular system of the uterus in precisely the same way that all the muscular bundles of the uterine wall are related, and no more intimate connexion can be observed. In the smaller tumours no vessel with muscular coat is seen, but the proliferating muscle-cells appear to be in intimate relation with the endothelial lining of the capillary clefts. In larger growths vessels with one or two muscular

coats may be found, but in tumours of all dimensions the blood-supply appears to be chiefly maintained by the fissural capillary system. These clefts are often of considerable size and are lined by a single layer of endothelium. It has not been possible to show with certainty how many of these spaces in the larger tumours are capillary or lymphatics, but sections showing blood-corpuscles in the clefts demonstrate clearly

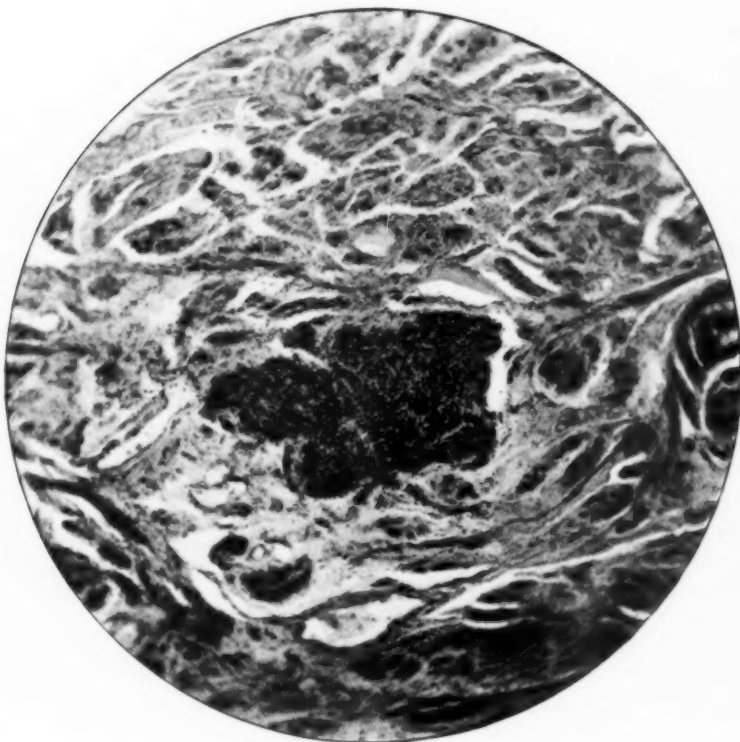


FIG. 1.

Interstitial myoma $\times 100$. Transverse section showing irregular shape and absence of capsule. Compare with surrounding muscular wall.

that some are capillary fissures. It will be seen that to some extent these observations confirm those of observers who describe the early proliferation as taking place in the adventitia of the uterine capillaries. Proliferation does take place immediately outside the capillaries, but the

cells concerned are not those of the vascular system, but cells common to the whole uterine parenchyma. The resulting tumours could only truly be described as angiofibromata if we regarded the whole musculature of the uterus, with its power of controlling the blood-sinuses, as analogous to the muscular wall of the arterial system.



FIG. 2.

Interstitial myomata $\times 100$. Section partly in the plane of the muscular bundles.
Showing irregular shape and absence of capsule.

In four cases the capillary vessels of the tumour show an excessive number of polymorphonuclear leucocytes. This is most marked in a case from which a sloughing fibroid polypus had recently been removed. The other three cases all had polypi in the uterine cavity.

(3) The *shape* of these areas of proliferation is most various, and in the youngest tumours appears to be influenced by the direction of the muscular bundles rather than by the relation of the muscle-fibres to vessels large or small. Thus the greatest irregularities are seen at those borders of the tumour where the section is in the plane of the muscle-fibres, the parts where a bundle is cut transversely being often smoothly bounded by a band of fibres running in the opposite direction.

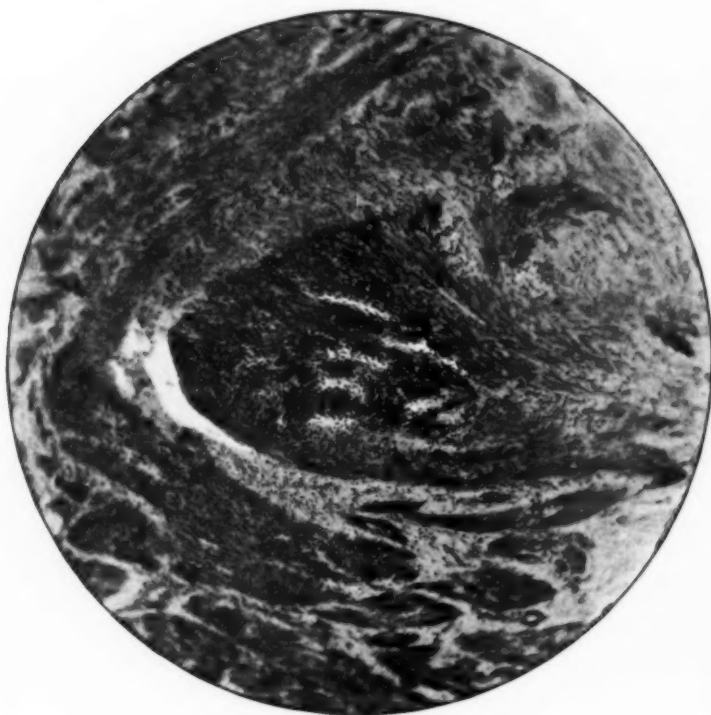


FIG. 3.

Interstitial myoma $\times 100$. Showing relation of cells to capillaries.

No concentric arrangement of fibres can be made out—the arrangement is varied in direction as in the normal musculature of the uterus. As the tumours enlarge they tend to become oval or spherical in shape, and at the same time a fibrous capsule is formed which marks off the little growth more definitely from the surrounding tissues.

(4) *Subperitoneal Fibroids* in many instances begin by proliferation in the muscle-bundle immediately beneath the peritoneum. In the smallest specimens cut no uterine wall is seen between the growing cells and the peritoneum, and no capsule is found on the free surface at a stage when a definite capsule is found between the growth and the uterine parenchyma. The mode of origin of these growths from the outer layers of the uterine muscle shows how extremely difficult it



FIG. 4.

Subperitoneal myoma $\times 100$. Showing relation to capillary clefts and muscular wall.

must be to determine that intraligamentous myomata ever originate in the unstriped muscle of the broad ligament and not from the outer muscular layer of the uterus.

(5) While pursuing this investigation sections have been cut of sixty to seventy uteri of all ages. In these uteri no fibroid seedling has been

found before the age of puberty, and those found in women long past the menopause consisted chiefly of fibrous tissue.

CONCLUSIONS.

(1) *That the youngest specimens of fibromyomata of uterus consist of proliferating muscle-cells.* It is quite true that very small tumours may be shown which consist largely of fibrous tissue, but, so far as these investigations have gone, they occur only in uteri past the menopause, or in uteri removed post mortem from women dying of other causes in whom there was no evidence that the tumour caused any trouble. The sections showing active proliferation were all found in women between thirty and forty years of age, and in all the condition of the uterus was causing symptoms. It seems reasonable to conclude that these uteri are representative of an active state of growth.

(2) *That the proliferating muscle-cells are those of the uterine parenchyma.* There is no evidence in the shape of the tumours, in their distribution, or in their relation to vessels to show that they originate in cells of the vascular system. To observe that the cells are intimately related to the uterine capillary system is only to recognize a fact true of any part of the uterine musculature.

(3) *That the stimulus producing the proliferation has some relation to the activity of the sexual organs,* since myomata are not found before puberty nor in an active condition after the menopause. As to the nature of this stimulus little or nothing is known, but in the examination of these specimens certain negative points appear. There is no evidence of an infective nature either in the endometrium or vessels. The endometrium shows chronic glandular hypertrophy, a condition common in all congestive conditions of the uterus, and in the smallest growths no excess of lymphoid cells is seen. It may, of course, be claimed that the proliferating cells which appear to belong to the uterine parenchyma are really embryonic remains. It seems more probable, however, that the whole uterus should be regarded as foci of cells awaiting some stimulus to development, and it may be that the stimulus which produces myomatous tumours in sterile uteri is allied to that which initiates the normal function of hypertrophy in presence of an impregnated ovum.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) said he could corroborate Mrs. Willey's statements as to the absence of capsules in many small fibroids, and had been surprised to hear it asserted that they always had capsules. He asked whether Mrs. Willey had examined microscopically some of the curious minute fibroids sometimes seen beneath the peritoneum which had the appearance of keloid scars.

Dr. HEYWOOD SMITH said that Mrs. Willey had quoted an authority who had considered that fibroids as a rule took their origin in the neighbourhood of blood-vessels. He (Dr. Smith) thought it might help to elucidate the subject if those who had large experience in hysterectomy for fibroids would state the percentage of those, the subject of fibroids, who were either unmarried or, if married, were sterile. He considered that fibroids were often the result of the suppression of sexual impressions, and that the intermittent and perhaps periodic congestions, leading in some instances to small interstitial hæmorrhages, might be the starting-point of the fibrous deposit, which was a morbid attempt to strengthen the part subjected to the hæmorrhagic strain.

Dr. MACNAUGHTON-JONES noticed that Mrs. Willey had excluded sepsis as an occasional factor in the etiology of some myomata. Mary Dixon Jones, who had very exhaustively studied the origin of myoma, considered that there was an infective starting-point in their origin, and other observers (Galippe and Landouzy) confirmed this. Alban Doran had contributed a valuable paper on the relationship of the young connective tissue to the muscle-cells, and the character of these cells in the tumour. Stanmore Bishop¹ had reviewed the entire literature of the subject, and had published the result of his examinations, entering fully into the arterial changes and the relation of the arteries and minute blood-vessels to the muscular fibres. From the very small size of the growth examined by Mrs. Willey he thought it was difficult to draw general conclusions as to the origin of the larger myomata.

Mrs. WILLEY, in reply, said she had observed no capsule in any of the smallest tumours; capsule formation appeared first at the point of greatest pressure. Subperitoneal seedlings on large tumours consisted chiefly of fibrous tissue. No evidence of sepsis was found in uteri with growing tumours, either in the endometrium or around the vessels.

¹ "Changes in Fibromatous Uteri," *Brit. Gyn. Journ.*, Lond., 1902-3, xviii, p. 270.

Late Results and Post-mortem Findings in a Case of Vesico-vaginal Fistula following Abdominal Panhysterectomy for Carcinoma of Cervix.

By Mrs. STANLEY BOYD, M.D.

IN our present state of knowledge it seems desirable to record any facts that may contribute to a right appreciation of the after-effects of extensive operations for the removal of cancer, and I have thought that the following case, completed by a post-mortem examination, may be interesting to those members of our Section who perform or recommend abdominal panhysterectomy for cancer of the uterus.

Mrs. J., aged 44, was admitted to the New Hospital for Women, January 12, 1904, complaining of hæmorrhage, often profuse, after coitus, and discharge from the vagina. Locally there was a thickened hard cervix and some limitation of movement of the uterus. The acid urine showed a trace of albumin, and urea 2.1 per cent. Removal of a wedge from the anterior lip under anæsthesia resulted in a diagnosis of squamous carcinoma of the cervix. The uterus was removed by Kelly's method of abdominal panhysterectomy four days later. The operation was rather difficult owing to dense adhesions about the appendages. The ureters were freely defined, and the separation of the bladder from the uterus gave rise to some difficulty on account of the firm fibrous nature of the connective tissue in this region, but there was no sign of new growth invading along the ureters or into the pelvic connective tissue. A good margin of vagina was removed with the uterus and connective tissue. There was troublesome oozing from veins in the vaginal vault, checked only by under-sewing. The pelvic cavity was drained by a strip of iodoform gauze, above which the pelvic peritoneum was sutured in a continuous line.

On examination of the specimen both anterior and posterior lips showed a marked hard prominence, the surface of which was reddened, but apparently not denuded of surface epithelium. This reddened area had a sinuous outline and slightly raised edges. The sutures put in when the wedge was removed had cut out. It will be noted, therefore, that the case was an early one and favourable for radical operation. Except for some bronchitis within the first three or four days, the

patient recovered well from the severe operation; but on the sixth day after operation a small amount of urine was noted to be coming from the vagina. Urine drawn off by catheter showed a cloud of albumin (as before operation). The abdominal wound healed by first intention, but the fistula persisted in spite of touching with pure carbolic. Only small quantities of urine passed by it, and a good deal more was passed naturally, but phosphatic deposit collected about both the fistula and the upper part of the vagina. The urine remained acid and always contained some albumin and pus-cells.

Cystoscopic examination a month after the operation showed the fistula as a small three-cornered opening in the neighbourhood of the right ureter, and the presence of a small papilloma on the left side.

The patient left hospital with the fistula, declining then, as also later, any further operation with a view to closing it; apparently the inconvenience was not sufficiently great to drive her to it. I used to see her at intervals, and gradually the right angle of the vaginal scar, in which the fistula was situated, contracted more and more, so as to form quite a pit.

Two years and nine months after operation, in October, 1906, she reported that she had had an illness, when she passed thick clay-coloured urine, after which less urine came by the fistula. I thought possibly a small abscess in the neighbourhood of the fistula had burst and been followed by some further cicatrization of the fistula. The urine showed a trace of albumin, but no pus. This kind of attack was repeated in 1907, and again in June, 1908, but I had no opportunity of seeing her during these illnesses, nor of examining this "clay-coloured" urine. After the attack in 1907 I felt a mass high up in the pelvis which I thought might be a suppurating gland, accounting possibly for the discharge of pus; it gradually subsided, and the last time I saw her, shortly after the attack in June last, the fistula was reduced to a tiny pin-point opening surrounded by a frill of mucous membrane. The catheter urine was then neutral and contained albumin. There was no recurrence of the cancer.

In August last I was asked to readmit her as she was extremely ill, and was said by her doctor to require immediate operation. Most unfortunately the New Hospital was closed for repairs and I was unable to do so, but she went into the Great Northern Hospital under Mr. Peyton Beale, who has very kindly allowed me to make use of the notes he has sent me.

On September 14 I received the following account of her death and

the autopsy from Dr. Skinner: "Mrs. J. died on September 12 after a severe illness lasting over a month. We had opened and attempted to drain a perinephric abscess about three weeks previously, but the symptoms remained severe, and all further treatment had to be purely palliative. Summary of post mortem: The bladder showed no signs of malignant disease. Very foul cystitis and a sloughy-looking papilloma were present. The left kidney and ureter were fairly healthy in appearance. The right ureter was completely occluded in a mass of cicatricial tissue just before its entrance into the bladder. The right kidney was totally disorganized, and was part of a large perinephric abscess which spread in various directions and mostly upwards (sub-phrenic), and had led also to a right-sided empyema." Dr. Shaw, Pathologist to the Great Northern Hospital, informs me that the macroscopic appearances were such that, he was quite sure, there was no recurrence of growth; no sections were cut for microscopic examination.

Thus this patient lived four years and nearly nine months after her operation, and remained free from recurrence of cancer, only to die just as certainly from the effects of her operation. The formation of the fistula in the first instance appeared to be the result of some sloughing, due either to injury in separating the tough fibrous tissue about the base of the bladder, or to dissecting out the ureter too far into the bladder-wall—I think the former. The later occlusion of the ureter was a gradual process, due possibly to the formation of an abscess in the connective tissue round about the fistula (which, if present, must have burst into the bladder), or to cicatricial thickening round about the denuded ureter. Is such a condition one of the dangers resulting from too clean dissection out of the ureter at its vesical end? Further back in the pelvis we are accustomed to carefully leave it in close relation with its covering peritoneum, and here its vessels enter; but in front, where it lies in and crosses connective tissue that we want to remove widely, it is often pretty freely denuded. It certainly was in this case. This is the only case of injury to the ureter or bladder, either directly or as the result of the sloughing, that I have had in my own practice in a series of thirty-two cases of this operation. Probably this patient need not have succumbed to this complication. I saw her only at intervals and at times unfavourable to a thorough systematic examination, and can find no note of attention having been paid to her renal regions—certainly a most unfortunate oversight. If I could have persuaded her to come into hospital again, such a systematic examination would have been the usual preliminary to cystoscopy, and the condition

of the right kidney would almost certainly have been discovered. A timely nephrectomy might have saved her life.

Report of Pathology Committee.—As the sections of the original growth cannot be found, and as the alleged fibrous tissue surrounding the ureter was not examined microscopically, the Committee are unable to express any opinion as to the nature of the case.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) said the publication of after-histories was of great value, especially in cases of carcinoma. It was unfortunate that a microscopic examination was not made of the fibrous tissue around the ureter and of the glands. He hoped that Mrs. Boyd would be able to submit the primary growth to the Pathology Committee, as the description of it appeared to be somewhat unusual for a squamous carcinoma. He had often wondered what happened to patients with ureteral fistulæ when the fistulæ closed, as Wertheim had shown they frequently did, and whether the extensive removal of the tissues around the ureters might not lead to cicatricial obstruction of those tubes. This case was a valuable contribution to the after-history of the extended abdominal hysterectomy.

Dr. LEWERS said that he had had a case of vaginal hysterectomy for carcinoma of the body many years ago, in which a ureteral fistula had developed shortly after the operation. Nothing of an active kind was done for it, and the patient went out of the hospital. She came up several months later to report herself, and the fistula was found to have healed spontaneously. She was seen several times subsequently, and there was no evidence of anything wrong with the kidney.

Dr. BLACKER had had three cases of ureteral fistulæ occurring after total abdominal hysterectomy. The first occurred in a patient who had had supra-vaginal amputation of the cervix performed, and who subsequently developed intense dysmenorrhœa and hæmatometra. At the operation for the removal of the uterus the right ureter was found densely adherent to the scar tissue round the cervix, and was freed only with considerable difficulty. The patient after the operation developed a ureteral fistula and ultimately septic changes in the corresponding kidney. Removal of the kidney therefore became necessary, and after this had been carried out she made a perfect recovery. The second case was one of Wertheim's operation for carcinoma of the cervix, and did not present any unusual difficulties. The patient, however, developed a ureteral fistula apparently from secondary sloughing of the duct. At the time she left the hospital one month after the operation the incontinence of urine had almost ceased; but it returned after she went home, and a few days later

an acute attack of suppression of urine developed with severe headache, vomiting and a high temperature. The suppression lasted for twenty-four hours, and then luckily passed off, the other symptoms abated, and the patient after being gravely ill for a week finally recovered, and when last seen a few days ago was quite well, all the urine being passed by the bladder, the fistula having entirely healed. In the third case operated upon by Wertheim's method for advanced carcinoma of the cervix the fistula became evident about the end of the first week after the operation. For about one month the patient remained fairly well, but after that time she developed a temperature, and the urine both in the bladder and that coming from the fistula was found to contain an almost pure culture of the *Bacillus coli*. Some eight weeks after the original operation the woman died with symptoms of pyæmia, and at the post-mortem examination a small abscess containing about half a drachm of pus was found at the site of the fistula. This was undoubtedly a serious complication of hysterectomy when it did occur, but happily in a considerable number of the cases the fistula healed spontaneously.

Mrs. BOYD, in reply, said that the limitation of movement was due partly to adhesion of the tubes, partly to the fibrous tissue about the bladder. She had spoken of the case as favourable, because the growth was limited to the cervix itself. In all her earlier operations she had followed Kelly's method, but in some of her more recent cases she had clamped the vagina after Wertheim. She referred to Wertheim's recent paper, in which he stated that in his last 158 operations he had seen necrosis of the bladder-wall in four, and of the ureter in ten cases. He further stated that if the operation was to be widely and satisfactorily carried out, a certain proportion of such accidents were unavoidable, but that spontaneous closure of fistulae might be looked for in most of them. She thanked Dr. Blacker for giving the result of his experience. There was great similarity between one of his cases and hers, as her case also died ultimately from the results of septic absorption. The treatment in his first case was, of course, most satisfactory, and was what she felt ought to have been done in her own case had the condition been earlier detected. She would be most happy to agree to the President's suggestion that the microscopic section of the cervical growth should be submitted to the Pathology Committee. The microscopic slide had been removed for re-examination by the late Pathologist of the New Hospital, who had confirmed the original diagnosis.

The role of the Perineal Body during Labour and the Conduction of Delivery in relation thereto.

(ABSTRACT.)

By R. H. PARAMORE, M.D.

THE perineal body plays no part in the support of the viscera, nor does its rupture facilitate prolapse. This opinion depends on its thinness, the occasional occurrence of its central rupture, and its frequent absence in multiparæ (previous rupture), but more especially because its complete rupture is frequently not followed by prolapse. Yet the perineal body exercises a far-reaching influence during childbirth, which is neither necessary nor good. That it is not necessary is shown by the occurrence of normal labour in its absence (previous rupture); that it is not good is the task I have undertaken to show.

The foetal head passes through the pelvic cavity flexed and is born by extension. In its descent through the outlet of the pelvis, the pelvic floor becomes transformed into a broad gutter-like declivity, at the lower end of which the posterior commissure of the pubo-rectalis muscle is found. This muscle arises from the pubes and passes backwards by the side of the vagina, perineal body and anal canal, and encircles the gut in the perineal flexure. It is not displaced by labour as much as one would think; at the acme of birth its posterior commissure is at least 5 in. from the fourchette. The dilatation of the anus (1 to 1½ in.), and the increase in length of the base of the perineal body (3 in. or more), shows how much the tissues below the pelvic floor stretch. The distance from the lower margin of the pubo-rectalis to the posterior margin of the anus makes up the 5 in. The recognition of the position of this muscle posteriorly, so far from the vulva, is necessary to realize the part played by the perineal body in the continued extension of the head during labour.

The tension of these thinned out perineal tissues determines the more forward projection of the anterior segment of the head, the head being ovoid in shape. Owing to the stretching of the perineal tissues, the plane of the vulvar aperture becomes extremely oblique and approaches the coronal plane. But the plane of the pelvic floor aperture is not nearly so oblique; it is not far removed from the horizontal plane;

the location of its posterior commissure *above and behind* the anus demonstrates this. The posterior extremities of these two planes are considerably distant from one another, but their anterior extremities are close together. Since these apertures, although superimposed, are not in planes parallel to each other, it follows that an ovoid body passing through them at the same time must, whilst it is passing through one with its long axis perpendicular to the plane of that aperture, pass obliquely through the other. As the head reaches the sphincter muscle of the pelvis (pubo-rectalis) it begins to pass through it with its long axis at right angles to the plane of the aperture. When, however, its anterior extremity has extended beyond this aperture it impinges upon the perineal body. The resistance of this determines the continuance of its extension (primiparæ). The deflection of the advancing pole of the head is easily accomplished on account of its length. So that, if the head enters the vulvar aperture, now much displaced, with its long axis at right angles, the head must have changed its course, and its long axis can no longer be passing at right angles through the plane of the aperture of the pubo-rectalis. Thus this muscle, instead of embracing a circle of the foetal head, finds itself applied to an ellipse, the circumference of which is greater than that of a circle. Therefore the muscle is subjected to an increased stretching.

If, however, the vulvar aperture is destroyed by a laceration, the movement forwards of the anterior extremity of the head does not occur; an oblique diameter does not occupy the pelvic floor aperture, and this, therefore, escapes a greater distension.

That the perineal body may in this way bring about changes which result in prolapse is shown by the occurrence of prolapse with the perineum untorn, but so stretched that it is evident it has played this part during childbirth. On the other hand, complete rupture of the perineal body is often seen with no prolapse. It is evident that a perineal tear, by allowing birth with the least possible distension of the muscle, may undoubtedly in many cases prevent an injury which predisposes to prolapse; but as a factor to this end it is to be regarded in the same light as the size of the foetal head.

If these perineal tears are prevented during childbirth by opposing the advance of the head or directly supporting the perineum, no good other than a preservation of its tissues is effected, but much harm may be done to the actual pelvic floor. Indeed, an early perineal tear, beginning at the fourchette, is often a blessing in disguise. Perineal tears,

when they do not involve the sphincter ani, are trifling injuries, and should be treated in the same way as other superficial wounds. The only reasons for suturing them is to check hæmorrhage and prevent infection (of the wound and of the vagina). When the tear is complete the gut-wall must of course be repaired; but from the point of view of subsequent visceral support, suture is unnecessary, for we know that prolapse frequently does not occur even when the rupture is complete. This is explained by the fact that the main mass of the pelvic floor musculature passes behind the anal canal and remains intact in spite of such a tear. Its activity is thus continued even when the vagina and gut open into an artificial cloaca.

The continued extension of the head can be prevented by adopting the method introduced by Toff. When the head appears at the vulva, two fingers are placed between it and the pubes, and traction exerted backwards; simultaneously the head may be pressed downwards and forwards from above the anus. This method not only preserves the perineal tissues from being torn, but it also prevents an injurious distension of the muscle above it. In practice it is easy to carry out, considerably shortens labour, and is a real help to the patient.

DISCUSSION.

Dr. GRIFFITH expressed his opinion that Dr. Paramore was running his hobby, of the importance of the musculature of the pelvis as against all other structures, too far. To say that the perineum was of no importance as a factor in the support of the pelvic viscera was not altogether supported by evidence. Rupture of the perineum and sphincter might be complete without prolapse, and yet all operators had met with fortunate cases where the thorough repair of such a laceration occasionally alone led to a complete cure of a considerable prolapse, although, as everyone knows, it very frequently failed. The value of the perineum as one of the factors in determining rotation of a descending part of a foetus, whether head, breech, or shoulders, was undoubted. Where the perineum was extremely tough and undilatable, and the cause of delayed delivery, he advocated median incision rather than the use of the forceps.

Dr. MACNAUGHTON-JONES said that the conclusions drawn by Dr. Paramore would lead to a reversal of all previous obstetrical teaching. He himself had taught students that the perineum played an important role in the delivery of the foetal head, as a point of resistance in favouring its delivery

in the long axis of the outlet. While not following Dr. Paramore through his various differentiations of the action of the pelvis muscles, and acknowledging the logical conclusions which he drew from his view of these, he (Dr. Macnaughton-Jones) would be sorry to think that it would go out from that Section that there should be any divergence from the obstetrical rule of at once closing a perineal laceration, quite independently of any septic risk, or, as Dr. Paramore had said, any cosmetic effect. Dr. Paramore would acknowledge that rectocele and vesicocele constantly occurred with what Howard Kelly called "relaxed vaginal outlet," where there was no apparent laceration, but in which the perineum was weakened. The same lesions occurred from laceration. He had just seen a patient in whom there was both a severe rectocele and cystocele, with laceration of the sphincter, but no uterine prolapse or retroversion. Many years since Goodell had taught that the old method of "supporting the perineum" was most dangerous, inasmuch as it tended, by pressure on the devitalized tissue, to cause rupture. He advocated "relaxation of the perineum with the thumb acting on the head in the vagina, and the forefinger inserted into the rectum drawing the perineum forward."

Dr. AMAND ROUTH thought that scientific observers should be slow to form the opinion that any part of the human frame was without its uses. Till myxœdema was understood the use of the thyroid gland was not known. They were beginning to know something of the uses of the thymus gland, and in a recent paper Mr. Keetley had advocated the transplantation of a diseased appendix rather than its removal, with the idea, amongst other considerations, of preventing that senility which removal of the appendix was said to induce. There can be no possibility of doubt that the perineal body served many useful purposes. It gave support to the anterior wall of the rectum and the posterior wall of the vagina, and indirectly to the anterior wall of that passage. Its integrity was essential to the preservation of the tone of the vaginal and vulvar outlet. In labour the yielding elastic tissues of the perineal body enabled the vulvar outlet to become distended without laceration. What would happen in labour if the vulva and vagina had not a perineal body posteriorly? Supposing the circumference of the vaginal outlet to be the same size as now, and its antero-lateral structures and their relations the same, the recto-vaginal septum would still be the part most influenced by the pressure of the advancing head, and, being unable to yield to any appreciable extent, it would inevitably tear, and in most cases the tear would extend into the rectum. The presence of the perineal body tends to prevent this accident. The method advocated by the author in occipito-anterior positions of vertex presentations, viz., of inserting two fingers between the occiput and the pubic arch, was an excellent one, but not for the reasons given by the author. It was not because the procedure tended to induce flexion, as the author said, but because it brought the occiput lower down, and permitted the suboccipital region of the head or even the neck of the child to be applied to the back of the pubic arch, thus greatly facilitating extension, and lessening the engaging diameter

of the head. He hoped that Dr. Paramore would not allow his theories to prevent him in his practice from doing all he could to preserve the integrity of the perineal body.

Mr. HEY GROVES strongly dissented from the author's main contentions. The perineal body served as an important tie between the levatores ani muscles, each of which separately formed a sling running from the pubes to the coccyx round the rectum and vagina. If the perineum was ruptured each levator muscle tended to fall like a curtain against the pelvic wall, and this predisposed to a descent of the pelvic viscera. The efficiency of the pelvic floor as a visceral support depends, not only on the strength of the levatores ani, but upon the union between the muscles of the two sides. It was a consideration of this factor which led Mr. Hey Groves to devise the operation described by him before the Obstetrical Society, four years ago, for the cure of vaginal cystocele. This consisted in the union of the levatores ani muscles between the bladder and vagina. Later experience had not only shown the efficiency of this operation, but had led to the same principle being extended to other conditions of prolapse associated with a ruptured or stretched perineum. That is to say, every such case was treated not simply by repair of the perineum but by a deliberate exposure of the adjacent edges of the levatores ani muscles, which were sewn together between the anus and vulva. The fact that the perineum is sometimes torn right into the rectum without prolapse resulting merely represents an exception to the rule illustrated by the vast majority of cases where prolapse follows perineal rupture. The pelvic viscera are kept in place by several factors, and whilst undoubtedly the levatores ani are the most important of these, the perineum is hardly less so, serving as it does to tie the levator muscles together and so increase their efficiency.

Dr. R. H. PARAMORE, in reply, said he felt sure that when clinicians commenced to investigate the pelvic floor musculature, and to examine it in the living body, they would abandon the views hitherto held as regards the visceral support and the question of prolapse. If the perineal body was of the importance that Dr. Griffith and other speakers had imputed to it, these gentlemen must show cause why prolapse did not always result when complete rupture occurred. It was evident that some other reason must be looked for to explain the good results (in some cases) of perineorrhaphy in prolapse. That reason was to be found in the fact that during the surgical treatment and the process of repair, other factors had come into play. The patient had been put to bed. The pressure within the abdomen had thereby been markedly diminished, and the force with which the viscera had been previously thrust down on the pelvic floor had been correspondingly lessened. Dr. Griffith had stated that the resistance of the pelvic floor caused the foetal head to rotate during childbirth. Dr. Paramore said that he had given considerable thought to this question of internal rotation, and denied that the resistance of the pelvic floor caused a rotatory movement to the head during childbirth, an

opinion in which he was supported by no less an authority than Bumm¹; and said that the resistance of the soft parts could only cause projection forwards—that is, extension—but this movement was not rotation. This, however, was a question that needed more detailed inquiry. Mr. Hey Groves had stated the perineal body was important because he believed that the levator ani muscle on one side was attached to the levator ani of the other side by means of the perineal body, without which connexion both muscles were useless. It was, indeed, true that the pre-rectal fibres of one side joined those of the other in the perineal body. These fibres, however, were inconspicuous; they could not be felt by palpation. He (Dr. R. H. Paramore) felt the method of preventing extension of the head during childbirth was of the greatest possible value, and he hoped members of the Section would give it a trial. If they did so, not only would they preserve the perineal body; but they would prevent an increased stretching and possible injury of the pubo-rectalis muscle, a structure which was all-important from the point of view of the future welfare of the patient and her visceral position.

¹ Bumm, "Grundriss zum Studium der Geburtshülfe," Weisb., 1905, p. 206.

Obstetrical and Gynaecological Section.

February 11, 1909.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

A Venous Aneurysm on a Uterine Fibroid.

By HERBERT R. SPENCER, M.D.

THE specimen exhibited (*see figure*) is a fibromyomatous uterus with an aneurysm on a large vein coursing over the surface of one of the sub-peritoneal tumours. Large veins on the surface and in the substance of these growths are not uncommon. They are often thin-walled, and I have known one to perforate spontaneously, flooding the peritoneum with blood to such an extent as to endanger the patient's life, which was only saved by abdominal section. A few cases of rupture of veins either on the surface of fibroids or in the broad ligaments have been recorded by other observers; but I have not found, in connexion with these growths, a record of a venous aneurysm which would seem specially to predispose to rupture.

The patient, a single woman aged 32, was admitted to University College Hospital on October 20, 1908, complaining of a lump in the abdomen, which she had noticed for a year. It had steadily increased in size, but had given rise to no symptoms except constipation. Menstruation began at the age of 13, lasted from three to five days, was slight in amount and unattended with pain. There was no discharge between the periods. The patient had recently recovered from an attack of pelvic peritonitis, but was in good health and gave no history or sign of rheumatism, gout, or syphilis. There were no visible varicose veins in any part of the body, and the arteries, heart, lungs and kidneys appeared to be healthy.

On examination a large tumour was found in the abdomen, extending upwards to $6\frac{1}{2}$ in. above the pubes on the left side, and 6 in. on the right. It was a typical fibroid with several nodules on its surface. On vaginal examination the cervix was found to be pushed forward, so that it was close to the symphysis. Behind it, filling the pelvis to within $\frac{1}{2}$ in. of

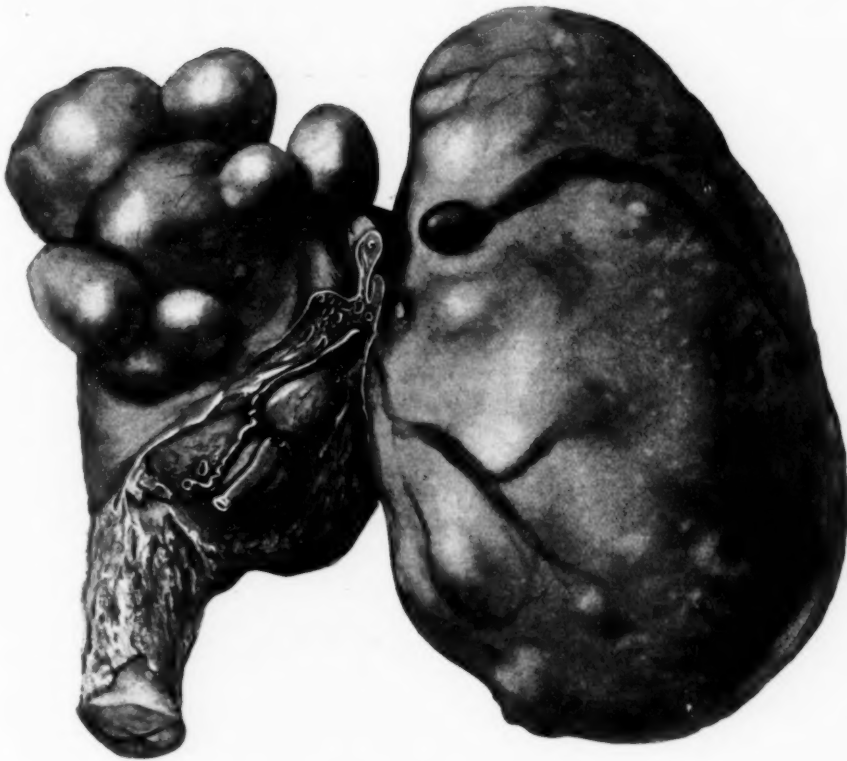
the pubes, was a very hard tumour, continuous with the tumour in the abdomen. On account of the size and the increase of the tumour, and the danger of pressure on the bladder and ureters, it was decided to remove it. This was done by total abdominal hysterectomy on October 24, 1908. The inflamed right tube and ovary, covered with lymph, were first removed, and the delivery of the large pelvic tumour and the opening of the vagina were thereby facilitated. The peritoneum was closed with a purse-string suture, the vagina was left open; no gauze drainage was employed. The patient made a good recovery; the wound healed by first intention. The patient left the hospital on November 16, 1908, and was seen in excellent health in February of this year.

The uterus, with the tumour, weighed 3 lb. 6 oz., and, apart from the large retro-uterine tumour, measured 14 cm. by 7 cm. by 9 cm.

The right tube was thickened, and it and the ovary and part of the back of the tumours and uterus were covered with adhesions. The ovary contained a corpus luteum 2.5 cm. in diameter. The uterus had numerous subperitoneal tumours projecting from its surface, mostly varying in size from that of a marble to that of a walnut or a billiard-ball; but a very large tumour projected from the posterior aspect of the upper part of the body, and had a basis of attachment 6 cm. in diameter. It measured 17 cm. by 11 cm. by 10 cm.

Coursing over the surface of this tumour were seen two thin-walled veins, which felt as if they lay in channels in the tumour. The veins were from 3 mm. to 5 mm. in diameter, and the trunk of the larger vein close to the cut left Fallopian tube was dilated into a venous aneurysm, 13 mm. by 10 mm. in diameter and projecting 8 mm. from the surface, with its long axis in the direction of the vein. The wall of the aneurysm was thicker and more opaque than the rest of the vein. The aneurysm, which contained no clot, had two openings communicating with the vein on its deep surface; they were somewhat nearer together than the length of the aneurysm, owing to the bulging of the latter. A microscopic section of the vein and tumour was taken at a distance of 6 cm. from the aneurysm. The wall of the vein had undergone hyaline degeneration; the tumour was a fibromyoma showing hyaline and mucous degeneration.

The portio vaginalis and external os were normal. On bisecting the uterus the cervical canal and the corporeal canal each measured 6 cm. in length. The mucous membrane of the body was hypertrophied. There was a small submucous fibroid 1.5 cm. by 1 cm. in diameter in the upper part of the body.



The uterus, with numerous subperitoneal fibromyomata projecting from it, is seen from the left side (three-quarters actual size). The large tumour was impacted in the pelvis. Coursing over it are two large veins, on the upper of which, near the cut Fallopian tube, is seen the aneurysm. Three slight bulgings of the vein are seen posterior to the aneurysm. The cut vessels of the broad ligament are distended. The utero-ovarian artery has been cleaned, and the uterine vein is seen to be flattened by the presence of a small fibroid beneath it.

Dr. Herbert Spencer: A venous aneurysm on a uterine fibroid.

With regard to the causation of the aneurysm it would seem as if the thin degenerated vein had yielded to the abnormal pressure produced by obstruction to the venous flow. Evidence of this obstruction may be seen in the specimens, and is well illustrated on the plate, in the compression of the ovarian veins between the large tumour (which was impacted in the pelvis) and the uterus, and of the uterine vein which is seen to be flattened by the pressure of a small fibroid on the lower segment.

A Uterus with Two Interstitial Fibromyomata; one showing Red Degeneration, the other Normal.

By FRANK E. TAYLOR, M.D.

H. B., AGED 45, was admitted into Chelsea Hospital for Women, under the care of Dr. A. E. Giles, on January 8, 1909, complaining of abdominal pain and swelling. She has been married twenty-one years and has had three children, the last seventeen years ago. The patient has been ailing since November, 1906, when she had a severe attack of pain in the lower abdomen which confined her to bed for three weeks. Since then there has been a gradual and progressive enlargement of the abdomen. Menstruation has been very irregular and scanty for the last two years, there being amenorrhœa for six months, which was terminated three weeks ago by the appearance of a scanty pink flow lasting two days. Notwithstanding the presence of abdominal pain the patient was able to keep about until quite recently, when she was again confined to bed for two weeks on account of an attack of severe abdominal pain. She has also had much bearing-down pain. In December last the patient had retention of urine necessitating the passage of a catheter. The bowels were relaxed. During her illness the patient has suffered from dyspepsia and has lost flesh.

On examination a rounded firm mass was felt rising from the pelvis into the right iliac region to within $1\frac{1}{2}$ in. of the umbilicus, and a second swelling was felt on the left side lower down and of less firm consistence. The cervix was drawn high up in the pelvis and was small, and the connexion of the tumours with the uterus was somewhat obscure.

On January 11 Dr. Giles performed cœliotomy and removed the fibroids and uterus by supravaginal hysterectomy, the left uterine appendages being also removed. During convalescence symptoms of

intestinal obstruction developed, but were overcome by the administration of purgative enemata.

The specimen removed consisted of the body of the uterus, enlarged by fibroids, with the left normal appendages attached. It weighed $4\frac{3}{4}$ lb. In the posterior wall of the uterus was a globular fibromyoma 4 in. in diameter, encapsuled, softish, and a uniform mahogany red colour. In the anterior wall of the uterus was also a globular fibromyoma 4 in. in diameter, encapsuled, hard, white, whorled and free from any sign of degeneration. The uterine cavity was not encroached upon by either tumour, and the endometrium was not thickened. Histologically, the red fibromyoma showed loss of the outlines of the muscle-fibres, with feeble and diffuse staining, with nuclear "ghosts" and disappearance of the nuclei. Dr. Taylor considered that this specimen showed several points of interest both from a clinical and pathological standpoint.

The association of pregnancy with red degeneration had been much dwelt upon; in this case the last pregnancy had occurred seventeen years previously—at too remote a period to have any significance in this condition. The symptoms usually present in cases of red degeneration are pain, tenderness, and rise of temperature and cachexia from absorption of toxic products from the degenerated tissue. Pain, as detailed above, was the most marked feature in the history of this case, whilst tenderness was not elicited by palpation of the tumour. Pyrexia was absent during the patient's stay in hospital, whilst loss of flesh during the illness may be taken as evidence of cachexia.

Pathologically, the fact that of two fibromyomata in one uterus identical in size and position (one being interstitial in the anterior and the other in the posterior wall) one should show typical red degeneration whilst the other was normal could only be accounted for on the assumption that some local nutritional disturbance in the degenerated tumour was responsible for this condition. In this connexion it was interesting to note that at the last meeting of the North of England Obstetrical and Gynæcological Society Lorrain Smith and Fletcher Shaw, as the result of the examination of four specimens, had ascribed red degeneration of uterine fibromyomata to thrombosis of the vessels of the tumour. From the examination of about thirty specimens of this condition he (Dr. Taylor) denied that thrombosis of vessels occurred in the majority of these cases, though he had seen thrombosis of the vessels and even extravasation of red blood-corpuscles amongst the tissues of the tumour in a few cases. Further, the suggestion that some infective micro-organism was the cause of this form of degeneration could be negatived

by the fact that repeated inoculations of culture media had invariably remained sterile.

Lastly, Dr. Taylor had endeavoured to ascertain the nature of the colouring matter present by squeezing out the tissue-juices in a muscle-press, and after suitable dilution examining the fluid so obtained spectroscopically. He found the two-banded spectrum of oxyhæmoglobin, which was replaced by the one-banded spectrum of reduced hæmoglobin on the addition of such reducing agents as Stokes's fluid and ammonia sulphide. He therefore concluded that red degeneration of uterine fibromyomata was an aseptic necrobiosis of the tumour-cells accompanied by a different staining of the tissues with hæmoglobin due to some local disturbance of nutrition.

A Modified Champetier de Ribes Bag, made entirely of India-rubber.

By VICTOR BONNEY, M.D.

THE circumference of the bag varied with the amount of water injected into it. With 10 oz. it measured ten inches; with 14 oz., eleven inches; with 19 oz., twelve inches; and with 23 oz., thirteen inches in circumference. He claimed the following advantages for it: (1) It could be varied in size according to the requirements of the case, particularly in regard to the membranes having ruptured or not prior to its introduction; (2) it formed a dilating wedge more comparable to the natural bag of membranes; and (3) it retained its shape when empty and was thus more durable than the form of bag in common use.

Dr. DRUMMOND MAXWELL asked for information on the measurements of the bag when it had reached the maximum contents of fluid mentioned by Dr. Bonney. He had been surprised to find lately the great variance of size of bags sold officially as Champetier de Ribes bags. The diameter of some of them barely reached 3 in. measured across the superior surface, and he had met with one bag that considerably exceeded the 4 in. With this last bag an unfortunate complication of labour had been associated. It had been inserted in a case of placenta prævia with the vertex presenting; on the bag being expelled, the vertex descended and the patient delivered herself spontaneously without further manipulation. An examination of the patient shortly after delivery, on account of her collapsed condition, discovered a tear of the lower uterine segment. The patient was brought to hospital, the rent in the

broad ligament plugged, and after a protracted convalescence she recovered. In the absence of any further manipulations than the introduction of the bag, it was reasonable to attribute the tear to the faulty measurement of the bag. Surely the outstanding advantage of the original bag, with its fixed measurement and content, lay in the fact that when expelled the accoucheur knew he could proceed safely to any form of extractive manipulation. If some of the small bags became popularized he would expect to hear of a good many tears of the cervix, while if larger ones were used, capable of indefinite expansion, there would be an unreasonable delay in waiting for their expulsion.

Sarcoma of the Mesosalpinx.

By FREDERICK McCANN, M.D.

SARCOMA primarily involving the Fallopian tube is a rare disease. I have only been able to find records of five cases—viz., those published by Gottschalk, Max Sänger, Emil Senger, von Kahlden and Janvrin, and two of carcino-sarcomata quoted by Peham.

In May of last year (1908) I was fortunate in meeting with a curious example of a sarcoma invading the right Fallopian tube. The patient, a widow aged 52, was extremely anæmic, feeble and emaciated, with sunken eyes and hollow cheeks, and a marked growth of hair on her chin and upper lip. She gave the following history: Her menstrual periods had ceased for two years, but in October, 1907, she commenced to lose blood from the vagina, and this had continued at intervals, alternating with a yellow-green discharge. A month prior to my visit she complained of abdominal pain, swelling and sickness. Indeed, she was conscious that a "lump existed in her inside." Her own doctor had noted that she was steadily losing flesh and rapidly becoming more anæmic. She had had three children, the youngest being 26 years of age, and had been a widow for twenty years. On examination her skin was of a yellowish hue and the mucous membranes pallid. Indeed, she presented the appearance of a patient with pernicious anaemia. The pulse was rapid and feeble, the appetite impaired, and her general condition was one of extreme weakness. The abdomen was distended and tender on palpation, but a distinct elastic tense tumour could be detected reaching to the level of the umbilicus and rising out of the pelvis. Bimanually the lower segment of the tumour was found to occupy the right side of the pelvis. The uterus, which was not enlarged, and could be palpated apart from the tumour, was situated in front and to the

left. A diagnosis of malignant ovarian disease was made and operation recommended.

On May 29, 1908, I opened the abdomen and at once a quantity of dark blood welled up through the abdominal wound. The tumour was then exposed and found to be of a deep livid colour with omentum and intestine adhering to it, whilst the peritoneum was thickened and œdematous. The adhesions were separated and ligatured, and a quantity of blood-clot was removed from the abdomen. When the tumour was isolated it was seen to be connected with the right Fallopian tube, the corresponding ovary being small and apparently healthy. The pedicle, which was fairly narrow, was secured by separate ligature of the vessels and subsequent covering with peritoneum. The body of the uterus was not enlarged, and the appendages on the left side also appeared to be healthy. All blood-clot was removed from the abdominal cavity, and after washing out with saline solution the abdominal wall was sutured in layers. The operation was done rapidly on account of the feeble condition of the patient; she, however, began to improve when the bleeding was arrested. Her further improvement was most marked. She took her food well and her colour returned. The wound healed well, and all the stitches were removed. The temperature had never been above 99° F. Her friends were amazed at her rapid progress. This, however, was short-lived, for on June 14 I was again called to see her. A sudden change for the worse had occurred the previous day, accompanied by pallor and abdominal distension. I found the abdomen distended, the swelling being most marked on the left side, and impaired resonance was elicited in the left flank. She was very anæmic and extremely feeble. The radial pulse was rapid, badly filled and just perceptible. I was of opinion that a rapid recurrence had taken place, and the question of further operation was carefully considered. I decided to reopen the abdomen, and if the patient would stand it to do a more extensive removal of the pelvic organs, as she was certain to die if left alone.

When the abdomen was reopened on June 14 the peritoneum was found to be still more thickened and gelatinous, and several veins required to be ligatured in the abdominal wall. There were most extensive intestinal adhesions which entirely prevented my being able to reach the swelling on the left side of the abdomen, which appeared to be composed of matted intestine. Some dark sanious fluid was evacuated from the peritoneal cavity. As a complete removal of the disease was obviously impossible, I decided at once to close the abdomen. After

this second operation her feebleness gradually increased, and she died five days later. Permission could not be obtained to have a post-mortem examination, and thus the opportunity of verifying what really had occurred was lost.

DESCRIPTION OF THE SPECIMEN.

The tumour when removed close to its attachment to the uterus measured 7 in. by $4\frac{1}{2}$ in. at the widest end, and $2\frac{1}{2}$ in. in thickness. It was composed of two distinct portions; the inner or uterine half was firm in consistence, the outer or ampullary half was softer, and at its extremity the œdematous fimbriæ of the tube were seen. The shape of the tumour was that of an irregular ovoid, and the peritoneal surface was smooth except at those portions to which intestine, omentum and parietal peritoneum had adhered. It was of a deep livid colour, suggesting the presence of blood in the growth.

On close examination the somewhat attenuated Fallopian tube could be detected stretching across the posterior surface of the ampullary half for a distance of 3 in. The tube could be isolated close to its point of division, but when traced towards the fimbriated extremity it appeared to become incorporated with the new growth. When incised along the superior border a marked difference was at once noted between the two portions of the tumour. The ampullary portion was seen to consist of new growth, organized blood-clot and a large mass of clotted blood. The appearance partially resembled what is seen when a hæmatosalpinx is incised. At one portion the capsule had ruptured, permitting the escape of some of the contained blood (fig. 1).

It is possible that during the operation a slight tear might have been produced, but evidently some escape of blood had occurred previously, for clot was found free in the abdominal cavity. This portion of the tumour was extremely friable. The inner or uterine half presented quite a different appearance on section. It was firm and fibrous in appearance, resembling organized blood-clot, and prior to the microscopical examination it was suggested that the tumour was a hæmatosalpinx, as the relationship of the Fallopian tube had not been accurately determined. I was, however, of opinion that this portion consisted of new growth and not organized blood-clot, and that the new growth was probably a sarcoma. The extensive staining with blood-pigment which seemed to permeate the growth evidently masked its proper colour, as it was the paler areas which made me conclude that



FIG. 1.

The drawing represents the specimen removed (three-quarters actual size), posterior surface. **A**, the œdematous tubal fimbriae. **B**, the Fallopian tube, which is seen running towards the fimbriated extremity; a bristle has been inserted into the cut end of the tube. **C**, the situation of the pedicle with the blood-vessels. **D**, the uterine half, which was firm in consistence, being composed of organized blood-clot as well as new growth. The ampullary half was softer in consistence and had ruptured on its anterior surface, permitting the escape of some of the contained blood and clot.

it was a sarcoma. The swelling gradually tapered towards the uterine end of the tube, so that the pedicle—*i.e.*, where the tube was divided—did not appear abnormal to the unaided eye.

The tumour had evidently originated in the mesosalpinx, and was beginning to invade the Fallopian tube which was stretched over it. The appearance and shape of the tumour at first suggested the possibility that it was entirely tubal in origin, as at one extremity the oedematous fimbriae were conspicuous. When the course of the Fallopian tube was followed the true nature of the growth became apparent.

Under the microscope (fig. 2) the growth was found to consist of rounded and oat-shaped nucleated cells with areas of degeneration. The



FIG. 2.

Photomicrograph of a section of the tumour. Note the elongated cells with rod-shaped and rounded nuclei. The vessels were represented by channels between the cells; there were no giant-cells nor epithelial elements.

blood-vessels were represented by channels in the midst of the tumour tissue. There were no epithelial elements and no giant-cells. The amount of blood-pigment was a conspicuous feature in the sections.

The case, however, presents several points of interest in addition to the rarity of the disease. The clinical history coincides with that of carcinoma of the Fallopian tube, for in both a sanguineous uterine discharge is a leading symptom.

The somewhat rapid anæmia was accounted for by the amount of blood lost from the tumour, which was very vascular and, as already mentioned, had the appearance of a large hæmatosalpinx. Profound anæmia is, however, not uncommonly associated with the presence of a large sarcoma, even although there has been little or no hæmorrhage from it.

The absence of post-mortem evidence as to the nature of the recurrence leaves an important gap in the history of the case, but there can be no doubt that the rapidity of the growth possessed by sarcomata of the ovary and uterus is sometimes remarkable. I have seen an ovarian sarcoma double its size in the course of ten days.

If the nature of the growth had been known at the first operation the proper treatment would have been to remove the uterus, tubes, ovaries, and broad ligaments in one piece. Still, it must be confessed, that the results of operations, however extensive, for sarcoma of the internal genital organs are disappointing except for early cases.

It will remain a matter for conjecture whether a recurrent growth had rapidly developed on the left side or whether death was due to the widespread intra-abdominal adhesions. Certainly as far as the unaided eye could tell, the appendages on the left side appeared to be free from disease. There was no evidence at the second operation that recurrence had taken place on the right side. The absence of a post-mortem examination also leaves room for doubt as to whether the disease originated primarily in the mesosalpinx. The uterus, although not enlarged, might have contained new growth in its interior, but, if so, it must have been very small, so that I incline to the view that the case was probably one of sarcoma primarily involving the mesosalpinx with secondary invasion of the tube-wall.

In the year 1886 the first example of primary sarcoma of the Fallopian tube was recorded by *Senger*, of Breslau, and appeared in the *Centralblatt für Gynäkologie* (1886, p. 601). The disease was discovered after the death of the patient, aged 51, from diabetic coma. Douglas's pouch was obliterated by adhesions. The left tube, which was normal for a distance of $1\frac{1}{2}$ cm. from the uterine end, became gradually expanded, forming a dilatation (saccule) the size of a pigeon's egg with smooth exterior, partly nodular and partly fluctuating when palpated. The interior was filled with a greyish-brown crumbling tumour mass closely adhering to the inner wall, but softer towards the centre. The lateral end of the saccule ended in the quill-sized tube which then formed a second saccule the size of a hen's egg. Although the exterior was

smooth, at several places prominences were produced by the growth in the interior. The cavity of this saccule was occupied by a single globular polypoidal tumour the size of a pigeon's egg, with a narrow pedicle attached to the anterior wall of the sac. This polyp was greyish white in appearance and of soft consistence. From the inside of this saccule the sound could penetrate into a normal-sized tube lumen, but only for a few millimetres, as it was further impervious. The ovary was atrophied. The right tube, which was normal for 2 cm., was continued gradually into a plum-sized cyst with smooth exterior and fluctuating when palpated; then the tube was normal for $\frac{1}{2}$ cm., and was further continued into a second dilatation of similar size, and then became normal. The right ovary and parovarium were normal. The canal of the tube and the interior of both dilatations were smooth like the intima of a vein, but at several places were polypoid growths varying in size from a pin's head to a bean, very vascular and rather firmly attached. The saccules were filled with a brown fluid. At the bottom of Douglas's pouch was a flattened protuberance, the size of a 50 pfennig piece, of soft grey structure. Microscopically the growths consisted of a delicate connective tissue with round cells somewhat larger than white blood-corpuscles. This Senger regarded as being an undoubted small round-celled sarcoma. The disease had penetrated through the entire mucosa to the sub-mucosa. The muscular wall of the tube, although hypertrophied, was free from sarcomatous infiltration. At one spot in the left tube gland-tubules were noted in the sections. These he regarded as being derived from the parovarium. The metastatic growth in Douglas's pouch was a small round-celled sarcoma.

Gottschalk the same year exhibited a specimen of primary sarcoma of the right tube removed by Landau from a married woman aged 31, who had borne three children. She had always menstruated regularly, but for three months had suffered severe pain in the right iliac region. On examination the uterus was normal in size and position, but an elongated swelling the size of a walnut occupied the position of the right tube. At the operation the abdominal end of the tube was found to be involved, whilst the lumen remained patent. A blood-cyst the size of an apple occupied a position at the back of the tube. Its walls were formed by adhesions extending from the ovary on that side to the sacrum. There were also suspicious-looking nodules in the floor of Douglas's pouch. It was impossible to remove these nodules, but they were proved by microscopic examination to have a similar structure to that of the tubal tumour—viz., small spindle-celled sarcoma. Small spindle

nucleated cells, closely packed together, were arranged in clusters with only a little interstitial material. The nodules were supposed to be secondary to the primary tubal disease. She died on the fourth day after operation.

Sänger's case, fully detailed by him in Martin's "*Krankheiten der Eileiter*," p. 286, was a woman aged 42, who had been married eighteen years and was sterile. For six months she had noted an abdominal swelling. She had a cyst of the left parovarium and, besides, double chronic disease of the appendages. The left tube curved upwards and outwards, becoming more swollen towards the closed abdominal end, so that the outer third resembled in girth the small intestine. It was of somewhat hard consistence. The right tube was closely adherent to the ovary. When isolated it was retort-shaped, and from the uterine end to the abdominal end was enlarged to the thickness of the thumb. When the left tube was opened it was found to be filled with soft papillary growths varying in size from a pea to a hazel nut. In the right tube there were fewer papillomata, but at the abdominal end a single finely pediculated growth formed of several irregular nodules bound together. It was 3 cm. in length and 1.5 cm. broad. Six months after the operation the woman died with metastases at the sides of the uterus and in the inguinal glands. Microscopical examination showed the great preponderance of small round cells with some epithelial plugs. *Sänger* considered that the growth was a small round-celled sarcoma with a decided proportion of epithelial elements. The tubular structures noted by *Senger* were considered by *Sänger* to be papillomata and dendritic outrunners developed from normal or diseased plicæ.

Von Kahlden's Case.—Woman, aged 51, the mother of three children, the last being born when she was aged 40. Her menstruation had ceased for three years. Ever since Christmas, 1894, she had a feeling of weight and pressure in the hypogastrium, but without pain. Latterly she had become much emaciated. Examination in July, 1895, disclosed an elastic soft tumour filling the pelvis. Hæmorrhagic fluid was removed by a vaginal incision. She died on July 17. The necropsy revealed a double-sided tumour with metastases in both ovaries, numerous greyish-white metastases in the peritoneum and omentum, and on the peritoneal surface of the diaphragm. Numerous thrombi were found in the plexus pudendus, from which emboli in both pulmonary arteries had originated. The mitral valve was covered with two recent thrombotic layers. The pelvic organs, when removed, were hardened in Müller's

fluid. The right tube was converted into a sac, of which, on transverse section, the greatest vertical measurement was $16\ \mu\mu$, and the greatest transverse $19\ \mu\mu$. The cavity was filled with soft cauliflower masses, which extended to the fimbriated extremity, some even projecting free. The cauliflower masses involved the entire mucosa, so that the latter could not be distinguished, and, in addition, the muscular wall. The unaided eye could detect that the peritoneal surface was sharply defined from the growth. The tumour mass extended close to the uterine end of the tube, but the interstitial portion remained free. The ovary lay under the tube, and at one part was adherent to it. It contained a body the size of a walnut, with a fibrous capsule, which, on section, contained soft cauliflower-like masses similar to those in the tube. The left tube was longer than the right; the interstitial portion was free. The inner half of the tube resembled that on the right side, as it was transformed into a wide sausage-shaped structure filled with cauliflower-like masses. These were also sharply differentiated from the peritoneal covering, and under it a layer of unaffected muscular wall. In the outer half the tube returned by degrees to its normal shape, and it could be observed that the tumour masses were no longer equally distributed over the interior, but only on the under portion of the tube, so that two-thirds of the tubal circumference was normal. The tumour masses consisted of separate well-marked nodules. The left ovary was smaller than the right, only the size of a hazel nut, possessing a firm capsule and in its interior cauliflower-like masses. The uterus was slightly enlarged, and contained a fibromyoma, the size of a hazel nut, in the posterior wall of the fundus. After microscopical examination von Kahliden regarded the tumours as sarcomata of both tubes, leading to involvement of the lymphatics of the broad ligaments and the ovaries. The new growth had developed in the mucosa of the tube between the epithelium and the muscularis. The structure consisted of round and oval cells, with, in addition, giant-cells. The tubular structures already noted by Senger and Sanger bear, according to von Kahliden, a different interpretation. He argues that their component cells are only distinguished by their arrangement from the remainder of the tumour-cells, and even under a high magnification no distinction can be demonstrated. Von Kahliden considers that they are developed from the endothelium of the lymphatics which proliferate, forming several layers, thus producing peg-shaped structures, which, finally becoming detached from the wall, then appear as papillary or tubular bodies in the centre of the tumour nodules. The giant-cells present in the sections he regards as also being endothelial in origin. He

says: "The double-sided character of the growth resembled what is not infrequent in carcinoma cases. Indeed, macroscopically, sarcoma resembles carcinoma, not only in the existence of bilateral disease and the expansion of the tube by soft crumbling tumour masses, but also in the development of the disease in a mucous membrane the seat of previous inflammation. The distinction under the microscope will be better ascertained when more examples of the disease have been investigated."

CASES QUOTED BY PEHAM. DORAN.

(1) Age 51; one child, aged 29. Regular till a year before operation. For three months abdominal swelling, recently pelvic pain after exertion. Tumour size of a child's head in right of abdomen, on left side another reaching to umbilicus. Abdominal section. Both appendages removed, strong adhesions to intestine and parietal peritoneum. Uterus and right ovary (atrophied) not removed. Death on second day from "heart failure." Sarcomatous polypus found in uterine cavity. Right tube, a large cystic tumour characteristic retort-shape containing brown sanious fluid, papillomatous masses as big as beans on the inner wall. Left tube, with left ovary underneath it, flattened, apparently not infected; medullary masses sprang from inner wall of tube, obliterating lumen. The tumour tissue from both tubes was of a very mixed character, mostly sarcomatous, bearing on the surface papillomatous masses of a distinctly cancerous nature. (Von Franqué.)

(2) Age 43, two-para, last pregnancy twenty-three years. Catamenia regular. Swelling of abdomen two and a half months; no pain; vesical irrigation; emaciation. Tuberous masses in lower part of abdomen; pedunculated tumour to right, and a firm tumour to the left. Abdominal section; removal of both appendages. Uterus very large, not removed. Metastatic deposits in peritoneum of bladder and Douglas's pouch. The right tube formed a hydrosalpinx free from new growth. The left tube formed a cyst of the usual shape containing an odourless puriform fluid. Cauliflower masses sprang from its inner wall: they were made up of mixed sarcomatous and carcinomatous elements, as in the previous case. Alive eighteen months after operation, with ascites, masses in abdomen, and a cancerous ulcer on the posterior lip of the os externum. (Schäfer—operator, Krönig.)

From the reports of these cases it is not clear whether the tubal disease was primary, for in one a sarcomatous polypus is noted as being present *in utero*, and in the other the uterus was very large.

These, then, are examples of sarcoma arising in the mucosa, and from what we know concerning the distribution of the disease when attacking the uterus, we would expect to find that the muscular wall would also be the seat of a primary growth. Of this a case reported by Janvrin is supposed to be an example. A description of the specimen appears in the *New York Medical Journal* for March, 1889. "The tube showed a club-shaped enlargement, the greatest diameter of which was 2 in. The tube was patent throughout and its epithelium intact; it contained no pus at any time. Its course through the neoplasm was very tortuous. The peritoneum covering the neoplasm and ovary was normal in appearance and without increase of its connective tissue elements. The new growth had developed entirely outside the Fallopian tube (*sic*) between the basement membrane, upon which its epithelial cells rested, and the lower surface of the peritoneum forming the broad ligament. It consisted mainly of embryonic connective tissue, the dense white fibrillated and the yellow elastic forms being present in smaller amount, and also some smooth muscular fibre, zones of mucoid tissue, and cells of many shapes belonging to various forms of tissues. The growth was in no sense an inflammatory deposit, and might best be termed a composite myxosarcoma." This case is also somewhat doubtful, as it may have been an example of chronic salpingitis, but, under any circumstances, the description is not sufficiently detailed to place its true nature beyond cavil.

I have been unable to find any record of a sarcoma primarily involving the muscular wall of the Fallopian tube, but it will be apparent from the description here given that the disease originating in the mesosalpinx and invading the Fallopian tube might give rise to confusion as to the true origin unless a detailed examination of the specimen is made.

AGE-INCIDENCE.

Primary cancer of the Fallopian tube is most frequent at, and for a few years after, the menopause, and parous women are more subject to it. Sarcoma would appear to resemble carcinoma both in the age-incidence and the relation to parity.

Age			Parity
51	...	Senger	Not stated
37	...	Gottschalk--Landau	Three
42	...	Sänger	Sterile
51	...	von Kahlden	Three
51	...	von Franqué	One child
43	...	Schäfer--Kronig	Two children
52	...	McCann	Three children

Sarcoma of the mucous membrane of the uterus is met with occasionally during childhood, whilst shortly after the onset of menstruation the number of cases increases. The maximum, however, is reached shortly before and soon after the menopause.¹

In contradistinction to carcinoma, sarcoma is relatively infrequent after the sixtieth year. Sarcoma originating in the muscular wall of the uterus is rare in young women. At or near the climacteric seems to be the most common period for the disease to develop.

I have read the three cases reported by C. Dixon Jones,² and agree with the criticisms upon them given by Sanger. From the autopsies of over 3,000 adult women, thirty-five specimens of tubal pregnancy were discovered. They were all from women who had died suddenly. On examining the thirty-five specimens, three examples of sarcoma of the Fallopian were found (!), which are named by the author myeloma. It was assumed that a large intraperitoneal hemorrhage was the cause of death, and blood-clot was found attached to the ruptured portion of the tube.

Doran,³ who has written so ably on malignant disease of the Fallopian tubes, says: "Where else do we hear of a case of sudden death from rupture of a sarcomatous tube?"

I think, however, that sudden death is quite possible in a case such as has been described in this paper, and that the resemblance to a tubal pregnancy was very close. Still, the bursting of a sarcomatous tumour with effusion of blood into the peritoneum must be a phenomenon of extreme rarity. The absence of the clinical history and certain details concerning both the macroscopic and microscopic appearances has led me to omit Dixon Jones's cases from my list.

Report of Pathology Committee.—We have examined the specimen with microscopic sections, and find that the Fallopian tube can be readily traced for a distance of 3 in. from the fimbriated extremity upon the surface of a portion of the tube towards the site of the pedicle. A microscopical section from the uterine end of the tube shows perfectly normal structure. Sections through the middle of the tube and adjacent portion of the tumour made at right angles to the long axis of the tube show the lumen and mucous folds of the tube to be normal; the muscular walls are separated by blood-clot from the adjacent

¹ McCann, "Cancer of the Womb," p. 115.

² "Three Cases of Myeloma (Sarcoma)," *Amer. Journ. Obst.*, N.Y., 1893, xxviii, pp. 324-340.

³ Allbutt and Playfair, "System of Gynecology," 1st ed., 1896, p. 826.

growth, and there is no evidence of invasion of the tube by the growth. We agree that the tumour is a spindle-celled sarcoma, as described by the author, and we believe that it originated in the mesosalpinx, and not in the Fallopian tube.

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Mr. ALBAN DORAN said that, for the first time in this country, gynæcologists had an opportunity of inspecting a specimen of sarcoma of the tube. For that opportunity their thanks should be accorded to Dr. McCann, for previously the opinion of a high authority, Professor Säger, had alone induced pathologists to put much faith in other reports of this rare disease. There could be no doubt that the new growth in the specimen now exhibited was a sarcoma, and in all probability the Fallopian tube was its primary seat. Dr. McCann, however, had very properly noted how in von Franqué's case a sarcomatous polypus was found in the uterine cavity after the patient's death from the operation. That polypus might have been the primary growth, but the possibility that the uterus was the seat of such a growth in Dr. McCann's case was but slight. Although only eight fairly reliable cases of primary sarcoma of the tube had been reported, it was clear that the mortality due to operation was much higher than that which followed the removal of the carcinomatous tube. Landau, Janvrin, and von Franqué, all lost their patients within the first week, McCann's, already exhausted by hæmorrhages, rallied but for a week or two, whilst only Säger and Schäfer had fair results. Von Kahlden's patient died of pulmonary embolism, the result of the disease, so that, like Emil Senger's, she never underwent an operation. Janvrin's report was not to be found in any medical library in London; according to the illustration published with that report the disease was associated with salpingitis. In

an article on Diseases of the Fallopian Tubes, in the first edition of Allbutt and Playfair's "System of Gynaecology," Mr. Doran had criticized Charles Dixon Jones's doubtful cases. Dixon Jones examined thirty-five tubal tumours removed after death from women who had died suddenly. Out of the few specimens sufficiently fresh to allow of microscopic examination, he claimed to have detected no less than three samples of sarcoma of the tube, an extremely rare disease. We all know that sudden death associated with a tubal tumour almost invariably signifies ectopic gestation in the affected tube. The three cases seemed so doubtful that Mr. Doran considered them hardly suitable for discussion in a text-book, and on that account simply referred to them without comment in his article as revised for the second edition of the *System*. Dr. McCann, however, had shown that sarcoma of the tube might give rise to dangerous internal hemorrhage, so that Mr. Doran was now inclined to believe that one or more of Dixon Jones's three cases might have been genuine instances of sarcoma of the tube.

[Mr. Doran desires to add that the above remarks require modification, since the report of the Pathology Committee has shown that the sarcoma lay entirely in the mesosalpinx without invading the tube. Parona's case of "lipoma of the Fallopian tube" was somewhat similar, but the tubal walls were *probably* invaded by the fatty growth in the mesosalpinx.]

On Four Cases of Ovarian Cysts in Association with Vesicular Mole.

By HENRY RUSSELL ANDREWS, M.D.

A FEW years ago a good deal of interest was taken in the association of vesicular mole and chorio-epithelioma with excessive production of lutein tissue. Dr. Cuthbert Lockyer read a paper on the subject in April, 1905, before the Obstetrical Society of London. I have had under my care in the last few years two cases of vesicular mole in association with lutein cysts, and two others associated with ovarian cysts, the nature of which was undetermined.

(1) E. S., aged 20, was admitted to the London Hospital on November 27, 1907. She had had no previous pregnancy. Her last menstrual period was in August, 1907, but the uterus reached up to the umbilicus. She complained of pain on the left side of the abdomen, vomiting and bleeding. A diagnosis of vesicular mole was made, and the uterus was emptied under an anæsthetic. A small ovarian cyst was found on the right side, and the patient was told to come back in a few

weeks for ovariectomy. She left the hospital on December 12 and was readmitted on February 6, 1908. She complained then of pain in the right iliac fossa, severe for the last two days. A small tumour was felt lying in front of and to the right side of the uterus, and the left ovary was found to be slightly enlarged. I opened the abdomen and removed a right-sided ovarian tumour, the size of a Williams's pear. The tumour was deeply congested in consequence of a complete twist of its pedicle. It contained eight lutein cysts, each about the size of a large grape. The stroma was so deeply engorged with blood that it was impossible to say whether there was a general proliferation of lutein cells or not. The fluid gave the spectroscopic bands of lutein pigment (Dr. Flack and Dr. R. D. Maxwell). The specimen preserved in the bottle was frozen before being cut: it shows six cysts, each about the size of a marble, arranged round the periphery of the tumour; the cysts are lined by a pigmented membrane seen in some of them in the form of strands of a vivid yellow-brown colour. Microscopical sections show the presence of lutein cells. The left ovary, about twice the normal size, containing several small cysts, which were apparently simple distension cysts, was not removed. This patient has now (February, 1909) entered on the ninth month of a normal pregnancy.

(2) A. W. was admitted into the London Hospital on January 4, 1909, as an urgent case. She was aged 24, and had had one child three years ago and no miscarriages. The catamenia were regular until three months ago; then there were six weeks of amenorrhœa, followed by six weeks of slight intermittent bleeding. During the last six weeks there had been pain in the left side of the abdomen on and off. Recently the pain had become more severe, but there had been no sudden exacerbation. On admission the patient was rather anæmic, the pulse was rapid, and there was some abdominal pain; a soft elastic mass was felt rising out of the pelvis and reaching up to the umbilicus; no contractions were felt and no dipping sign could be obtained. On vaginal examination the cervix was soft, and a little blood-stained discharge was coming through the external os. In Douglas's pouch there was a small soft mass which was exquisitely tender. The tumour in the abdomen could not be felt from below, but I felt convinced that part of it at any rate was the body of the uterus, though bimanual examination was unsatisfactory. The diagnosis lay between—

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|--|-----|-----|-----|--|
| (1) Vesicular mole | ... | ... | ... | } Together with some acute intra-abdominal trouble |
| (2) Normal pregnancy farther advanced than the patient thought ... | ... | ... | ... | |
| (3) Extra-uterine or interstitial pregnancy. | | | | |

I made a diagnosis of vesicular mole complicated by some acute trouble, possibly co-existent extra-uterine pregnancy, and decided to operate at once. I had not thought much of a twisted ovarian tumour, because the small swelling in Douglas's pouch felt too soft for an ovarian cyst. While examining her under the anæsthetic something slipped away from the front of the upper part of the abdominal tumour, and I was able to say for certain that this tumour was the uterus. On opening the abdomen a small amount of free blood escaped. The swelling in Douglas's pouch was found to be a small right-sided ovarian tumour tightly strangled by twisting of its pedicle, and ruptured. I think it possible that the rupture had occurred during bimanual examination. After removal of this tumour a similar tumour of the left ovary was found, with a very long pedicle not twisted. This tumour had apparently been lying in front of the uterus, and had helped to make bimanual examination unsatisfactory. This tumour was removed. The uterus was distinctly larger than would be accounted for by a twelve weeks' pregnancy, but I did not think that my diagnosis of vesicular mole was established sufficiently firmly to warrant removal of the uterus. The bleeding from which she had suffered might be accounted for by the twisting of the pedicle of the right ovarian tumour. I was interested to find that Dr. Drummond Maxwell, who, if he had agreed with my diagnosis of vesicular mole, had done so chiefly out of politeness, agreed unhesitatingly that the pregnancy was molar after seeing the two ovarian tumours. The abdomen was closed and the patient put back to bed. She was given morphia on several occasions with the idea of preventing abortion if the pregnancy were normal. There was intermittent uterine hæmorrhage of slight amount, and on January 25, seventeen days after the operation, she passed a vesicular mole. I shall be interested to hear the opinion of Fellows as to whether I ought to have removed the uterus together with the ovarian tumours. If it contained a vesicular mole it was a useless organ and a possible source of danger. The patient left the hospital on February 9. The mounted specimen consists of part of the mole and one half of the left-sided tumour. The tumour of the right ovary had exactly the same appearance, except that it was engorged with blood from twisting of its pedicle. The tumour is irregularly oval in shape, measuring $4\frac{1}{2}$ in. by $3\frac{1}{2}$ in. Its external surface is wrinkled and shows numerous projecting bosses formed by cysts with translucent contents. In the recent state these cysts stood out on the surface like "bubbles about to burst," to use Mrs. Scharlieb's description. The cut surface shows about sixteen cysts,

varying in size from that of a pea to that of a bantam's egg. Some of the cysts are filled with a clear albuminous jelly of a pearly-grey colour (fluid coagulated by formalin). In other cysts, from which the coagulated contents have fallen out, the characteristic brownish-yellow colour of the wall is seen. Microscopical sections of cyst-wall show that it is lined by a typical lutein cell-layer, several cells in thickness, thick enough to be recognized macroscopically.

(3) A. E., aged 20, was admitted into the London Hospital on July 27, 1905. She had had two children, the last in July, 1904. During April, May and June she had amenorrhœa, followed by slight bleeding throughout July. A week before admission, and again on the day of admission, she had a fit in which she lost consciousness and bit her tongue. She had never had any fits before. The urine contained a good deal of albumin, and there was some œdema of the legs and thighs. There were no eye-changes. The uterus was the size of a six months' pregnancy. On August 1 she was delivered of a large vesicular mole. After this she had persistent pyrexia, although there was nothing to point to infection of the uterus. On re-examination a cystic swelling was found behind the uterus. I opened the abdomen and removed two small ovarian cysts, both suppurating. Unfortunately they were both thrown away before any note had been made about their appearance, so I cannot say whether they contained lutein cysts or no. I have a vague recollection that both of them were multilocular. The patient made a good recovery.

After removing the first lutein cyst shown to-night I wondered whether this polycythic lutein change might not possibly undergo a certain amount of resolution, the life of the lutein cell being a brief one. A case that I saw in consultation with Dr. C. I. Kirton last year tended to confirm this idea.

(4) In April, 1908, I saw a patient aged 32, who had had slight bleeding of one month's duration, severe abdominal pain and jaundice in early pregnancy. She had had one stillborn child and one miscarriage previously. She thought herself three months pregnant, and the uterus was as large as at seven months. Dr. Kirton had noticed a small tumour in Douglas's pouch. I found that the uterus contained a vesicular mole, which I removed under an anæsthetic. The left ovary was enlarged to about the size of a goose's egg or a little more. As the patient was ill and had lost a good deal of blood I did not advise ovariectomy at once, but said that it might be necessary later on. I asked the doctor to examine her from time to time, and wrote to him a

few weeks ago to ask what had happened to the tumour. On January 22, 1909, he wrote: "I observed the tumour from time to time, and it got smaller and smaller and has now disappeared, or, at any rate, cannot be felt. . . . The patient has since become pregnant again." I examined her myself on February 5, 1909, and found that she was about four and a half months pregnant. I could not make out any enlargement of the left ovary. She told me that she had noticed a small swelling in the left side of her abdomen on several occasions in the first few weeks after removal of the mole. It is, of course, impossible to say for certain that this small tumour was made up of lutein cysts, but it is at least probable that it was.

I have only been able to find two recorded cases in which ovarian tumours found in association with vesicular mole have been noticed to become gradually smaller until the ovaries got back to their original size, and have not heard of any cases in which ovarian tumours other than lutein cyst tumours were associated with vesicular mole.

Gouilloud reported the following case in 1907: When the patient was first seen she was four months pregnant. On each side of the uterus was a tumour the size of a foetal head at term. Three weeks later she was delivered of a vesicular mole. When examined eighteen days later the tumours were smaller. She was examined again at intervals of four months and a year, and the pelvic organs were said to be normal. Two years after the expulsion of the mole she aborted at four months, the ovum being normal. The ovaries at this time were not enlarged. Gouilloud considers that these lutein tumours should not be removed unless twisting of the pedicle occurs. This accident has occurred in several of the recorded cases, and in both of those which I show to-night.

Albert recorded a case in which, at the time of expulsion of a vesicular mole, bilateral ovarian tumours, each as large as a fist, were found. Two months later the ovaries were found to be once more of normal size.

I have not been able to find many new papers on the subject since that of Dr. Cuthbert Lockyer mentioned above. The papers of Patellani, Seitz, and the first of Wallart appeared just before Dr. Lockyer's paper was read, and were quoted in the discussion on his paper. Patellani collected 84 cases of polycystic degeneration of the corpora lutea in connexion with vesicular mole and chorio-epithelioma. In 44 cases there was a vesicular mole only. In 18 cases chorio-epithelioma followed a vesicular mole. In 22 cases there was chorio-epithelioma but no mole,

In about 91 per cent. the tumours were bilateral. The usual size of the tumours was about that of a fist. He considered that the typical appearance was of value from a diagnostic point of view.

Seitz examined ovaries from 36 cases of normal pregnancy and found in them an increase of lutein tissue, differing from that in lutein tumours only in degree. He considered that the increase of lutein tissue could not be held responsible for the formation of a vesicular mole, and that some abnormal condition of the decidua was a more probable cause. He looked on the changes in the ovaries in association with vesicular mole as being comparable with the œdematous change seen in them in association with uterine fibroids. If Seitz's view as to the changes in the ovaries is correct—viz., that they are due to disturbances of the circulation—it would seem to be better to watch these patients carefully and not to remove the enlarged ovaries unless torsion of the pedicle or suppuration occurs. Wallart agreed with Seitz's views and looked on the proliferation of lutein cells in lutein tumours as being simply a proliferation of the normal interstitial gland tissue.

Kleinhans and Schenk carried out experiments on rabbits to test the theory of Born and Fraenkel as to the function of the corpus luteum in early pregnancy. In twenty-five cases they removed all the corpora lutea about nine days after coitus, taking all possible care to disturb the parts as little as was compatible with thoroughness. At first they used the cautery, afterwards a sharp spoon or forceps. In six out of the twenty-five rabbits pregnancy continued undisturbed, proving that in rabbits at any rate the presence of a corpus luteum is not necessary for the development of the ovum.

I wish to thank Dr. Drummond Maxwell for his help with notes and sections.

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**Case of Hydatidiform Mole with Albuminuria and Kidney of
Pregnancy ; Sudden Death from Cardiac Failure.**

By G. BLACKER, M.D.

MRS. R. A. J. (Register No. 1895), aged 33, was admitted into University College Hospital on July 18, 1908, complaining of swelling of the face and of the ankles. She had been married six years, and had had one child, now five years of age, and no miscarriages. Her first pregnancy and confinement was quite normal. On admission the patient was about nineteen weeks pregnant, the last period having occurred on March 4, 1908. For a few weeks past she had noticed some swelling of the eyelids and ankles, and four weeks ago severe hæmorrhage had occurred from the uterus, a blood-stained discharge continuing at the present time. Up to the date of their cessation the periods occurred regularly every four weeks, lasted four to five days, and were not at all excessive. She did not complain of any pain, had never suffered from leucorrhœa, micturition was normal, but the bowels were rather loose. There was no history of any previous renal disease, and she had never had scarlet fever.

Examination showed the patient to be a well-nourished woman; the lips and mucous membranes were pale, the lower eyelids were swollen and puffy, and there were dark rings round the eyes. There was well-marked œdema of the dorsum of the feet and of both ankles. The pulse was regular in rhythm, but rapid—98 per minute. The radial arteries and the brachial felt somewhat thickened, and the blood-pressure in the radial arteries was 196 mm. Hg. The maximum cardiac impulse was in the fourth interspace diffuse and forcible in character, a diastolic shock being felt, and the whole chest vibrating with the cardiac rhythm. There was no impulse to the right of the sternum, while the upper limit of cardiac dullness was masked by emphysema of the lung. The heart-sounds were well heard; the first sound was somewhat loud, while over the whole of the præcordium the second sound could be heard accentuated with its maximum intensity at the aortic and pulmonary cartilages. The lungs showed signs of emphysema, and at the base of the left lung behind there was some impairment of resonance and blowing breathing. The knee-jerks were exaggerated, the plantar reflex flexor in type, and

examination of the eyes at this time showed no changes in the fundus. The uterus reached 1 in. above the level of the umbilicus, no foetal parts could be felt, and no foetal heart-sounds heard. There was a slight blood-stained discharge from the vagina. The urine was acid, had a specific gravity of 1020, contained 50 per cent. of albumin, and microscopic examination showed the presence of numerous hyaline, granular and a few epithelial and fatty casts.

The patient was placed on a strict milk diet, freely purged and ordered a simple diuretic mixture. Nine days after admission the albumin amounted to only 25 per cent., but there was still some swelling of the ankles. Considerable bleeding occurred from the uterus on two occasions at this period. On the twelfth day there was only a cloud of albumin, but an examination of the eyes by my colleague, Mr. Herbert Parsons, showed that while the vision remained unaffected both eyes exhibited signs of albuminuric retinitis. In the left eye there was a small triangular exudate on the left inferior temporal artery, and close to it a small hæmorrhage. In the right eye there was an exudate of the same character, but no hæmorrhage.

On August 5, eighteen days after admission, the albumin still amounted to about one-third, and the patient was complaining of a good deal of headache, mainly occipital in position. Apart from this, however, her general condition was good. Three weeks after her admission the albumin varied from 20 to 30 per cent., the pulse was rapid—100 to 104 per minute—and the arterial tension registered 185 mm. Hg. On August 13, the twenty-sixth day, the vision when tested was found to be $\frac{6}{3}$, with difficulty in both the right and the left eye. Numerous spots of hæmorrhage and exudate were noted as present in both eyes, but there were no stars at the macula; at this time the patient was complaining a good deal of pain about the eyes and eyebrows. On August 17 there was a severe hæmorrhage from the uterus, accompanied with a good deal of abdominal pain. The albumin still amounted to about a quarter. Two days later there was another severe attack of bleeding from the uterus; at this time the ankles were swollen slightly, and an examination of the eyes showed more exudate and neuritis in the left eye, but no change in the condition of the right. In view of the two severe hæmorrhages which had occurred, the fact that the vision was deteriorating, and the albuminuric retinitis increasing, while the foetus was probably dead, it was decided to induce labour.

On August 21, twenty-four weeks from the last period, the uterus was noted as reaching to a level of 3 in. above the umbilicus. It was

thought to contain a mole or an excess of liquor amnii, no foetal parts being felt, and no foetal heart-sounds heard. At 10.30 a.m. three solid bougies were introduced into the uterus without difficulty or hæmorrhage. At 3.30 p.m. the patient had a severe loss of blood, but after this no bleeding occurred until 8.30 p.m., when there was some recurrence of the bleeding. Vaginal examination showed that the cervical canal was sufficiently dilated to admit one finger readily, and presenting in the canal some friable tissue could be felt, which, on scraping away, proved to be fragments of a hydatidiform mole. No uterine pains were present, the uterus was contracted but not tender, and there were no signs pointing to any internal hæmorrhage. The pulse was 108, and of good quality, and the patient's general condition was good. The temperature was normal, and at no time during her stay in the hospital did the temperature rise above 99.4° F. It was decided to administer an anæsthetic the first thing the following morning, and to empty the uterus; in the meantime the bougies were removed and the vagina was tightly plugged with iodoform gauze. The patient remained comfortable during the night, there were no uterine pains, but no further bleeding occurred, and the condition and rate of the pulse did not alter. At 4.30 a.m. the following morning, immediately after the administration of an enema, she became collapsed, fainted, and died in a few minutes, before the house surgeon could be called to her. Transfusion of saline fluid, the injection of strychnine, and the performance of artificial respiration, all failed either to restore the heart's action or the respiration.

A post-mortem examination was allowed only on the abdomen, and was performed eight hours after death. The lungs, obtained through the diaphragm, showed signs of bronchitis and emphysema. The heart was a little hypertrophied, the muscle-wall rather pale, but the valves and orifices normal. A complete examination of the pulmonary vessels was not possible, but the patient's condition just before her death did not in the least suggest that of a person dying from a pulmonary embolus. The liver was somewhat enlarged and showed cloudy and fatty changes in the liver-cells. The kidneys were slightly enlarged, their surfaces were smooth on stripping off the capsule, the cortex was a little wider than normal and pale, the medulla normal in appearance, and the consistence of the kidney tissue was not tougher than natural. The pelves of the kidneys were normal. The remaining abdominal viscera were all healthy. The uterus reached nearly up to the ensiform cartilage, and was removed entire with the rest of the pelvic organs.

Microscopic examination of sections from the kidneys shows well-marked changes in the epithelium of the convoluted tubules and of the glomeruli. The cells have lost their outlines, the nuclei are no longer recognizable, or stain very badly, the outlines of the tubules are obscured, and their interior is filled with a mass of granular necrotic material which in many instances completely blocks them. The appearances presented are those of cloudy swelling and coagulation necrosis of the epithelium, and are limited mainly to the tissues of the cortex (fig. 8).

Microscopic examination of the liver shows cloudy swelling and some degeneration of the liver-cells in the centre of the lobules round the intralobular vein. The protoplasm stains badly, and the cells present a granular appearance. The interlobular vessels are dilated, as if they had been congested, but contain no blood.

The uterus, with the mole, and the portions of the bladder, rectum and vagina removed, weighed 8 lb. 9 oz.; after hardening it was divided by a sagittal section. The specimen shown (fig. 1) consists of one-half of the uterus, with a portion of the bladder, vagina and rectum divided by a sagittal section. The uterus measures from the fundus to the external os 25 cm., antero-posteriorly $15\frac{1}{2}$ cm.; the uterine wall varies in thickness from 10 mm. to 7.5 mm. The cervical canal, which measures 2.25 cm. in length, is dilated to the extent of $1\frac{1}{2}$ cm. The interior of the uterus is occupied completely by the hydatidiform mole, which appears in the section to be attached to about two-thirds of the anterior wall of the uterus.

No trace of the foetus, of the umbilical cord or of the amniotic cavity appears on the surface of the section. To assert that they are absent completely is, however, impossible without making a series of sections through the whole of the uterine cavity. Over the posterior wall of the uterus to within 6 cm. of the fundus the chorion, with the villi lying in contact with it, has been separated artificially, no doubt by the introduction of the bougies.

In the lower uterine segment immediately above the internal os there is a layer of blood-clot interposed between the chorion and the anterior uterine wall measuring 4 cm. by 1 cm., and intermingled with the tissue of the mole in the cervical canal and immediately above it posteriorly there is more blood-clot. Elsewhere in the uterus, with the exception of some small areas about $\frac{1}{2}$ cm. in diameter, there is no trace of any blood. The mole is made up of vesicles varying in size from 3 cm. by $1\frac{1}{2}$ cm. in the case of the largest to others only just visible to the naked eye. Over the attached area of the mole there are

in numerous situations small masses of what appear to be fibrin. In different areas of the section the individual vesicles are separated from one another by small areas of opaque greyish-white tissue. The uterine wall macroscopically appears to be normal in structure, and nowhere is there any definite sign of an invasion of the uterine muscle by the tissue of the mole. In the present stage of its development it is impossible to say with certainty whether the mole is developed from the whole or from only a part of the chorion. The general direction of growth of the vesicles tends to show that the placental site would have been situated on the anterior wall of the uterus. Both ovaries are somewhat enlarged; the left, measuring 4 cm. by 2.75 cm., contains a number of small cystic spaces, the largest about $1\frac{1}{2}$ cm. in diameter, and filled with blood-clot.

The right ovary, somewhat pyramidal in shape, and measuring 7 cm. by 5 cm. at its widest part, also contains a considerable number of similar cystic spaces, the largest filled with blood-clot and measuring $1\frac{1}{2}$ cm. in diameter. Both Fallopian tubes were quite healthy.

Microscopic examination of sections taken from the anterior wall of the uterus show the following appearances (fig. 2): The uterine muscle presents a somewhat degenerated appearance; the outlines of the fibres are obscure and the nuclei stain faintly; the individual fibres are separated from one another by a number of clear spaces, no doubt due to oedema, but the vessels and sinuses are normal in structure. Superficial to the muscle-fibres four distinct layers can be recognized: (1) the spongy layer of the decidua; (2) the compact layer of the decidua largely degenerated and necrotic; (3) the fibrin layer of Nitabuch; (4) a cellular layer derived from the proliferating epithelium of the villi.

The spongy layer of the decidua is thin and flattened out, the gland spaces are arranged parallel to the surface, the epithelium of the glands has disappeared, and in many places the glands themselves are largely wanting. There is a good deal of small-celled infiltration throughout the spongy layer and in the most superficial portion of the muscular fibres, and there is also some in the deeper portions of the compact layer of the decidua.

The greater part of the stratum compactum has undergone marked degenerative changes, the whole presenting a homogeneous, somewhat granular, and in parts a hyaline appearance, staining faintly, the outlines of the cells having disappeared, and their nuclei being no longer recognizable. Here and there the remains of a few decidual cells can



FIG. 1.

Dr. Blacker: Hydatidiform mole.

be seen, and scattered throughout the layer a few polymorphonuclear leucocytes, while in places the degenerated tissue has been invaded by some of the cells of the cell-layer on the surface derived from the proliferating epithelium of the villi. The major part of the compact layer of the decidua has, in fact, undergone almost complete transformation into the so-called fibrin layer of Nitabuch. There is a well-marked small-celled infiltration in its deeper portions continuous with that seen in the spongy layer. This small-celled infiltration is exceedingly well shown in some of the sections (fig. 2). It is made up mainly of polymorphonuclear leucocytes and forms a well-defined layer in the deeper

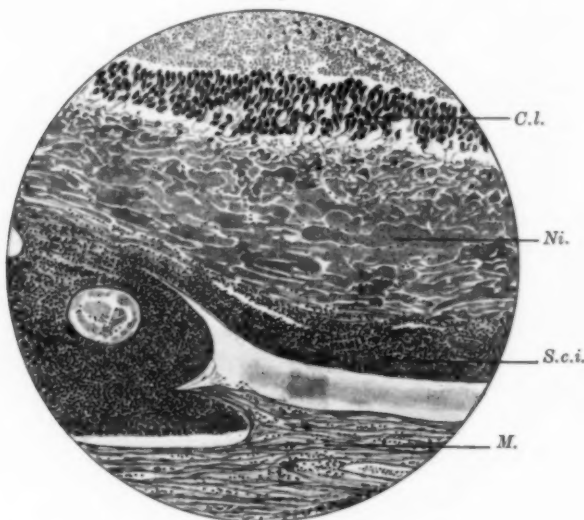


FIG. 2.

Section of uterine wall showing: Muscle-fibres, *M.*; small-celled infiltration in spongy layer and deepest part of compact layer of decidua, *S.c.i.*; the fibrin layer of Nitabuch, *Ni.*; the cell-layer of proliferating epithelium of mole, *Cl.*

parts of the stratum compactum, so dense in some places as to obscure the rest of the tissues.

The fibrin layer of Nitabuch is present in all the sections and forms a very characteristic feature; it varies, however, in thickness, and in a few is almost entirely wanting, so that the epithelial cells of the mole come into direct relation with the remains of the decidua and even with the superficial layers of muscle tissue.

Forming a definite cell-layer of varying thickness upon the surface of the necrotic zone, in some portions invading it or even penetrating beneath it, are numbers of cells arranged singly, in masses, as a single or as many layers, and derived from the epithelium of the chorionic vesicles. In some situations they may be recognized as masses of opaque protoplasm of varying shapes, with vacuolar spaces within them, and containing numerous round or oval well-staining nuclei, no doubt taking their origin from the syncytium; but the greater number are single epithelial cells, polygonal or polyhedral in shape, with a well-

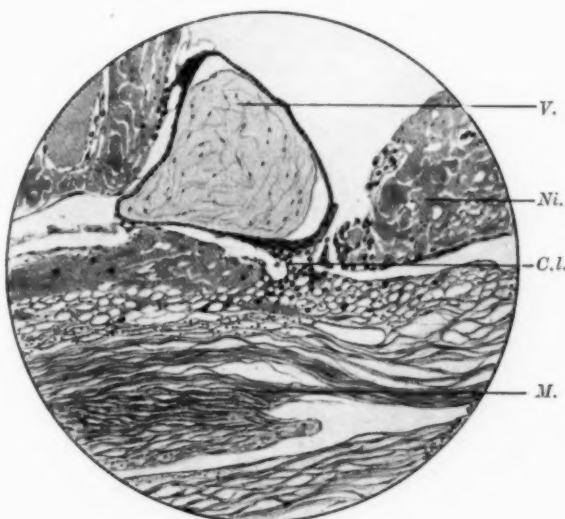


FIG. 3.

Showing invasion of spongy layer of decidua by proliferating cells of villus.

staining round or oval clear vesicular nucleus, and derived from the proliferating cells of the layer of Langhans. The protoplasm of the latter cells is granular and stains faintly. In many parts of the sections the cell-layer can be traced into direct continuity with the epithelium on the surface of the vesicles (fig. 3), but in others the cells form isolated masses on the surface of the decidua. Here and there they extend right through the necrotic zone, and having apparently destroyed this layer of tissue they are to be seen actually invading the muscle tissue of the uterine wall, or spreading laterally on the deeper surface of the

compact layer lying superficial to or invading the spongy layer of the decidua (fig. 4).

Sections from the mole itself show that in many of the vesicles the embryonic connective tissue cells of the stroma are still recognizable, and fill practically the whole of the interior of the villus, although separated from one another by a homogeneous exudate (fig. 5). In the larger villi the connective tissue is arranged parallel to the outer surface of the vesicle, and is largely wanting in the centre. In none of the vesicles are any capillary vessels to be recognized. In most of them a single

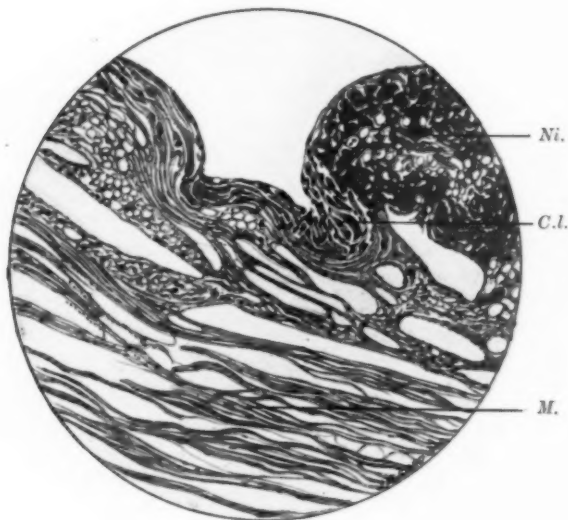


FIG. 4.

Showing absence of fibrin layer, *Ni.*, and direct invasion of the stratum spongiosum by the proliferating cells of the mole, *C.I.*

layer of epithelium is present on the surface, evidently representing the layer of Langhans, and in some the syncytium is also present and recognizable. Many present solid buds of proliferating epithelium on their surfaces, and in most instances the buds are clearly formed of the cells of Langhans's layer (fig. 5). In some villi, however, the buds are covered also by a layer of syncytium, and in others there are definite buds to be seen composed solely of syncytium. There are a considerable number of vacuolar spaces both in the proliferating cells and in

some of the masses of protoplasm. Examination of the opaque grey areas seen in some parts of the mole show that they are made up of degenerate villi surrounded by masses of necrotic cells and granular material, intermingled with which are some well-defined cells with well-staining nuclei derived from Langhans's layer (fig. 6).

The attachment of the villi to the decidua can be demonstrated (fig. 3) to be due to the invasion of the latter by masses of cells derived mainly from Langhans's layer. These appear gradually to invade and to destroy the superficial portions, and penetrating to the deeper



FIG. 5.

Villi with proliferation of the cells of Langhans's layer with vacuolar spaces and some masses of syncytium.

layers firmly attach the vesicles to the substance of the decidua. In a few places a mass of epithelial cells can be seen within the lumen of a vessel (fig. 7), and isolated cells or collections of cells are found in the superficial layer of the muscular wall of the uterus.

It is a matter of extreme difficulty to determine whether the cells recognizable in the decidual layers are in reality the remains of the degenerated decidual cells, or are the proliferating cells of the epithelium of the villi which have invaded the tissue and then undergone

degeneration. In some sections, however, the degenerated decidual cells can be recognized lying side by side with darkly-staining epithelial cells certainly derived from the mole, and which have found their way into the substance of the decidua.

The microscopic characters of the mole are practically identical with those described by Marchand in his historical paper published in 1895.¹ The characteristic hypertrophy of the cells of the epithelium of the villi, especially of the cells of Langhans's layer, the extensive degeneration and almost entire destruction of the decidua, more particu-



FIG. 6.

Section of one of the opaque grey areas from mole, showing degenerate villus surrounded by mass of degenerate cells, intermingled with which are some masses of syncytium and proliferating cells of Langhans's layer.

larly of the compact layer, the presence of a well-marked fibrin layer of Nitabuch, formed mainly of the necrotic decidua, the layer of small-celled infiltration, and the invasion of the decidua, and even in some places of the vessels and of the uterine muscle itself, by the cellular elements of the mole are all well shown in this specimen.

¹ *Zeits. f. Geburts. und Gyn.*, Stuttg., 1895, xxxii, p. 405.

Microscopical examination of the ovaries shows that they contain a large number of small cysts, nearly all cysts of the corpora lutea, and containing a definite layer of lutein tissue in their walls.

A careful examination of seven sections taken from different parts of the two ovaries shows that they contain twenty-two cysts of which thirteen are derived from corpora lutea, while two others which contain a large amount of blood are probably of the same origin, the hæmorrhage having destroyed and obscured the lutein cell-layer. The inner surface of the cysts is lined by a layer of tissue resembling fibrin,

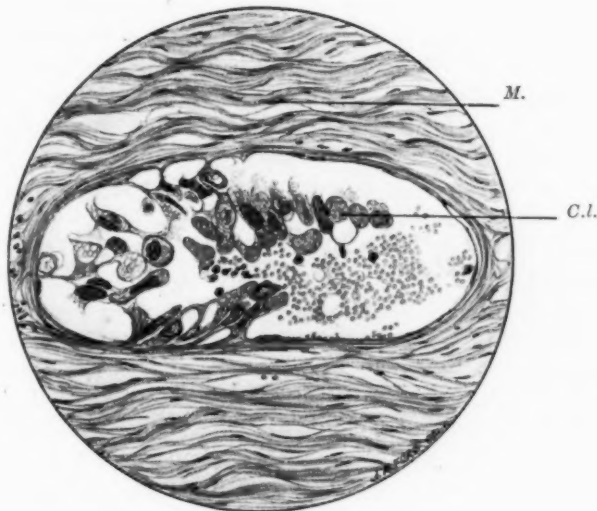


FIG. 7.

Showing a collection of epithelial cells, *C.L.*, of the mole lying within the lumen of a blood-vessel in the muscular tissue of the uterine wall, *M.*

and there is no epithelial lining within the lutein cells. In addition to these cysts there are scattered throughout the stroma of the two ovaries eleven collections of lutein cells of varying size. No separate lutein cells are, however, to be found in the stroma. There certainly, therefore, appears to be a definite excess of lutein tissue in the two ovaries.

In another case of hydatidiform mole which I have recently had under my care the same condition of cystic degeneration of the ovaries

was present, and in this case too most of the cysts were derived from corpora lutea. The patient, a young lady aged 30, who had been married four months and was thought to be three months pregnant, was seen with her medical attendant on account of a very rapid increase in the size of the uterus. On abdominal examination a tumour was found reaching up to the level of the umbilicus and intermittently contracting, but in it no foetal parts could be felt and no foetal heart-sounds heard. It seemed more elastic than the normal pregnant uterus, and was diagnosed as a case of pregnancy with hydramnios. In the right iliac fossa a cystic body was present about the size of a closed fist attached to the uterus and evidently an ovarian cyst. The patient's general health was unsatisfactory, and she had lost a good deal of flesh since the commencement of her pregnancy. The case was considered to be one of pregnancy with hydramnios complicated by the presence of a small ovarian cyst, and abdominal section was performed. It was then found that both ovaries contained a number of cysts; the right ovary, the larger, therefore, was completely removed and the cystic portion of the left was excised, the remainder of this ovary being left as the patient was a young married woman only 30 years of age. A few days after the operation uterine hæmorrhage set in, and as the bleeding was severe and the discovery of the cystic disease of both ovaries had led me to suspect the presence of a mole *in utero*, an anæsthetic was given and the uterus emptied of a large hydatidiform mole weighing 2 lb. 9 oz. The second operation was performed on October 27, 1908. The patient made an uninterrupted recovery, but up to the last ten days or so she has suffered from almost continuous slight uterine hæmorrhage which has now apparently ceased. Her general health has improved very markedly, and she is gaining weight steadily at the rate of $1\frac{1}{2}$ lb. to 2 lb. a week.

Microscopical examination shows the great majority of the cysts in the ovaries in this case to be cysts of the corpora lutea. On a previous occasion, when showing a specimen of chorion-epithelioma with lutein cysts in both ovaries before the Obstetrical Society,¹ I expressed the opinion that the excess of lutein cells and the formation of these cysts was in reality due to the pregnancy. If we consider for a moment the changes which occur in the mole and in the ovary in cases of vesicular degeneration of the chorion, we cannot but be struck by the marked similarity between them. In the mole we have an excessive proliferation of the epithelium of the villi with the formation of cystic spaces, no

¹ *Trans. Obstet. Soc. Lond.* (1907) 1908, xlix, p. 104.

doubt due to serous transudation, while in the ovary we have a marked proliferation of the cells of the corpora lutea with the development of cysts possibly of similar origin. Both are embryonic rapidly growing tissues, characterized, even in normal conditions, by their marked capacity for hyperplasia, and we may well assume that both would respond readily to any abnormal stimulus. That the cystic degeneration of the ovaries so frequently met with in cases of vesicular mole and chorion-epithelioma is due to the presence of cysts of the corpora lutea must be accepted as definitely proved, but the fact that the two are so frequently associated does not show that they stand in any causal relationship to one another, and it may well be that they are the result of the same stimulus, whatever its nature ultimately may prove to be, acting on both the uterus and the ovaries.

In a great many apparently normal ovaries, as Cuthbert Lockyer¹ has pointed out, there is in the aggregate a great deal of lutein tissue present, and it has been shown to exist in the ovary in considerable quantities in cases of normal pregnancy. I do not think the hypothesis that an excess of lutein tissue plays an important part in the causation of the overgrowth of the epithelium of the villi is by any means proved, and I doubt whether it is really necessary to remove the ovaries when the cysts they contain are derived from the corpora lutea, especially in view of the fact that cases have been recorded² in which, after the expulsion of the vesicular mole, the enlarged ovaries have dwindled in size. The present case certainly gives no support to the view of Virchow that pathological changes in the uterine wall are an important factor in the causation of a vesicular mole. The uterine musculature is practically normal, and the decidua, although markedly degenerated, shows no signs of any old inflammatory change. The small-celled infiltration present is limited to certain parts of the decidua, and clearly bears a relation to the invasion of the maternal tissues by the cell-layer of the mole. The changes in the decidua are essentially secondary and of a degenerative character, and present the appearances we might expect to find if the maternal tissues were undergoing solution by some enzyme secreted by the cells of the villi, as Teacher has suggested is the case in the destruction of the decidua by the cells of the trophoblast in the early human ovum.

The view supported by Marchand³ that a vesicular mole is due to a

¹ Lockyer, Cuthbert, *Journ. Obstet. and Gyn. Brit. Emp.*, Lond., 1905, vii, p. 12.

² Albert, quoted by Russell Andrews, *Journ. Obstet. and Gyn. Brit. Emp.*, 1904, v, p. 460.

³ Marchand, *loc. cit.*

defect in the ovum itself, possibly acquired by it at the time of impregnation in some cases, appears to possess the greatest degree of probability, and certainly seems to explain best the facts which are known with regard to the etiology of these tumours. At the same time the possibility Ballantyne¹ suggests is one of extreme interest—namely, that a hydatid mole results when the embryo fails altogether to appear; that is to say, when there is an arrest of development prior to the appearance of the traces of an embryo.

The present case is one I have thought worth recording because the opportunity of examining a hydatidiform mole *in situ* in the uterus does not often occur, and because it is an interesting example of the development of the albuminuria of pregnancy in association with the so-called kidney of pregnancy.

There can be no doubt from the clinical history and the findings at the post-mortem examination that the albuminuria developed during the pregnancy and did not precede it, and it is therefore an example of albuminuria occurring with, and probably due to, a hydatidiform mole, and not a case of chronic nephritis in the course of which a molar pregnancy occurred, a variety of kidney diseases which some writers consider to be one of the factors in the production of this curious degeneration of the chorion. In view of the recent occurrence of the kidney lesion the high blood-pressure and the cardio-vascular changes which were present are of great interest. It must be remembered, however, that a certain degree of high blood-pressure may appear within a few weeks of the onset of an acute nephritis, especially when this is caused by an acute toxic condition, and in this case it may have been in part due to the pregnancy.

The gross changes in the kidneys—viz., the slight general enlargement, the increase in the thickness of the cortex, the granular degeneration and the appearance of necrosis of the epithelium of the convoluted tubules seen on microscopic examination—are precisely those which have been described in cases of the acute nephritis of pregnancy, and are very suggestive of a toxic origin.

Albuminuria is not an uncommon complication of a hydatidiform mole. In 19 of the 100 cases collected by Dorland and Gerson² œdema and albuminuria were present, and it occurred in 6 of the 25 cases recorded by Williamson in his paper on this subject read before the

¹ Ballantyne, J. W., "Antenatal Pathology," *The Embryo*, 1904, p. 613.

² Dorland and Gerson, *University Med. Mag.*, May, 1896, pp. 565-590.

Obstetrical Society in 1899.¹ In only one of these was any history obtained suggesting that the patient had had nephritis previously, while two of the cases died, one of uræmia and one of uræmia and hæmorrhage. The present case proves that the albuminuria of pregnancy can develop in the absence of a fœtus and attain such a degree of severity as to cause the presence of a large amount of albumin in the urine with numerous casts, marked changes in the retinæ, and to require the induction of premature labour. Is it not probable that the statement to be found in many books that nephritis is an etiological factor in the



FIG. 8.

Section of kidney showing degeneration and coagulation necrosis of epithelium of convoluted tubules.

development of a hydatidiform mole is an error, and that the mole is really the cause of the nephritis; and if this be so may not the frequency with which albuminuria occurs in these cases tend to support the view put forward by Veit, Schmorl, and others that eclampsia is an auto-intoxication due to a poison derived from the syncytium? If this is so, then the marked proliferation of the epithelium of the chorionic villi met with in these cases may lead to the over-production of such a toxin

¹ *Trans. Obstet. Soc. Lond.* (1899), 1900, xli, pp. 303-338.

and so explain the frequency with which albuminuria and nephritis are found associated with this variety of molar pregnancy. At the same time the complete absence of the foetus renders it difficult to accept the explanation offered by Marchand—namely, that the syncytium is concerned with the metabolism of the foetal tissues from the maternal blood and the cells of Langhans's layer with the elimination of waste products, and that when excessive overgrowth of these cells occurs increased tissue metabolism will take place in the foetus with a resulting increase in the amount of waste products to be eliminated, and the accumulation of these toxic waste bodies in the maternal blood may lead to the production of such a condition as acute nephritis.

The pathological changes in the kidneys and the clinical history appear to me to be in favour of the view held by some authorities, and ably supported by Dr. Herbert French in his Goulstonian Lectures for 1908, that the condition of the kidneys associated with oedema and albuminuria occurring mainly in the early months of pregnancy, and the condition of the kidneys found so commonly in cases of eclampsia occurring mainly in the later months or at full term, are in reality the same, and that they differ from one another only in degree. They are both varieties of nephritis, one subacute and the other very acute, but both no doubt due to some poison and quite comparable, as Dr. French points out, to the varying condition of the kidneys seen in cases of nephritis following scarlet fever. If future investigations should show that this view is really a correct one, then our conception of the kidney affections associated with and due to pregnancy will be simplified very much. The four cases recorded by Dr. French in which a previous attack of eclampsia was followed by the occurrence of albuminuria and oedema in the early months of a subsequent pregnancy seem to me to be strong evidence in favour of such a view.

The fact that albuminuric retinitis was present is also an interesting feature of this case, since it developed while the patient was in the ward under treatment. It is met with more often in the nephritis of pregnancy than in any other form of acute kidney disease, and is of bad prognostic omen, for of the seventy-one cases recorded by Dr. French of the albuminuria of pregnancy without eclampsia in which albuminuric retinitis was found, no less than 15 per cent. died.

No doubt, in view of the unfortunate death from sudden heart failure, it would have been better to have given an anæsthetic, and to have emptied the uterus at one sitting, rather than to have relied upon the introduction of bougies to induce labour. A considerable number of

cases have now been recorded in which the patients have died from hæmorrhage as a result of the presence of a hydatidiform mole; among the 210 cases collected by Findley,¹ for example, there were seven deaths from this cause. Although in this instance the patient did not actually die of hæmorrhage, yet no doubt her death from cardiac failure was in part due to her anæmic condition, the result of the several attacks of severe bleeding she had had. A lesson to be learnt, therefore, is that it is better not to temporize in any case in which the presence of a vesicular mole is suspected, and the sooner the uterus is evacuated the less the danger for the patient.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) expressed his thanks for these two valuable communications. Dr. Blacker's specimen was admirably described and beautifully illustrated, and the condition of the kidneys was of great interest. He had long thought that a great deal too much importance was being given to these so-called lutein cysts. When one remembered the frequency with which cysts of the corpus luteum are met with both in the unimpregnated and in the pregnant, and the large number of hydatidiform moles which had been removed in times past without the subsequent development of ovarian tumours (he did not think he had had a single case of such development in his own practice), when one further recalled that hydatidiform moles and chorion-epithelioma occurred unaccompanied by the cysts, he thought that the lutein cysts might, in many cases at all events, be left alone, and the evidence brought forward by Dr. Blacker and Dr. Russell Andrews strengthened him in that opinion.

Dr. CUTHBERT LOCKYER said the thanks of the Section were due to Dr. Russell Andrews for recording a series of such interesting cases and for raising a valuable topic for discussion—that of the relationship between polycystic lutein changes in the ovaries and the occurrence of vesicular mole and chorio-epithelioma. This subject had interested Dr. Lockyer since the year 1901, when he investigated a uterus and bilateral ovarian cysts sent to him by his colleague, Mr. Malcolm. This case was published in the *Transactions of the Obstetrical Society*, London, in 1903, as one of the chorio-epithelioma uteri removed twenty-one days after the expulsion of a vesicular mole and associated with compound lutein cystomata. Through the courtesy of Mrs. Scharlieb, Drs. Williamson and Oldfield, the speaker was provided with three more cases of the same kind, and they formed part of the subject-matter of a paper on the "Corpus Luteum," published in 1905, and to which Dr. Russell Andrews had referred. The points definitely known about these compound lutein cysts were that they possessed strikingly characteristic features both to the naked eye and

¹ Palmer Findley, *Amer. Journ. Med. Sci.*, Philad., cxxv, 1903, p. 403.

on microscopical examination; secondly, they were never found excepting in association with vesicular mole and chorio-epithelioma. The point yet to be decided was—what relationship do these ovarian and ovular changes bear to each other? Was Pick's view (based on the Born-Fraenkel hypothesis) correct, which assigned a "chorio-epitheliomatous reaction" to the excess of lutein substance; or, could the two not dissimilar conditions of the ovary and ovum be due to a common cause, the nature of which was still undetermined? The time had not yet come when this difficult point could be settled. Dr. Lockyer had availed himself of every opportunity to examine ovaries removed from pregnant women. Such material was slow in accumulating, and it was especially difficult to obtain both ovaries from the same subject; he had only studied both ovaries in three cases; in one case from a patient who died during the first month of gestation of septic peritonitis; in another from a patient who underwent hysterectomy for fibroid at the seventh month, and the third from a case of Cæsarean section and Wertheim's panhysterectomy performed at seven and a half months' gestation. These ovaries showed no excess of lutein tissue if the presence of the corpus luteum of pregnancy be excluded, and in no way resembled the polycystic ovaries which formed the subject of discussion. The same might be said of single ovaries derived from cases of ectopic gestation, where it had been the speaker's experience to observe in several instances cystic ovaries enlarged to the size of a small orange, but these cysts had never been of the lutein variety. On the other hand, Seitz and Wallart claimed to have found excess of lutein tissue persisting throughout pregnancy, and disclaimed any causal relationship between excess of lutein and syncytium. On the last occasion, when this subject was discussed at the London Obstetrical Society, Dr. Blacker and the President took the view that there could be no causal relationship between the proliferation of lutein cells and that of the trophoblast, the President basing his opinion upon the fact that there were no cysts in his own case published in 1896. Dr. Lockyer had similarly noted that in his case, reported in *Transactions of the Obstetrical Society*, 1902, there was no lutein excess, but here, as in Dr. Spencer's case, the tissues were obtained post mortem, the uteri in each case being gangrenous. In addition to control investigations on the ovaries removed in cases of normal pregnancy the solution of the problem under discussion would be aided by such clinical observations as those recorded by Dr. Russell Andrews. If large compound lutein cystomata were observed to disappear subsequently to the removal of the associated vesicular mole without harm to the patient, then Pick's theory fell to the ground completely. Just as the Born-Fraenkel theory (which states that the corpus luteum is essential to embedding of the ovum in the early months) received a severe blow from the fact that pregnancy went to term in the Essen-Müller case (where both ovaries had been removed in the early weeks), so was Dr. Russell Andrews's case of disappearance of a lutein cystoma an equally severe blow to Pick's elaboration and application of that theory to the causation of trophoblastic overgrowth. Through the courtesy of Mr. H. S. Clogg, Dr. Lockyer was able to give the details of another case where what

was probably an ovarian cyst of the lutein variety had disappeared. The patient was admitted into the Kingston Infirmary in October, 1905, suffering from sapræmia after abortion. There was evidence of great loss of blood, and the woman was bleeding from the uterus at the time of admission. The uterus was enlarged, and there was a swelling about the size of a hen's egg, noted by Dr. Armstrong as "in the neighbourhood of the left broad ligament." The patient considered herself to be three and a half to four months pregnant at the time of the abortion. The uterus was twice curetted for hæmorrhage and offensive discharge, but little material came away, and this was not examined microscopically. By February, 1906, the ovarian swelling had disappeared, and the woman went home. She returned again in January, 1907, when Mr. Clogg saw her, and diagnosed a large fibroid of the uterus and bilateral ovarian cysts; he operated, and found two large compound lutein cysts and chorio-epithelioma uteri. The woman died two months later from recurrence on the peritoneum. In this case, although there was no proof that the aborted ovum was a vesicular mole, or that the swelling was a lutein cyst of the ovary, yet this assumption was, the speaker thought, justifiable; and if this view be taken, the case shows the possibility of these cysts enlarging again after subsidence, and raises the question whether it is safe to leave them alone when discovered. Dr. Lockyer finally recalled the fact that he found a chorio-epithelioma in the uterus only twenty-one days after the expulsion of the mole, and another five weeks after expulsion of the mole, in cases of polycystic lutein disease; in each case removal of the uterus and ovaries had resulted in freedom from recurrence. Nevertheless, he hoped, for the sake of women the subjects of vesicular disease of the chorion, that future experience would prove that removal of the enlarged ovaries was not a necessity.

Mr. TARGETT referred to a similar case which he had operated on in October, 1906. The patient, aged 26, in the fourth month of her second pregnancy, came under observation for an abdominal tumour of an indefinite nature, situated above the pregnant uterus. A diagnosis of ovarian cyst with pregnancy was made. The operation revealed two polycystic ovarian tumours, the size of large cocoa-nuts, one of which was wedged in the pelvic cavity behind the uterus. They had very thin translucent capsules, and tore readily in removal. Double ovariectomy was performed. As the tumours were typical lutein cysts, it was feared that the pregnancy would be molar, though there was no undue enlargement of the uterus. The abdominal wound healed well, but on the twenty-second day the patient miscarried. The fœtus was partially macerated, and the placenta contained large infarcts; on careful examination it was found that a small portion of the placenta was mingled with the cysts of a vesicular mole. For some months after this occurrence the patient was in a low state of health, with much mental depression. She had irregular monthly hæmorrhages, sometimes lasting for seven or eight days, at others only a "show." But on one occasion, about a year after the operation, when travelling in Germany, the bleeding was so excessive that the vagina had to be plugged. At this time there was loss of weight and anæmia, but no

enlargement of the uterus, and no evidence of any pelvic growth. She was treated with calcium chloride and ovarian extract. Slight bleeding occurred at less frequent intervals until the summer of 1908. Since then there has been no hæmorrhage, and the patient is now (February, 1909) in excellent health. The case was of interest as showing the association of large lutein cysts with vesicular degeneration of the chorion, although only a small portion of the placenta was so affected.

Dr. R. H. PARAMORE said he was interested in the case reported by Dr. Blacker from the point of view of the causation of the albuminuria in association with hydatidiform mole, and of the sudden cardiac collapse which occurred, ending in death. He suggested that the underlying cause of these clinical signs, and the changes in the liver and kidneys which were found post mortem, was a mechanical one, and that this was shown by the rapidity of the pulse and the final syncope. In Bright's disease, with such an amount of albumin in the urine (50 to 25 per cent.), the pulse was, except in conditions allied to the case described, invariably slower than normal. In Dr. Blacker's case it was stated to have been rapid. Ahlfeld¹ had recently brought forward the view that eclampsia, which he (Dr. Paramore) took as being the finale of the albuminuria of pregnancy, was the result of a mechanical cause, in support of which he (Ahlfeld) adduced the fact of its occurrence in the great majority of cases in primiparæ, and stated that when it occurred in multiparæ it was usually due to the remains of a previous nephritis. Dr. Paramore said it was well known that the foetal head usually occupied a lower position in the pelvic cavity at term in primiparæ than it did in multiparæ; and that Stengel and Stanton² had shown, without intending to show it, that the maternal heart was situated at a higher level within the thorax at term in primiparæ than it was in multiparæ, demonstrating that the diaphragm was displaced more upwards in the former than in the latter as a result of pregnancy. These conditions could only be due to the activity of the abdominal wall musculature, which did not relax in primiparæ as much as it did in multiparæ. It was not the size of the uterine tumour, in this case evidently considerable, which produced the causative pressure conditions, but the rapid rate of increase in its mass which was responsible, and he would like to ask Dr. Blacker what the rate of growth was in his case, and what was the condition of the abdominal wall.

Dr. BLACKER thanked the various speakers who had taken part in the discussion. He had hoped, however, to have heard the views of some of the Fellows on the question of the albuminuria of pregnancy in relation to a vesicular mole, and to the acute necrotic changes found in the kidney in this case. In answer to Dr. Paramore, he was not a believer in the mechanical theory of albuminuria, and he thought it was negated on many grounds.

¹ Ahlfeld: "Zur Pathogenese der Eklampsie," *Zeitschr. f. Geburts. u. Gyn.*, Stuttgart, 1908, lxiii, p. 295.

² Stengel and Stanton: "The Heart and Circulation in Pregnancy and the Puerperium," *Univ. Pennsylv. Med. Bull.*, 1904-5, xvii, p. 202.

Obstetrical and Gynaecological Section.

March 11, 1909.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

A Specimen of Dermoids: One in each Ovary in an old Lady aged 92.

By W. RIVERS POLLOCK, M.D.

THE patient had been married and had had six children. She had been attended by Sir James Simpson and Dr. Matthews Duncan. After one of her confinements the patient had had an inflammatory attack, probably peritonitis, which may have been due to injury to one of these tumours, one of which reached nearly to the umbilicus; the other was about the size of an ordinary orange. Later in life she had had some pain in the abdomen and vomiting, which were thought by her doctors to be due to the fibroids of the uterus, from which she was informed she was suffering. She was told that they would probably settle down and give her no more trouble. This in fact they did, though the tumours were dermoids of the ovaries and not fibroids of the uterus. The old lady died last January from senility.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) said he was glad that this interesting specimen had been brought before the Section. He had for some time thought that it was not right in the case of a young childless woman with bilateral ovarian dermoids to remove the tumours until a fair opportunity had been given for the patient to become pregnant, and that in many cases it might be advisable to leave a portion of one ovary so as not to deprive the patient of the chance of maternity.

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Dr. ANDREWS said that he had removed an ovarian dermoid from a patient aged 75 who had had thirteen children. The tumour had caused no symptoms during pregnancy or labour, and had given rise to pain for only a short time before the patient came to him.

Dr. LEWERS thought that the interesting case described by Dr. Pollock was an exceptional one, and in no way invalidated the general rule that dermoid ovarian tumours should be removed whenever discovered. They were very prone to suppurate, and sometimes then discharged into the bowel or bladder, and, as a complication in labour, they were at times the cause of rupture of the uterus.

Torsion of Parovarian Cyst during Pregnancy.

By W. GIFFORD NASH, F.R.C.S.

A WOMAN, aged 28, who had been married six years and had no children, was admitted into the Bedford County Hospital just before midnight on November 13, 1908. Her last period ended on August 9, ninety-six days before admission. During the morning of November 13 she was seized with severe pains in the abdomen, vomiting and hic-cough. Her temperature was subnormal and pulse 108. I saw her half an hour after midnight, when it was evident some abdominal catastrophe had happened. There was a lump in the left lower abdomen, and on vaginal examination the pregnant uterus could be felt, but nothing else.

Operation: The abdomen was opened and some blood-stained fluid escaped. In the umbilical region was a dark claret-coloured swelling, about the size of a cocoanut, which was attached to the left side of the uterus by a pedicle twisted two turns.

The tumour was removed and the patient recovered and left the hospital on December 12. The tumour lay between the layers of the left broad ligament. The ovary which was enlarged and engorged with blood was attached to the back of the tumour. The Fallopian tube was stretched over the whole length of the tumour. On opening the tumour it was found to be a simple unilocular cyst containing blood-stained fluid. These four cardinal conditions show that the cyst was parovarian.

Hæmorrhage into an Ovarian Sarcoma producing Symptoms resembling Torsion.

By W. GIFFORD NASH, F.R.C.S.

A MARRIED woman, aged 29, was seen on the evening of October 26, 1908. Her periods had been regular. For six weeks she had felt some discomfort in the abdomen, and for three days she had had pain. Twenty-four hours before I saw her the pain had become acute, and vomiting had occurred. Her temperature and pulse were rising, so that when I saw her the temperature was 102° F. and the pulse over 100. In the left lower abdomen was felt a lump as large as a big cocoanut.

I diagnosed a twisted ovarian tumour, and next morning removed a solid ovarian tumour which had in places become adherent to the surrounding tissues by a recent peritonitis. There was no torsion of the pedicle and no evidence of there having been any. The patient made a good recovery. The tumour removed was claret-coloured, and after hardening was bisected. It consisted of a soft growth with profuse hæmorrhage into its substance. Microscopically the growth proved to be a round-celled sarcoma.

This appears to be an exceedingly rare condition, and I can only find one mention of hæmorrhage into a malignant ovarian tumour in the Transactions of the Obstetrical Society. This was a case exhibited by the late Dr. Cullingworth in 1891. Dr. Cullingworth did not mention what were the symptoms of his case.

In 1907 I had a similar case in a single lady, aged 57, who had Graves's disease and a very weak heart. On September 17, 1907, whilst I was examining her abdomen, she was seized with severe pain, followed by every sign of acute peritonitis. When this subsided a lump the size of a cricket ball could be felt in the left lower abdomen. I diagnosed a twisted ovarian tumour.

On October 7, at 5 a.m., she had another attack of acute pain and vomiting. The vomiting of reddish-black fluid persisted for seventy-two hours and the abdomen was enormously distended. When a fatal issue appeared imminent there was a sudden escape of flatus followed by recovery.

On October 20, 1907, I removed a large ovarian tumour consisting of a soft columnar-celled carcinomatous growth into which hæmorrhage

had occurred. There was no torsion of the pedicle. The patient recovered from the operation, but died fifteen months later from intestinal obstruction due to recurrent growth in the uterus.

There was a curious coincidence in this case, as in the same house seven years previously I removed a large malignant ovarian tumour and a columnar carcinoma of the small intestine from a lady who died about a year later from a recurrence in the uterus causing profuse hæmorrhage. In her case the symptoms were the same, but I cannot remember whether there was hæmorrhage into the tumour.

Remarks.—I have brought these cases forward for two reasons: firstly on account of the apparent rarity of spontaneous hæmorrhage into malignant growths of the ovary, and secondly because the symptoms are those of torsion of an ovarian cyst—viz., acute pain, vomiting, intestinal obstruction, with shock and low temperature at first, followed by fever and the signs of a local peritonitis. It is said that in some ovarian tumours the results of a twist are found by operation when no twist is present, as the twist had become spontaneously untwisted. I do not think in either of these cases there was any reason to suppose that such a thing had happened.

Report of Pathology Committee.—Sections have been cut from three other parts of the growth, but the tissues are too necrotic to enable us to form an opinion as to its nature.

Hæmorrhage into the Great Omentum and Peritoneal Cavity following a Strain.

By W. GIFFORD NASH, F.R.C.S.

A NURSE, aged 43, on January 1, 1909, whilst lifting an old lady, felt something give way in her abdomen, but went on with her work. On January 7 she came to see me complaining of a pain and discomfort in the abdomen, which had continued since the strain. On examining the abdomen, which was very fat, there was tenderness just below the umbilicus, but nothing else could be made out. I thought she had strained the left rectus. On January 12 she again came to see me, complaining of discomfort and flatulence in the lower abdomen. I did not examine her, and she was able to continue her work. On January 16 the abdomen was prominent, resonant in both flanks and along the transverse colon, and there were signs of fluid. I thought she had an

ovarian cyst which had become twisted, so took her into hospital for operation.

Operation (January 18, 1909): The abdomen was opened and the peritoneal cavity found to be full of blood-stained fluid. Eight pints were removed. The pelvis contained several subperitoneal fibroids, the largest of which, growing from the fundus of the uterus, was ligatured and removed. Above the umbilicus was found a soft fleshy mass, which felt like a placenta and which proved to be the great omentum infiltrated with blood. It was soft and friable and free from adhesions. It was ligatured with catgut and removed from its attachment to the transverse colon. The stump left behind was thick and infiltrated with blood, and near the splenic flexure there was blood effused under the peritoneum covering the colon. The patient made a good recovery, the only drawback being some rather profuse bleeding from the bowel on the seventh day following a dose of castor oil.

Remarks.—This appears to have been a rare accident, as I cannot find any reported case of hæmorrhage into the substance of great omentum. It seems probable that in this case the great omentum was bruised or torn at the time of the strain on January 1. It is possible that in bending down the omentum got caught on a small fibroid projecting from the fundus of the uterus and was torn when the patient raised herself to the upright position. It is difficult to understand why the symptoms developed so slowly, when we consider the state of the omentum at the time of the operation.

Further History communicated by Dr. Gifford Nash since exhibiting the Specimen.—After the first operation—January 18, 1909—the patient went on well, and got up about the ward. Then she developed slight phlebitis in her leg, and had to go back to bed. On March 12 the abdomen had refilled with fluid, and on Saturday, March 13, Dr. Gifford Nash reopened the abdomen above the umbilicus, and removed 7 or 8 pints of blood-stained fluid. The lesser omentum was found to be in the same state as the great omentum at the previous operation. It being impossible to remove it, and no definite bleeding point being found, the wound was packed with gauze. The patient died on March 14, and a post-mortem examination was made on the following day. The gastro-hepatic omentum was infiltrated with blood, and what looked like a small blood-cyst was cut into. In the left lobe of the liver was a secondary growth, and scattered about the surface of the liver were several small nodules. No other lesions of importance were found.

Report of Pathology Committee.—We have examined sections cut from the specimen, and find that the omentum contains a malignant growth, the histological appearances of which resemble those of a perithelioma.

In reply to the President Mr. NASH said that in the case of omental hæmorrhage there were no signs of there having been any adhesions of the omentum to the uterine fibroids.

A Case of Twin Pregnancy in a Fallopian Tube.

By HENRY RUSSELL ANDREWS, M.D.

A. J., AGED 33, was seen by me in consultation and sent into the London Hospital. She had had one child ten years ago and no miscarriages. The catamenia had been regular until three and a half months ago, since when there had been no discharge whatever except a slight amount of blood-stained discharge for the last twenty-four hours, after vaginal examination. She had had pain like cramp for three weeks in the abdomen and in the rectum, very severe for the last few days. She was pale and exhausted looking. The pulse-rate was 108. The abdomen was tender all over and rather rigid. On the right side was a rather indefinite mass rising out of the pelvis, reaching almost up to the umbilicus. The uterus was distinctly enlarged and the cervix soft. Behind the uterus was a soft, very tender mass continuous with the abdominal swelling.

Diagnosis: Extra-uterine pregnancy. From the absence of uterine hæmorrhage I thought it very probable that the fœtus was alive. On opening the abdomen and separating omental adhesions a gestation sac was found behind and to the right of the uterus, with a little recent blood-clot. Through the amnion, which was bulging through the posterior wall of the sac, could be seen twin fœtuses. I ruptured the sac and removed the whole of the placenta and two living fœtuses. The sac was made up of tube and broad ligament. Apparently the posterior layer of the broad ligament was giving way and the pregnancy was about to become abdominal. As the uterine end of the tube was thrombosed I excised a wedge-shaped piece of the uterine cornu and removed the whole tube, together with part of the broad ligament. There was a good deal of oozing from the floor of the pelvis and from

the anterior surface of the rectum, but this was stopped without difficulty and the abdomen closed. The patient made an uninterrupted recovery. Although a thick uterine decidua was seen on excising the cornu no membrane was passed.

The fœtuses are both males, of about fourteen weeks. One of them has a protrusion of small intestine in the umbilical region. The pregnancy was unilocular. Sections of the tubal mucous membrane show well-marked decidual reaction.

Twin pregnancy in a Fallopian tube is a comparatively rare occurrence. Schauta¹ found nineteen cases on record, Saniter² described another, and McCann³ recorded another three years ago. My case is the twenty-second recorded.

DISCUSSION.

Dr. MCCANN said he had met with an example of tubal twin pregnancy which was published in the *Journal of Obstetrics and Gynæcology of the British Empire* for December, 1906, and he now circulated a drawing of the specimen in order to compare it with that of Dr. Andrews. In both, the fœtuses were contained in one sac, and as far as he (Dr. McCann) could discover these two specimens were the only ones exemplifying this condition. It was stated that eighteen cases had been collected by Schauta, but a search of the literature had failed to find the records, whilst in Schauta's own case and that recorded by Saniter two separate ova were contained in the tube.

Dr. ANDREWS, in reply, said that he had not read Schauta's paper in full, and had only read the abstract in the *Zentralblatt*. He had excised the interstitial part of the tube because it contained a thrombus as thick as his little finger on the uterine side of the ovum.

A Fibroid Tumour spontaneously expelled from the Uterus seven and a half weeks after Delivery.

By ARTHUR H. N. LEWERS, M.D.

THE patient was aged 34. She had only been married twelve weeks when she was sent to see me on June 29, 1908, by Dr. Parsons, of Westbury, Wiltshire. She had had some difficulty in passing water three weeks after marriage. This became worse on June 18, when

¹ *Zentralbl. f. Gyn.*, Leipz., 1905, xxix, p. 45.

² *Zeits. f. Geb. u. Gyn.*, Stuttg., 1905, lv, p. 492.

³ *Journ. Obstet. and Gyn.*, Lond., 1906, x, p. 628.

retention occurred, and a catheter had to be passed on that date and for some days afterwards. For a few days before I saw her she had been able to pass water naturally. She gave a history that the water had passed very slowly for a long time—even before her marriage. Menstruation had been regular every four weeks before marriage. The period lasted six days, and there was a good deal of pain on the first day in the lower abdomen and back and down the thighs. Since her marriage she had “seen nothing.” The last period occurred during the last week of March, 1908. She was married on April 7, 1908, and counting from that date, if she were pregnant, the confinement should have been about January 12, 1909.

On examination (June 29, 1908) the breasts were found to be distinctly active. There was a hard swelling occupying the lower abdomen and reaching nearly to the umbilicus. On vaginal examination a hard convex mass was felt occupying the pelvis and bulging down the posterior vaginal wall. The cervix was high up behind the pubes and completely out of reach.

I thought most probably she was pregnant, and advised that she should be seen at intervals during the pregnancy to see if the pelvic fibroid would move out of the pelvis, as, if it did not do so, delivery could only be effected by Cæsarean section. Towards the end of October some vaginal hæmorrhage occurred, and Dr. Parsons sent her up to the London Hospital, where she was admitted under my care.

I examined her there on October 27, and found the pelvic fibroid had moved up completely out of the pelvis. The cervix occupied its normal position, and there was a medium-sized mucous polypus seen attached to it, which seemed to me to have probably been the source of the bleeding just mentioned. At this time the abdominal tumour reached the epigastric region, and its surface was irregular. After a few days' rest in hospital, as there was no further sign of miscarriage, she was sent out, but directed to come in again in time for the confinement.

Readmitted December 6, 1908. On December 9 she had a rigor, and the temperature rose to 102° F. Labour came on and she was delivered naturally of an eight months' fetus at 2 a.m. on December 10. The child, a female, was stillborn. On December 10 the temperature reached 103° F., and from that date till February 1 the temperature was persistently febrile at night, varying from 100° F. to 102° F. and 103° F., often falling to normal, or below normal, in the morning. She had a rigor on December 30, the temperature reaching 104° F. I examined her on December 17, and found the greater part of the abdomen still

occupied by an irregular tumour. It reached 2 in. above the umbilicus in the middle line, and, in the right nipple line, to within 1 in. of the costal margin. There was no evidence of any pelvic inflammation on vaginal examination. In spite of the persistent fever the patient's general condition remained fairly good, and though I thought that the fever was probably due to some degenerative change taking place in the fibroids, I determined to refrain from interfering as long as possible. On February 1, 1909, a fibroid tumour the size of a man's fist was expelled from the uterus, together with about 15 oz. of extremely fetid pus. The temperature fell the same day, and continued normal subsequently. Convalescence was rapidly established, and the patient left the hospital on February 16. On examination a few days before she went out (on February 11) the uterus still reached as high as the umbilicus on the right side; it was freely movable. An irregular projection could be felt at the highest point of the fundus, and another, the size of a tangerine orange, to the right side of the uterus, so that two other fibroids, besides the one extruded, still remain.

Dr. LEWERS, in reply to some questions, said the fibroid shown had undergone necrobiosis, suppuration had then occurred, and the fibroid had been expelled together with the other contents of the abscess (about 15 oz. of very fetid pus) into the uterine cavity, and so externally. Even some ten days after delivery the mass formed by the uterus and fibroid tumours still reached nearly to the ribs on the right side; so that, even if there had been any indication for exploring the uterine cavity, such as a purulent offensive discharge, which there was not, it would have been most probably quite impossible to have reached the situation of the sloughing interstitial fibroid. The question which arose while the fever continued was really whether an exploratory laparotomy, most probably to be followed by abdominal hysterectomy, should be performed. The event had shown that, in this particular instance, the expectant attitude adopted was the right one.

Two Cases of Ovarian Fibroid complicating Pregnancy.

By HERBERT R. SPENCER, M.D.

THE extreme rarity of the co-existence of ovarian fibroids and pregnancy—as shown by the researches of McKerron, Swan, and Coudert—is my reason for bringing these specimens before the Section. In the first case a calcified fibroid incarcerated in the pelvis allowed pregnancy to proceed without giving rise to any symptom till near the term, when

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Cæsarian section and ovariectomy was performed with a successful result in the case of both mother and child. In the second case a large ovarian fibroid was removed with the ovary from the right side, and a small one was enucleated from the left ovary in the fifth week of pregnancy. Abortion ensued; but, owing to the conservative treatment of the ovary, the patient was able to become pregnant again and was safely delivered of a living child. The writer is of the opinion that many ovarian fibroids and some dermoids might be dealt with in this conservative manner with advantage.

CASE I.

Mrs. G., aged 38, married two years and usually menstruating normally, became pregnant in 1903, the last period ceasing on February 24. She had been a patient of Sir John Williams, who diagnosed a uterine fibroid, and on her becoming pregnant advised her to be delivered by Cæsarean section at term. She was brought to me for operation by Mr. T. Hoskin, and was admitted into a nursing home when she was within a few days of term. She was in good health; the urine had a specific gravity of 1010, but contained no albumin. The child presented by the head. The os, very high up, admitted the tip of the finger. A craggy tumour as big as a double fist occupied the pelvis, and appeared to be immovable. In performing Cæsarean section before the onset of labour on November 28, 1903, by means of the usual longitudinal incision, profuse bleeding occurred, owing to the presence of the placenta in front. In order to deliver the child the deeper layers of the uterus were torn through, and the slightly irregular wound thus made was trimmed before the silk sutures were inserted; superficial sutures were also applied. I then pulled the uterus out of the abdomen and found that the tumour was a fibroid of the left ovary with very large veins in its pedicle. It was removed after tying the pedicle with silk. The child, a boy weighing 7 lb. 8 oz., was in good condition and cried at once.

The abdominal wound was sewn up with through and through stitches of silkworm gut, buried silk stitches for the fascia and silkworm gut for the skin. The wound healed by first intention, and the child, suckled by a wet nurse, weighed 8 lb. 7 oz. at the end of the month. Both mother and child were in excellent health when they left the home on December 30. The patient had been once pregnant since the operation, but miscarried at the tenth week in September, 1904. She wrote to me on March 3, 1909, that she and the child (who is 3 ft. 8½ in.

high and 3 st. 6 lb. in weight) were in perfect health. The abdominal scar was quite sound.

The tumour was a fibromyoma, measuring 13.5 cm. by 12 cm. by 9 cm., and weighing 1 lb. 8 oz. The surface was uneven, lobulated, and gritty from calcareous spindles. The ovary was, as usual, stretched out to form a capsule to the tumour, which under the microscope was seen to be a fibromyoma, hyaline and calcified in places. The cut surface of the broad ligament showed the orifices of numerous large veins.

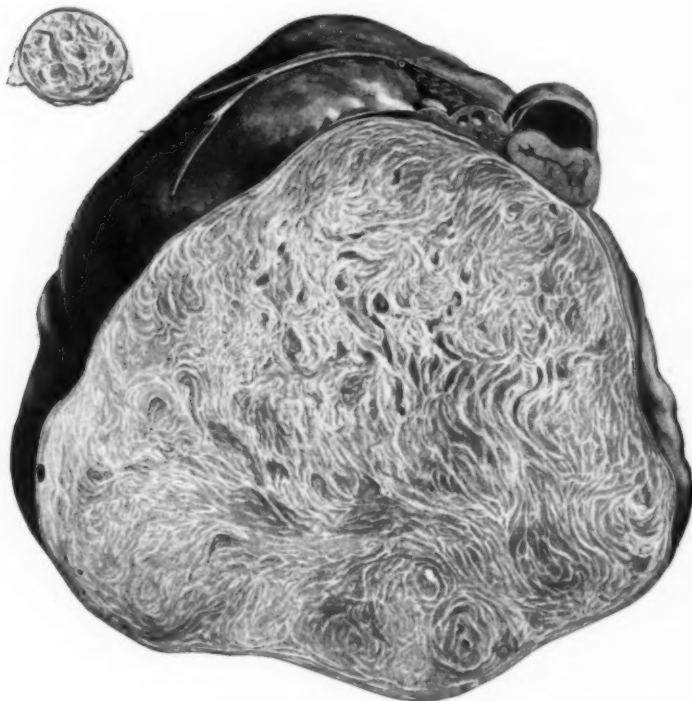
In this case the operation was undertaken in the belief that the tumour was a *uterine* fibroid growing from the back of the cervix and lower segment. It seems probable, however, that the treatment adopted was the best for an incarcerated calcified ovarian fibroid of the size of this specimen. Simple ovariectomy would have been extremely difficult even after withdrawing the uterus from the abdomen. Also the reposition of the tumour, even if possible, would have entailed much risk of injuring the large veins of the broad ligament by the sharp spicules on its surface. Coudert has published a case of fatal intraperitoneal hæmorrhage due to the reposition of such a tumour during labour.

CASE II.

E. M. D., aged 26, was admitted to University College Hospital on March 13, 1906, complaining of a swelling and pain, not constant, in the lower part of the abdomen; also of constipation and pain on defæcation. She had been married for eighteen months and had not hitherto been pregnant. The last period had ceased on February 11. She had first menstruated when aged 12, became regular when aged 16, and did not lose too much. The only family history of tumour was in the case of an aunt who had a tumour in the back.

The breasts were small but firm and contained colostrum. A very hard, smooth tumour, as large as the uterus at the fifth month of pregnancy, occupied the lower abdomen; it could be moved from side to side as if it floated on a little fluid. The mucous membrane of the vaginal outlet was pink, not cyanosed at all. The edge of the cervix was rather thickened. On vaginal examination the uterus was found to be pushed forwards and to the left by a craggy tumour continuous with the tumour felt in the abdomen. The uterus appeared to be a little widened and thickened. The left ovary was not felt. The diagnosis was ovarian fibroid complicating a pregnancy in the fifth week. On March 17, 1906, I removed the tumour through an abdominal incision

5 in. long, tying the pedicle with silk, and also understitching the vessels separately. The left ovary was then examined and found to contain a fibroid of the size of a small walnut. This tumour (*see figure*) was enucleated from the ovary after circumcision of its base, and the wound left in the ovary was sewn up with fine silk. The uterus was not much enlarged, but was redder and softer than normal. There was a small quantity of serous fluid in the peritoneum. The abdominal wall was



Bilateral ovarian fibroids complicating pregnancy (two-thirds natural size). The tumours were removed at the fifth week of pregnancy, the smaller, left-sided tumour being enucleated and the wound in the ovary stitched. The patient aborted, but subsequently became pregnant and had a living child.

sewn up with silkworm gut and buried fascial stitches of silk. The operation lasted thirty-five minutes.

The patient recovered well, the temperature only once rising above 100° F. (100·4° F. on the third day), after which it was practically

normal (highest 99·6° F.) till she left the hospital on April 12. A little bleeding occurred from the uterus on the second day after the operation, and the patient aborted on the fifth day. The wound healed by first intention. Subsequently the patient became pregnant, and was delivered of a full-term living boy on September 22, 1907, and with her child was in excellent health with a sound abdominal scar when examined on March 2, 1909.

The right tumour (*see figure*) weighed 3 lb. 8½ oz., and measured 18 cm. by 15 cm. by 11 cm. The surface was somewhat irregular and showed several large veins and a slender sinuous fibrous band on its surface. The Fallopian tube was very thin. Projecting from the surface of the tumour were the remains of the ovary containing a corpus luteum, 2·5 cm. by 1·5 cm. in diameter, capped by a cyst of the size of a filbert with a little hæmorrhagic cyst in its wall. Several small cysts were seen in the neighbouring part of the ovary. From the corpus luteum the ovarian tissue could be traced as a capsule round the tumour, finally becoming of membranous tenuity. Under the microscope the tumour was a fibromyoma. In the ovary were numerous degenerated corpora lutea; the large corpus luteum of pregnancy had the typical structure.

The small tumour from the left ovary measured 2·5 cm. by 2·25 cm. by 2 cm., was smooth above and rough below, where it had been enucleated from the ovary (*see figure*). A small portion of ovary was still attached to one side of the specimen. The cut section of the tumour had the usual fasciculated appearance of a fibroid. Under the microscope the tumour was a fibromyoma and was permeated with small vessels.

Report of Pathology Committee.—We have examined sections prepared from the specimens, and find that many of the nuclei are elongated and resemble those of muscle-cells, but find no definite muscle-bundles.

The PRESIDENT (Dr. Herbert Spencer), in reply to Dr. F. G. Stevens, thought that it was quite possible that these hard ovarian fibroids were all really fibromata and were correctly designated by the French term *fibrome*; but when they contained spindle-cells with elongated nuclei exactly resembling those seen in uterine fibromyomata, he thought it better to call them fibromyoma; they differed markedly from the hard ovarian fibromata containing typical fibrous tissue. He would bring the matter before the Pathology Committee for their opinion, though he thought it a matter on which it was not right to dogmatize.

A Fatal Case of Toxic Vomiting of Pregnancy with Microscopical Sections of Liver and Kidneys.

By R. DRUMMOND MAXWELL, M.D.

THE rarity of this variety of toxæmia of pregnancy fully justifies its being placed on record, and the interest of the case is not lessened by the difficulty in diagnosis.

S. C., a married Jewess, aged 26, was admitted to the London Hospital on the afternoon of October 2, 1908, under the care of Dr. Lewers, whose permission I have to record the case. The patient had been married six months. There was a history of three months' amenorrhœa. Practically from the onset of the amenorrhœa there had been vomiting, which was continued up to the day before admission, when the patient had been able to retain milk and lime water. The bowels were said to have been acting regularly. A female friend of the patient had volunteered to the Sister of the ward that the patient knew herself to be pregnant, and had been taking drugs to terminate the pregnancy.

On admission patient looked very ill, though not remarkably wasted. The tongue was coated; the mouth was septic, with sordes on the teeth. There was no suggestion of a blue line on the gums pointing to lead as the cause of the condition. Patient's mental condition was noticeable. She was dull and apathetic, but occasionally suggested a mild delirium. The heart-sounds were weak. Pulse 140, very feeble and running. No abnormal physical signs in the lungs. The temperature was slightly raised above the normal, 99·4° F. In the hypogastrium a swelling could be felt several fingers'-breadths above the pubes, confirmed on vaginal examination as the uterus pregnant between the third and fourth months.

October 3: At 1 a.m. in the morning, eight hours after admission, patient suddenly became delirious and almost maniacal. There was extreme restlessness and shouting. Prominent delusions were that her husband was being murdered, and that the nurse had cut off her tongue. Towards the morning patient became quieter shortly after the administration of paraldehyde, 2 dr. An examination of the urine in the morning following admission showed the following characters: Specific gravity 1345, acid, no albumin; three drops of urine completely reduce

Fehling's solution in a test tube ; $\text{Fe}_2 \text{Cl}_6$ gives a definite but not marked acetone reaction. Patient remained in a semi-conscious condition for some hours after the paraldehyde.

October 4 : The condition had apparently improved. The drowsiness had passed off, though the patient was not sensible. She passed the day talking incoherently.

On October 5 and 6 there was a gradual relapse to the former semi-comatose condition. The pulse ominously quickened to 160, and at times became uncountable at the wrist. There was a gradual rise in temperature from the fourth to the sixth day, culminating in a maximum temperature of 101.2°F . On the sixth day the urine contained neither sugar, albumin, nor any evidence of renal degeneration in the shape of casts, red or white blood-cells. The specific gravity was 1030. There was a deposit of urates. The limbs were flaccid, but there were occasional twitchings of the fingers and arms. The knee-jerks were absent, only the plantar reflex being obtained ; an attempt was made to investigate the condition of the optic discs, but proved unsatisfactory owing to the patient's restlessness and position in bed.

October 7 : The condition of partial coma remained unchanged. There was some return of vomiting, but not severe. There was a conflict of opinion as to whether optic neuritis was present or not. Incontinence of urine had existed for the last two days.

October 9 : In the last two days there had been no alteration in the condition of the patient, except in the direction of the coma deepening. Twitching movements of the hands were slightly more marked. The breathing had now become shallower and of a sighing type with long intervals between inspirations. The existence of optic neuritis was not confirmed. No albumin was detected in the urine. Patient died at noon with a falling temperature, 98.2°F . Pulse 170.

Autopsy, twenty-six hours later, showed the following lesions : Acute endocarditis with minute granulations on mitral valve (in all probability a terminal infection, and in no way influencing her symptoms). Several small hæmorrhages, the size of a split pea, found at the base of the brain in the tuber cinereum. Kidneys are seen in a condition of severe parenchymatous nephritis. There is considerable fatty change. No hæmorrhages are seen. The iron reaction is well given, pointing to a considerable degree of hæmolysis. The liver : There is severe fatty degeneration in the centre of the lobules ; cells in the cavities of the lobules contain much fat. The stomach shows no evidence of gastritis or any acute irritant poisoning ; there is some post-mortem digestion.

Placenta: Sections of the placenta are absolutely normal. Uterus: No attempt at abortion, either natural or artificial, was seen in examining the uterus with foetus in situ. The latter had only recently succumbed.

REMARKS.

There were many conflicting points in the history and signs of the case that rendered the diagnosis difficult; indeed, the diagnosis of toxæmic vomiting, though made very tentatively during life, was only confirmed by the autopsy.

As in so many of the milder or neurotic group of cases of vomiting in pregnancy admitted to hospital, there was complete cessation of vomiting in the ward. There is now little doubt that in the comparatively advanced period of the disease at which patient was first seen, she had entered on that deceptive stage when vomiting ceases and coma is about to supervene.

Mention was made in the examination of the case of the absence of a lead line. This, from the point of view of diagnosis, is well worth a few remarks in itself. As a cause of vomiting in early pregnancy, lead taken with criminal intent must always be thought of, as the practice has reached London from the Midlands, and its effect on pregnancies is a matter of common knowledge amongst women. Lest this view be thought exaggerated, I would record the fact that in the autumn of 1907, in the London Hospital, there were lying five women in various stages of abortion, due to this practice, all with a well-marked lead line, and three with albuminuria. All were suffering from colic, constipation and sickness, and with the albuminuria might easily have misled the mind bent only on a specific toxæmia of pregnancy. In my out-patient practice I now make a point of examining the gums of all my patients in early pregnancy complaining of gastric or intestinal disturbance, and from June to December last year discovered four cases who freely confessed to this cause of their condition.

The question of other drugs taken was never satisfactorily cleared up, but the complete absence of gastritis is strongly against the use of any of the more common so-called abortifacients. The absence of albumin and casts from the urine, as well as certain chemical products pointing to loss of hepatic function, is the puzzling feature of the case, and very difficult to understand in the light of the well-marked parenchymatous nephritis seen post mortem: One can only suggest that had the patient survived a little longer this important evidence of a toxæmia would not have failed to show itself.

The copious amount of sugar found in the urine shortly after admission completely threw one off the diagnostic track, and with the patient's mental condition I do not see how one could have arrived at any other immediate opinion than that the case was one of impending diabetic coma. Its complete absence from the urine next day, and thence onwards till death, made one incline then rather to a transient glycosuria due to a pontine lesion. Nothing definite was found on ophthalmoscopic examination of the fundi. At the post mortem no gross cerebral disease was discovered, and I feel confident that the minute and almost pinpoint hæmorrhage in the tuber cinereum was the result of the preceding maniacal disturbance, and had no relation to the glycosuria which one has now no doubt whatever owed its temporary presence to the dose of paraldehyde.

At the patient's death the diagnosis certainly uppermost in our minds was cerebral disease, and only the post mortem has cleared up the doubtful element in the case. It is a matter of regret that a careful chemical analysis of the urine was not undertaken to estimate the altered ratio (if any) between the urea and ammonium coefficients of nitrogen said to be characteristic of this toxæmia. I can only urge in defence that the characteristic clinical evidence of a toxæmia (albumin and casts) was absent throughout our observations. The amount of urine passed daily was difficult to estimate owing to incontinence. There is, however, no reason to believe that it was markedly diminished. This is the meagre record of a case of what is to us a rare complication of pregnancy, so rare that I imagine many of us have never seen a true case in their hospital practice, and yet apparently so common across the Atlantic that it is practically impossible to read the *American Journal of Obstetrics* or the *Johns Hopkins Bulletin* without meeting constant references to them.

It is in these circumstances that I record a somewhat atypical case lying undetected under one's close observation.

A Case of Primary Ovarian Actinomycosis.

By FRANK E. TAYLOR, M.D., and WELBY E. FISHER, F.R.C.S.

ACTINOMYCOSIS is one of the rarer forms of infection of the human tissues, whilst actinomycosis of the ovary constitutes one of the curiosities of gynæcology, only six cases of this condition being on record. In

none of these has the ovary been the primary seat of infection, but has been infected by extension of the disease originating either in the intestines or in the vagina, uterus and Fallopian tubes. In the case now recorded the ovary was the primary seat of the disease; it is therefore a unique case.

M. A. D., cook, aged 34, single, was admitted into Chelsea Hospital for Women under the care of Dr. W. H. Fenton on June 24, 1908, complaining of abdominal pain. Menstruation commenced when aged 11, and was associated with severe pain in the lower abdomen, but was quite regular until the age of 17, when the patient's health failed; she became thin and drowsy, and menstruation was completely arrested for eighteen months. Her health then improved, patient getting fatter and stronger, and menstruation returned, but was now very irregular and very profuse, the periods lasting ten to fourteen days and requiring two or three dozen diapers, the intervals varying from five days to three months.

About October, 1904, the patient being then aged 30, abdominal pain was first noticed. It was felt in the right hypogastrium, being very severe and throbbing in character, and was aggravated by the recumbent posture and relieved by walking. About this time she also began to sweat profusely at nights, and this continued until admission. After suffering in this way for about six weeks the patient saw Dr. Fenton, who found the right ovary enlarged; he diagnosed an ovarian cyst and recommended admission into hospital with a view to operation. This was declined.

The pain and discomfort continued intermittently for twelve months, the patient meanwhile continuing at work. She then began to suffer excruciating pain on micturition, and for two months passed blood and pus in great quantity in the urine, which also contained small fleshy masses about the size of grapes. A London practitioner was then consulted. He informed the patient that she had a displaced womb and ordered medicine and hot douches and inserted a ring pessary which had to be removed in three days on account of the pain caused thereby. In a couple of months, the pus having then disappeared from the urine, the pessary was reinserted. It now gave relief and was worn for three months. There was no vaginal discharge. The temperature was never taken. The bowels as a rule were constipated.

In February, 1908, patient had a severe flooding and a fainting attack, and the right side from the hip to the toes was disabled by cramp, which lasted continuously night and day for fourteen days. The

same doctor was again consulted, and he reinserted the pessary. There was now a thick yellow offensive vaginal discharge, and although the ring was removed in a few days the discharge continued. As the patient's condition was gradually becoming worse with marked anæmia and loss of flesh and strength, she again consulted Dr. Fenton, who, diagnosing a suppurating ovarian cyst, ordered her immediate admission into hospital.

On admission in June, 1908, the patient complained of severe hypogastric pains which were relieved by the four-hourly application of hot fomentations.

On pelvic examination the cervix was felt to lie low in the vagina. It was eroded and soft and seemed to expand widely into a hard tumour about the size of a child's head. The tumour was of an even consistence, was free from tenderness, and accurately filled the pelvis. It was incarcerated below the pelvic brim, above which it could neither be felt nor percussed. By manipulation in the genupectoral position it was pushed slightly into the abdomen without distress to the patient. The abdominal and thoracic organs and the nervous system showed no abnormality. The urine was acid, specific gravity 1010, no albumin, no sugar. On admission the temperature was 102° F., the pulse 102, and the respirations 28 per minute. On the evening of the same day the temperature rose to 103° F. and the pulse to 120. Next morning the temperature had fallen to normal, to rise in the evening to 102° F., and on the third morning, that of operation, had fallen to normal and remained so after removal of the tumour.

On June 26 Dr. Fenton performed a median cœliotomy. The right broad ligament was found to lie in front of and cover a rounded tumour about the size of a foetal head. The tumour was covered with purulent and caseous material which, on removal of the mass, adhered to the tumour bed. Both Fallopian tubes were occluded, and bilateral salpingo-oophorectomy was performed. The abdominal wound was closed in three layers, and a large rubber drainage tube was inserted. There was marked shock after the operation, the temperature falling to 96° F., with excessive sweating and cold clammy skin, and the temperature did not return to normal until the third day. There was much discharge through the drainage tube, which was changed for a smaller one next day, and removed on the fourth day. There was no abdominal distension. Urine was passed by catheter in fair amount.

On June 30—that is the fourth day after operation—at 3 a.m., after a good night's sleep, the patient passed 12 oz. of urine naturally, the

act of micturition being accompanied by acute abdominal pain. The temperature was 97° F., the pulse 114, and respirations 32 per minute. She became very ill during the day, and by the evening was much collapsed. The temperature had fallen to 96° F., the pulse being 132 and very feeble, and respirations 36. The skin was cold and clammy, perspiration was profuse, thirst was great, and the bowels were opened frequently. The abdomen was a little distended, there was no discharge from the wound, and no localized collection or swelling could be detected. On examination of the thorax a to-and-fro rub was heard at the base of the heart. It alternated in intensity with the respirations and indicated pericarditis. Vigorous treatment was carried out to counteract the heart failure—cold compresses were applied to the cardiac area. Tr. strophanthi 10 m was given four-hourly and brandy $\frac{1}{2}$ oz. every two hours. In the evening large Spencer-Wells forceps were passed through the drainage opening deep amongst the intestinal coils, and a tube was reinserted. An abundant flow of flaky sero-purulent discharge escaped.

The condition became worse during the night, the pulse being very feeble and rapid, approaching 200 per minute. Brown fluid was frequently vomited, and the bowels were opened several times. Champagne was given occasionally; saline injections *per rectum* were not well retained. Two and a half pints of normal saline solution were infused into the left median cephalic vein, patient being insensitive to the incision. The pupils were widely dilated.

On July 1, the next day, patient vomited green fluid frequently. Liquor strychninæ 5 m was administered, followed by 3 m three-hourly. The pulse was 132 and very feeble during the day. As the vomiting persisted the stomach was washed out in the evening. During the night the patient slept four hours, passing urine and stool in her sleep. After this there was a very gradual improvement; the salines and stimulants were gradually withdrawn, occasional vomiting continued, and only fluids could be retained for some weeks. Milk was badly borne. Sweating continued to be a marked feature. On July 7 the tube was withdrawn as the discharge had become very slight. On July 9 there was marked urgency of micturition with dysuria, and the urine became cloudy and offensive; the cystitis continued for ten days. On July 16, on vaginal examination, the uterus was felt to be fixed to the right pelvic wall by an indurated mass. A probe passed 4 in. along the original drainage tube track. On July 23 patient was discharged from hospital.

She was confined to bed at home for a week, and then went to the Convalescent Home at St. Leonards. After a week at the Convalescent Home the lower abdomen became swollen and hard. Hot fomentations were applied and a large abscess burst, pints of pus escaping. As the sinus persisted with copious discharge the patient was readmitted into the Chelsea Hospital on October 3. In general health she was greatly improved, but the abdominal wound had broken down, there being several openings discharging pus, the lowest of which led deeply towards the pelvic cavity. Shortly after admission urine escaped from this opening. Cystoscopy showed a small ragged opening at the fundus of the bladder.

On October 18 an anæsthetic was given, and the abdominal scar was incised and granulomatous material was removed by a sharp spoon. No micro-organisms could be found in this material, and inoculated culture media remained sterile. A catheter was inserted and retained for ten days. On the day following its removal urine again escaped, associated with a brief febrile attack, the temperature running up to 103° F. and the pulse to 120 per minute. The discharge of urine soon ceased, and the wound healed except along the original drainage tube track, where a sinus remained discharging yellow pus. From cultures of this pus the *Bacillus coli* was obtained.

Since early in December the patient has been taking iodides in large doses and her general condition has gradually improved. The sinus has not quite healed, a spot of discharge showing on the gauze dressing each twenty-four hours.

The structures removed at operation consisted of the uterine appendages of both sides. The left appendages showed evidences of chronic inflammation in a thickened and indurated Fallopian tube, with closure of the abdominal ostium, the ovary and mesosalpinx being normal. There was no sign of actinomycosis in the left appendages, either on macroscopic or on microscopic examination. The right appendages also showed a thickened and indurated Fallopian tube, attached to the broad ligament, which was infiltrated with soft friable tissue, from which a solid tumour representing the ovary had become detached. This separate tumour consisted of an irregularly ovoid solid mass, with a tuberoso and in places worm-eaten like surface. It was of a uniformly greyish-yellow colour, and measured, after hardening in formalin, 2½ in. by 2½ in. by 3½ in., being 10 in. in circumference. It weighed 7¼ oz.

On cutting across the mass uniform yellow pus was seen to exude

from numerous points, the cut section presenting a regular honeycomb-like structure, while towards one pole of the tumour a larger abscess cavity, about the size of a walnut, was present. Nodules characteristic of actinomycotic pus were not observed with the naked eye. Films of the pus were made and stained with methylene blue, and degenerate pus-cells and granular detritus were seen, but no micro-organisms of any sort. Agar tubes were also inoculated with the pus and incubated at 37 C., but remained sterile. A portion was excised for microscopic examination, and the tumour was then hardened whole in formalin solution. Sections were stained with hæmatoxylin eosin. The ovary was transformed into a mass of loose connective tissue, with marked small round-celled infiltration, scattered irregularly throughout which were small foci of suppuration; no trace of ovarian structure was left. The cellular infiltration of the connective tissue stroma consisted mainly of lymphocytes. A few giant-cells were observed scattered through the connective tissue, but these were not surrounded by zones of epithelioid cells and lymphocytes as in typical tubercles. The giant-cells were multinuclear, the nuclei being massed around the periphery of the cells, and in some of the nuclei typical somatic mitoses could be seen.

Further sections were subsequently taken from less degenerate parts of the tumour and stained for *Bacillus tuberculosis* by Ziehl-Neelsen's method, but this organism was not found, nor did the streptothrichal mycelium retain the red colour of the carbol-fuchsin, which showed that it was not an acid-fast streptothrix. Sections were also stained by Gram's method and counterstained with eosin. The scattered purulent collections were well seen by this method, as pink areas throughout which small masses of a streptothrix retaining Gram's stain were found. These masses were composed of a close feltwork of mycelium in which the filaments were somewhat granular in appearance, their structure being best seen at the periphery of the mycelial masses. No club forms were observed. These appearances are all typical of sections of actinomycosis as affecting human tissues.

In addition to the ovary the only other organ affected during the patient's illness was the bladder, the discharge from which, occurring first in November, 1905, was not examined microscopically, and its nature therefore must remain uncertain, but signs indicating that this organ was affected did not appear until twelve months after the disease had already asserted itself as an ovarian tumour, and had been recognized as such by Dr. Fenton at his first examination of the patient in

November, 1904. In the absence of any other discoverable focus of disease, and recognizing the great improvement in the patient's general health which resulted from removal of the diseased ovary, we conclude that we are justified in claiming that we have here recorded an undoubted case of primary ovarian actinomycosis. The source of the streptothrix, and its mode of ingress into the body, must, in our case, remain somewhat doubtful. Except for rare and brief holidays, the patient has lived in London as cook in doctors' families for the last sixteen years, and prior to that time was at school in Bristol, but was brought into contact with hay, straw and corn—the usual sources of actinomycosis—in 1903 and 1904, when the house in which she was engaged had a stable near the back door, the patient going into the stable regularly three or four times a day to fetch fuel, or sometimes to feed the horse. We may possibly here find the explanation as to how the disease was contracted, since the symptoms date from 1904.

Since the ovary occupies a secluded position in the closed abdominal cavity, it can only be infected either by direct extension of the disease from an affected contiguous organ or by the blood-stream. As in this case there was no evidence of disease in any adjacent organ the streptothrix must have reached the ovary by way of the blood-stream, the mode of entry into the body being, we suggest, some cryptogenic focus, *e.g.*, the tonsil, through which it is well known micro-organisms may pass into the blood-stream without producing any local lesion. It may further be pointed out that the ovary provides a peculiarly fertile soil for the growth of infective micro-organisms and of detached carcinoma cells, as evidenced by the frequent recurrence of large ovarian growths secondary to mammary and alimentary carcinomata. It can thus be readily understood that the streptothrix may have been deposited in the ovary by the blood-stream, and so have given rise to a primary ovarian actinomycosis.

We are indebted to Dr. Fenton for his kind permission to publish this case.

DISCUSSION.

Professor R. F. C. LEITH congratulated Dr. Taylor on the way he and his co-worker, Mr. Fisher, had elucidated this rare and interesting case. The specimen shown was a good and typical example of ovarian actinomycosis caused by the streptothrix actinomyces, but caution must be exercised before accepting the contention of the authors that the disease was primary in the ovary. Their contention, indeed, went further in that they considered it to be the first authentic case of this nature. Of the recorded cases of actinomycosis

involving the ovary—a not inconsiderable number—there were some in which the evidence favouring a primary incidence of the disease in the ovary was very strong, sufficiently so to convince their observers of their authenticity. The case described in 1892 by Professor Grainger Stewart and Professor Robert Muir, which he himself had seen, was believed by them to be primary, the path of entrance being the Fallopian tube. It was, of course, possible that this was wrong, and there were some considerations against it, but they could not displace it and others from their position until they had heard and been convinced by the arguments which space and time did not very well permit Dr. Taylor and Dr. Fisher to place before them that night. In regard to their own case the clinical history, both before and since the operation, certainly lent considerable support to their contention, but the actual relationships of the ovary to all the neighbouring tissues were not fully revealed. The operation disclosed them in part, but not so fully as post-mortem investigation did. In a case investigated in this manner by him and not yet published the ovary presented a picture very similar to that of the present specimen, though its size was hardly so great. It was densely adherent to all the surrounding tissues, particularly to the fimbriated end of the tube and to the anterior wall of the rectum. The terminal part of the tube was much dilated, its walls thickened, and its lumen in direct communication with one of the abscesses within the ovary. There had been an extension from the one organ to the other, and a study of the parts showed that it had almost certainly been from ovary to tube and not vice versa. The mucous surface of the anterior rectal wall showed no ulceration and no abnormality other than much puckering. The rectal wall was thickened, and in the dense fibrous adhesions binding it to the ovary there were plentiful actinomyces growth. Actinomyces colonies were also found in the rectal wall, particularly in the submucous coat and in the wall of the descending colon. His interpretation of this case, though still *sub judice*, placed the primary disease in the intestine, from which it had spread to the ovary, the intestinal lesions subsequently cicatrizing and healing. He ventured to suggest that a similar interpretation might be placed upon the present case, and he hoped Dr. Taylor would give them more definite information of the relationship found to exist at the time of the operation between the rectum and the ovary. The speculation of the authors that the actinomyces had entered the body by some hidden path, say the tonsil, without causing a local lesion, and been carried to the ovary by the blood-stream, was scarcely probable when consideration was given to the usual behaviour of this germ. One of its chief characteristics was the formation of a local lesion at or near the site of entrance, and a spread therefrom by direct continuity of tissue. True, it did tend to enter the blood-stream and spread by it, particularly the portal blood-stream, but this was characteristically a late method of extension. Further, if such a transference by the blood-stream as that suggested by the authors were probable, should not the ovary have suffered more frequently than history related? The number of recorded cases of human actinomycosis exceeded 1,000, but those of them in which the ovary was attacked numbered

very few indeed in comparison. On general grounds, then, the authors' contention was not a strong one, but on clinical facts it was. The early enlargement and mobility of the ovary, the success of the operation (upon which the surgeon was to be congratulated, for it was no easy matter to remove a spongy organ full of pus and densely adherent, as these actinomycotic cases were), the good recovery and continued health of the patient were strong points in their favour. The authors need not be surprised at their failure to isolate and cultivate the germ artificially from the pus. It was a most difficult germ to isolate from the tissues. Interesting work in its cultivation had been done comparatively recently by Dr. Homer Wright, of Boston, Mass., but this was a subject of more academic than practical interest, and he would not detain them longer by discussing it.

Dr. T. G. WILSON remarked how difficult it was to be quite sure that a condition like actinomycosis was really primary in the ovary, but had not come originally from some part of the bowel. Illustrating this point, he mentioned a case of actinomycosis he had seen, where the vermiform appendix was adherent to a right-sided ovarian mass containing pultaceous material, and that subsequent to operation the presence of actinomycosis was demonstrated in both the appendix and the ovarian mass. In this case the opinion was arrived at that the condition was certainly a primary infection of the bowel which had spread to the ovary via the adherent appendix.

Dr. GRIFFITH suggested that the absence of discussion on this very interesting specimen was due to the rarity of the disease and the probability that few members had met with a case. The only case he had seen affecting the generative organs in a woman was that of a lady, aged about 45, whose illness he watched through until her death. She was under the care of Mr. Furner, of Brighton, and was seen frequently by Sir Frederick Treves and Dr. Goodhart. The nature of the disease was not suspected until a late stage, the characters generally pointing to tubercular rather than malignant disease. She had borne three children, the youngest being ten years. There were two miscarriages between the first and second children. A tubal gestation with hæmatocele was attended by Dr. Griffith and Dr. Matthews Duncan in 1890, which left the uterus fixed posteriorly to the left by adhesions. Since her last confinement she had suffered from profuse menorrhagia, which was little restrained by drugs, and for which she refused other treatment. Since the summer of 1905 she had been ailing, and in the summer of 1906 she had an acute attack of appendicitis-like symptoms, followed two weeks later by left iliac pain, painful micturition with a night rise of temperature to 101° F. He (Dr. Griffith) saw her in October, 1906. The appendix symptoms had then disappeared; the uterus was retroverted and fixed. In the left broad ligament fixed to the side of the pelvis, but not to the uterus, was a flat, *very* tender hard mass. A week later she had definite appendicitis symptoms, and the lump on the left side had increased, extending close to the side of the uterus. On November 3 he did an exploratory laparotomy, finding extensive recent inflammation of all the pelvic organs and peritoneum, great care having to be taken as every part touched

bled very freely. The appendix was soft, pencil sized, and the mesentery much thickened. It was removed. Both tubes were thick, very vascular and adherent, the right less than the left. It was not possible to raise the ovaries from behind the fixed retroverted uterus because of the friability and vascularity of all the structures. The lump on the left side felt much less extensive on intra-abdominal examination than on its vaginal aspect. Near the lower edge of the omentum was a curious-looking, filbert-sized lump like a dark blood-clot, and found, after removal, to contain pus. For a week after the operation her improvement was marked. On the eighth day her temperature again ran up, and the bowels, which had been acting well, became again very constipated. The lump on the left side had not increased, and the extreme tenderness of all parts was diminished. On November 19, there being a further rise of temperature and much more pain on the left side of the pelvic brim, the lump also being softer, he (Dr. Griffith) opened it and drained it *per vaginam*. A very little pus escaped, but the bleeding was profuse, as if the uterine artery had been divided. Ten days later he found the discharge very offensive, there being free drainage through a large gaping opening, and the right broad ligament was for the first time found hard and swollen. Emaciation was proceeding with a great deal of pelvic pain; later, symptoms began of an abscess forming under the left gluteus maximus with great tenderness and some swelling which varied almost from day to day in extent—one day being hardly recognizable and another very marked. Mr. Furner therefore explored and found no pus, but the muscle-bundles were pale and in a peculiar rigid state. A few days after a gush of fetid pus with the characteristic yellow bodies made its appearance, and the diagnosis was then made. From this time the progress of events was downwards, and, in spite of large doses of iodide of potassium, there was no improvement. She died from emaciation after some months of severe suffering. Had the true nature of the disease been suspected it might have been discovered in the appendix and omental lump, the only two parts carefully examined in the laboratory at St. Bartholomew's, the report being chronic inflammation and no evidence of tubercle or malignant disease.

A Case of Primary Carcinoma of the Vagina. Removal of the Uterus and the whole of the Vagina.

By HENRY RUSSELL ANDREWS, M.D.

S. P., AGED 62, was admitted into the London Hospital on September 2, 1907. She had had ten children and three miscarriages. The menopause occurred at the age of 47. For the last six months there had been a very irritating, watery, odourless vaginal discharge, blood-stained at times, more profuse for the last two months. There was

discomfort in micturition, described by the patient as a sensation that something was blocking up the urethra. She complained of general weakness and of wasting for two months, so that her clothes had become loose. She was thin, anæmic and rather cachectic looking. On vaginal examination an ulcer about the size of a two-shilling piece was found high up on the posterior vaginal wall, to the right of the middle line. The ulcer bled on examination, had a hard edge, and was undoubtedly carcinomatous. It was difficult to determine whether it extended on to the vaginal portion of the cervix or not. On rectal examination there was no evidence that the carcinoma had invaded the cellular tissue. The uterus was freely movable.

On examination under an anæsthetic next day it was seen that there was an erosion of the cervix, but no extension of the carcinoma on to that organ. I thought that the best treatment would be to remove the whole vagina, together with the uterus, in one mass. A paravaginal incision was made on the left side, the side remote from the growth, not extending deeply into the vagina, but enlarging the orifice considerably. A circular incision was then made round the vaginal orifice, and the vaginal wall dissected off, first from the perineal body, then laterally, and then from the urethra. As soon as possible the vagina was converted into a closed bag by clamping its lower, free end in forceps bent at a right angle. The separation of the vagina was then completed until the cellular tissue round the cervix was reached. The bladder was separated from the uterus, and the uterovesical pouch of peritoneum opened. The fundus uteri was brought down in front, the broad ligaments tied and divided, and the uterus and vagina removed together. The peritoneum of the vesico-uterine pouch was sutured to that of Douglas's pouch, and gauze was packed lightly into the cavity below the peritoneal suture. The packing was changed on the third day and removed altogether on the fifth day. Cystitis appeared a few days after the operation, but was not severe, and the bladder soon recovered. The temperature rose above normal twice in the first fortnight after the operation. Convalescence was undisturbed otherwise except for some diarrhoea due to large doses of acid phosphate of soda. The patient left the hospital on September 22. The cavity between the rectum and urethra became obliterated. I saw her last on February 20, 1909, eighteen months after the operation. She was then quite well and had no pain or discomfort.

Sections of the ulcer show squamous-celled carcinoma. Sections of the adjacent portion of the cervix show no malignant infiltration. I

think that there is no doubt that this was a case of primary carcinoma of the vagina—a comparatively rare condition.

Howard Kelly describes four methods of operating on carcinoma of the vagina: (1) Simple excision of the carcinomatous area through the vaginal outlet. (2) Circular incision of the vagina below the diseased area, followed by a stripping off of the whole circumference of that portion which is to be extirpated; after this an abdominal incision, freeing the uterus and removing it with the upper part of the vagina. (3) A transverse incision through the perineum, extended up through the rectovaginal septum to the diseased area, which is then removed through the incision. This is the method adopted by Olshausen. (4) A posterior incision from sacrum to fourchette beside the rectum, splitting the vagina up to the diseased area.

Veit advises a trans-sacral operation. The vagina is isolated, the uterine arteries secured, and then the vagina is opened and the uterus removed.

Schröder separated the cervix from the vagina and then removed the whole vagina from above downwards.

I suppose that the whole vagina has been dissected out from below and removed together with the uterus before, but I have not found any record of it.

I consider that the operation which I performed in this case is better than any of those which I have enumerated, when the growth is fairly early. It is the only method by which the vagina can be converted into a closed bag, while by all the other methods there is risk of infecting raw surfaces with carcinoma cells. This point is of importance, as early recurrence is common. Preliminary scraping and cauterization cannot be performed satisfactorily in most cases of carcinoma of the vagina, as the growth cannot be exposed and fixed by volsellum forceps in the same way as can the cervix. In most cases the growth is high up on the posterior wall. The operation that I have described is not difficult, and can be performed in much less time than would be taken in removing the uterus and whole vagina from above.

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- HOWARD KELLY. "Operative Gynecology," Lond., 1898, i, p. 254.
OLSHAUSEN. *Centralbl. f. Gyn.*, Leipz., 1895, xix, p. 1.
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DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) had operated by Wertheim's method in a case of primary cancer of the vagina, and although the patient recovered, she died within about a year of recurrence. He thought there was some advantage in the operation performed by Dr. Andrews, especially as there would appear to be less risk of the vagina's tearing: this was very liable to occur in primary cancer of the vagina owing to its thinness and friability, and occurred in his case. He had seen at least three other cases of cancer of the vagina which he believed were primary. The risk of recurrence in these cases appeared to be very great, but Wertheim had had some cases of prolonged freedom from recurrence after the extended abdominal operation.

Dr. MCCANN said that he had operated on a patient with primary cancer of the vagina by a method which had not been mentioned by the author of the paper. A wide transverse incision was made through the anterior vaginal wall. The bladder and ureters were separated and the uterovesical pouch opened. The fundus and body of the uterus were rolled out by slow, gentle traction with a volsellum; the broad ligaments were ligatured from above downwards under visual control. The posterior vaginal wall was next divided on either side and the uterus and posterior vaginal wall pulled further outwards. The posterior vaginal wall was then divided transversely well below the growth from the peritoneal aspect, and was removed along with the uterus in one piece. The operation was performed on April 4, 1904, and the patient was examined and found to be free from recurrence in December of last year, 1908. The disease was a squamous carcinoma, and the specimen was exhibited at a meeting of the Obstetrical Society, and is described and figured in Dr. McCann's book on "Cancer of the Womb," p. 145. The disease was in an early stage and therefore favourable for operation; it is, however, important to remove the uterus as well as a portion of the vagina in primary vaginal cancer, for the frequent origin of the disease in the posterior wall of the vagina suggests the possibility that the uterine discharges may tend to keep up irritation or even play a part in the causation. The separation of a complete cuff of the vagina, including the disease, then closing this cuff by suture and removing it together with the uterus is the most scientific operation, and may be done from below; or a similar operation with clamping of the vagina below the growth may be carried out by the abdominal route. When the cancer originates in the lower segment of the vagina a free local excision is indicated. Dr. McCann had seen five examples of primary cancer of the vagina.

Dr. MACNAUGHTON-JONES said that in May, 1907, he had brought a case of primary carcinoma of the vagina in a woman aged 65 before the Gynecological Society. The growth was of a botryoidal character, and grew from the anterior and left side of the vagina, invading the vesico-vaginal septum and extending deeply into the tissue by the side of the bladder. The vulva, portio, and the rest of the vagina were healthy. It had only recently been discovered, and she was in an extremely cachectic state when she was operated

upon. The growth was widely removed down to the bladder, and the actual cautery was used. She died ten weeks after the operation from involvement of the bladder. A point of considerable interest in Dr. Russell Andrews's case was that he appeared to have satisfactorily proved that the disease occurred primarily in the vagina. Seeing the very few cases of primary carcinoma of the vagina recorded in British literature he had sought for some statistics abroad, and had at the time replies from Professor Jacob's clinic at Brussels, Fritsch's at Bonn, Krönig's at Freiburg, Bumm in Berlin (Halle), Rosthorn's at Heidelberg, Franz's at Jena, and Zweifel's at Leipzig. He had also heard from Professor Martin, then at Griefswald, who considered the disease of extreme rarity. From all these sources he had only been able to collect twenty-three instances of primary disease. During the thirteen years Professor Fritsch was at Bonn he had not seen a single case, but in Breslau he had had four cases. Including his work at Jena and Leipzig Professor Krönig could only recall four cases. Bumm, out of 433 cases, had only one case of primary occurrence. Out of 1,400 cases of cancer of the genitalia Zweifel had four. Pozzi also regards primary cancer as very rare. Roger Williams, who had gone carefully into the statistics of carcinoma of the vagina, came to the conclusion that 1 per cent. of all cancer in women is vaginal. This estimation came from the statistics of some 13,000 cases. The interesting point in his (Dr. Macnaughton-Jones's) case, as shown in the drawing which he handed round, was that it exemplified the two types of disease as described by Roger Williams—the tubular, or more chronic form, with actively proliferating columnar epithelium, and the nodular or squamous type, occurring in nests, in which the dissemination occurred more widely and spread more rapidly. George Noble, of Atlanta, had referred to the frequency with which primary carcinoma originated in the region of the urethra, as well as in the posterior wall, and this was what happened in his (Dr. Macnaughton-Jones's) case. All authorities were agreed as to the disheartening results of operation, even the most radical measures being usually unsuccessful in preventing recurrence. In deciding the point of primary infection it was absolutely necessary to exclude the portio and vulva. He congratulated Dr. Russell Andrews on the thoroughness of the measures he had adopted in so completely removing the disease, and its successful issue up to the present time.

Dr. LEWERS said he was surprised to find that some of those present considered this condition so very rare. He had himself seen, speaking from memory, at least fifteen or twenty cases of it. In none of these, however, was the disease at a sufficiently early stage as to induce him to attempt a radical operation, and he had been, therefore, obliged merely to scrape and cauterize the growth in such as had seemed to require some active treatment.

Dr. ANDREWS, in reply, said that this was the only case of primary carcinoma of the vagina that he had seen. From what he had read he had considered that the condition was rare. He was surprised to hear that some of the speakers considered that primary carcinoma of the vagina was fairly common. Dr. Macnaughton-Jones had shown that it was rarely seen on the Continent.

Obstetrical and Gynæcological Section.

May 13, 1909.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

The Operative Treatment of Rupture of the Uterus, with an account of three cases treated by Abdominal Hysterectomy.

By THOMAS WATTS EDEN, M.D.

I DO not propose, in this paper, to consider either the causation or prevention of obstetric rupture of the uterus, but only the treatment of this accident. It appears to me that the subject is too wide to be profitably discussed in all its bearings on a single occasion. I accept the current classification of the varieties of rupture as *complete*—*i.e.*, opening up the peritoneal cavity—and *incomplete*—*i.e.*, not involving the peritoneum; and I also recognize that in the majority of cases the injury to the uterus is accompanied by colporrhæxis—*i.e.*, laceration of the vaginal vault.

The last occasion upon which the treatment of rupture of the uterus was discussed in this Society was in January, 1900, when the subject was introduced by our present President, Dr. H. R. Spencer. It will be convenient to begin with a brief résumé of the general course of the discussion upon that occasion. Dr. Spencer related four cases of rupture of the uterus which he had treated by packing the rent with iodoform gauze; all recovered, and the author stated that they were the only cases which he had ever known to recover from this accident. Two were cases of incomplete rupture of only moderate severity, the description being in each that "the half hand could be passed into the broad ligament"; the other two were cases of complete rupture of the most severe and dangerous kind, accompanied with free intraperitoneal

bleeding. They were treated by first evacuating the effused blood from the peritoneal cavity *per vaginam*, clots being removed with the hand passed through the tear, and the remainder expressed by abdominal manipulation and pressure through a tube passed through the rent in the same way; finally, the rent was packed *per vaginam* with iodoform gauze. Speaking to the general subject, Dr. Spencer recommended that all cases of incomplete, and most cases of complete, rupture of the uterus should be treated by packing; he considered hysterectomy hardly ever necessary, and, when required, preferred the vaginal to the abdominal method. In some cases, which he did not define, he thought abdominal section with suture of the rent and cleansing of the peritoneum might be carried out. Subsequent speakers were in general agreement with Dr. Spencer; all condemned the removal of the uterus except in certain undefined circumstances, and packing or drainage was preferred. None but Dr. Spencer appeared to have practised evacuation of the effused blood through the rent in the manner described. This discussion may, I suppose, be accepted as representing the current opinion among obstetric teachers in London at that time.

A year after this discussion (in 1901) Varnier, of the Clinique Baudelocque in Paris, read a paper on the same subject at the French Congress of Gynæcology and Obstetrics. He related twenty-three cases of rupture of the uterus which had come under his notice between 1885 and 1901. From 1885 to 1897 eleven cases occurred, all of which were treated by what he called obstetrical methods—*i.e.*, packing, douching, &c.; ten of these cases were fatal. This appalling loss of life convinced Varnier that the treatment was wrong, and he decided forthwith to abandon it and treat all cases which he might meet with in future by immediate surgical intervention. Between 1897 and 1901 twelve further cases occurred. He explained that the obstetrical clinics in Paris were not at that time fitted up for abdominal operations, and consequently, when a case of rupture came in, a good deal of delay often occurred before the necessary preparations for operation could be made. In this way six of the twelve cases died before an operation could be carried out; in the other six cases abdominal hysterectomy was performed as speedily as possible, and of these cases three recovered. With this result Varnier was more than content. Generally speaking, he insisted upon the necessity of an immediate abdominal section for exploratory purposes in all severe cases of uterine rupture, whether complete or incomplete. He questioned the practical utility of the orthodox division into complete and incomplete rupture, and remarked

upon the difficulty, in many cases, of being sure whether the hand, when passed *per vaginam*, was in the peritoneal cavity or not; he further insisted upon the grave risks attending extensive subperitoneal lacerations. He favoured removal of the uterus rather than suture, firstly as a precaution against sepsis, and secondly because he believed that a ruptured uterus which healed up could never be trusted in a subsequent pregnancy. He says: "When a woman who, in attempting to reproduce the species, has ruptured her uterus and escaped with her life, she should be invalidated from the service; for it is not with cripples that an army takes the field." In this connexion he mentioned fifteen cases of rupture of the uterus which had recovered, and in which pregnancy subsequently occurred. In five of them rupture again took place, and three proved fatal.

There is no doubt that upon this question Varnier was in advance of his day; as far as I can find, he was the first obstetric teacher of repute who boldly advocated that surgical intervention should be freely resorted to in the treatment of uterine rupture.

I well remember the comfortable impression made upon my mind by Dr. Spencer's experience, and in consequence of it I remained prepossessed in favour of packing until a case of ruptured uterus came under my own notice which convinced me that it was useless to attempt to treat cases of severe rupture by this method. This case was the following:—

CASE I.

M. S., aged 29. First labour normal; second labour, which was at term, commenced at 2 a.m. on January 22, 1905, and the doctor was first called to the case at 2 p.m. Being unable to diagnose the presentation, he obtained the assistance of a colleague, who came at 4 p.m. and found the uterus acting powerfully, the os the size of a crown piece, the membranes unruptured, the presentation transverse with the head on the right side and a limb presenting in the bag of waters. Being able to reach the sacral promontory with ease, he also thought there was slight pelvic contraction. Subsequent measurement, however, showed that the pelvis was normal. Nothing was done at the time and labour was allowed to proceed till 7 p.m., when he found the os fully dilated, the membranes ruptured, and an arm prolapsed into the vagina. At 7.45 p.m. an anæsthetic was given with the object of performing internal version. On pushing up the presenting shoulder *per vaginam* hæmorrhage began, and it was thought that part

of the placenta had become detached; no difficulty was, however, experienced in introducing the hand, or in turning the child, but the operation was hurried somewhat on account of the rather free bleeding. There was a little difficulty in delivering the head and arms, which had become extended. After delivery (8 p.m.) the hæmorrhage ceased and the uterus retracted well. The child was dead. For an hour or more the placenta did not appear, though the patient seemed well; there was no external bleeding and the pulse was under 100. After several attempts had been made to express it, she became pale, restless, and complained of abdominal pain; although the uterus was well retracted, all attempts to express the placenta failed. I was then sent for and saw the patient at 10 p.m.—*i.e.*, two hours after delivery.

At that time the pulse was 120, temperature 96.2° F. (mouth). The uterus, completely retracted and obviously empty, could be felt on the right side of the abdomen at the usual level; the left flank was dull on percussion, but there was little abdominal tenderness. *Per vaginam* posteriorly on the left side of the vaginal wall in its upper one-third a deep tear was felt which extended into the cervix, and through this rent the umbilical cord passed. It was clear that the placenta had escaped from the uterus, and a little chloroform was accordingly given to allow of a complete exploration. The fingers were then passed into the uterine cavity, finding it empty; on following up the cord the rent could be traced high up the lateral wall of the uterus. The placenta, together with a good deal of blood-clot, was found lying among coils of intestine. With the aid of gentle traction on the cord the placenta was delivered through the rent; a great deal of blood-clot came away with it. A long glass tube was passed through the rent and the peritoneal cavity flushed with several pints of warm weak lysol solution; no boiled water being available. An attempt was then made to plug the rent with iodoform gauze, but, owing to the absence of any resistance, the gauze simply passed into the peritoneal cavity without exerting compression upon the sides of the tear. By this time the patient's condition was bad—pulse 140 to 150—but the uterus remained well retracted. She was therefore wrapped up in hot blankets; one pint of saline was administered by the rectum; strychnine was given hypodermically; and she was then removed by ambulance, with as little delay as possible, to Charing Cross Hospital. On admission her temperature was 97° F., pulse 128, respiration 32, and a considerable improvement in her condition had taken place. During the night, however, the pulse-rate increased somewhat, although there was no sign of leakage of blood from the vagina; saline

solution was twice transfused into the median basilic vein, and her general condition remained fairly good. I saw her again at 9 a.m. on the following morning; her pulse was then 135, temperature 96.4° F. The uterus was well retracted, there was a large area of impaired resonance on the left side, which shifted slowly on change of posture, and within the last half-hour a distinct ooze of recent blood had occurred through the vaginal packing. It was clear that hæmorrhage had recurred, so I decided at once to open the abdomen as the quickest means of establishing control of the bleeding.

The operation took place fourteen hours after delivery. On drawing the uterus out of the abdominal incision a great deal of free blood was found in the peritoneal cavity, and a linear rent about 3 in. long was first seen in the anterior wall of the broad ligament; in the posterior wall was another wide, deep tear, and a good deal of blood-clot lay between the layers of the broad ligament. Through this rent a finger was passed, and the presence of an extensive laceration in the postero-lateral wall of the uterus detected, which ran from near the insertion of the Fallopian tube along the whole length of the uterine body and cervix, and into the left lateral vaginal wall. It was clearly through the large rent in the posterior wall of the broad ligament that the placenta had passed into the peritoneal cavity. It was also obvious that such an extensive injury could not be repaired, and it was at once decided that the uterus must be removed. The gauze was lying loosely between the retracted widely-gaping edges of the uterine rent, and exerting no compression whatever upon the bleeding points. Several venous sinuses in the torn wall were oozing freely, but there were no arteries bleeding. The uterus was removed by supravaginal amputation, and after the abdomen had been closed the rents in the portio vaginalis and left vaginal wall were plugged *per vaginam* so as to arrest any possible oozing from these surfaces. The patient's condition was very bad after the operation, and for nearly twenty-four hours there was little improvement. She then began to rally and ultimately made an uninterrupted recovery.

The probable course of events in this case was that an incomplete rupture into the left broad ligament occurred during version, which, though easy, was accompanied by rather free bleeding. The patient's condition was not at first alarming, but subsequently the rent was made complete by unsuccessful attempts to express the placenta. Free intraperitoneal bleeding then took place, and the patient's condition became grave.

The operation afforded the clearest possible demonstration of the

futility of attempting to arrest hæmorrhage from a complete laceration of the uterus by packing it from below with gauze. The gauze simply passed through the tear into the peritoneal cavity, leaving the bleeding points on the separated and retracted edges absolutely uncontrolled. Naturally, packing cannot control bleeding unless some counter-pressure on the part of the tissues can be obtained, and in complete rupture this is impracticable.

Another lesson, though it is an old one, which I felt this case enforced was that it is never too late to operate for internal hæmorrhage. Although hæmorrhage had been going on more or less for fourteen hours, and venous transfusion had been three times practised, yet effective control of the bleeding was successful in saving her life. Since this case I have preferred abdominal hysterectomy for rupture in two other cases which I will now mention.

CASE II.

S. S., aged 33, was admitted in labour to Queen Charlotte's Hospital on April 10, 1908. She had had slight pains for about twelve hours. Her previous obstetric history had been somewhat chequered. The first and second confinements terminated naturally; in 1903 she underwent the operation of ventrofixation for prolapse, and in 1905 she had her third labour; it was prolonged, and was terminated with forceps, but presented no other difficulty, and the child survived; in January, 1907, she was operated upon for ruptured tubal gestation on the right side, and in the following July became pregnant for the fifth time. On admission the abdomen was pendulous, and there was a moderate degree of pelvic contraction, the true conjugate being estimated at over 3'5 in., the contraction being due to flattening. The head was not engaged in the brim, but resting upon the sacral promontory. The cervix was unusually high up, fully dilated; but, as the membranes were unruptured and as the pains had ceased for a time, no interference was practised. After waiting for twenty-four hours, some feeble pains occurred, and upon my advice the resident medical officer then ruptured the membranes and delivered the child by version. The operation was difficult, and both arms, as well as the head, became extended; it was delivered alive, but the heart was very feeble, and all attempts to start respiration failed. It weighed 9 lb. 3½ oz.

After waiting an hour for the placenta the resident medical officer introduced his hand, and found a large tear in the upper part of the vagina and cervix through which the cord passed. Following the cord,

the finger passed into the peritoneal cavity, and the placenta was withdrawn along with a good deal of blood-clot. The patient's condition was not alarming, but there was a good deal of shock and her pulse-rate was 120. At about 2 p.m., an hour after the delivery of the placenta, I saw her, and under anæsthesia made out the presence of a large rent beginning in the right postero-lateral vaginal wall, involving the cervix and passing directly into the peritoneal cavity. There was no prolapse of intestine; the uterus was well retracted and its cavity empty. I decided to open the abdomen immediately, and as preparations for the operation had been already made it was begun forthwith.

On opening the peritoneal cavity we first encountered a strong band of adhesion between the upper part of the anterior uterine wall, just below the fundus, and the abdominal parietes. This was divided and the uterus drawn out. Another extensive area of adhesion was found low down on the right side in the position of the right appendages, and no doubt attributable to the previous operation for extra-uterine gestation. There was not a large amount of free blood in the peritoneal cavity. An extensive laceration, about 5 in. in length, was then found in the posterior layer of the left broad ligament. Passing the finger through this laceration, I found a ragged tear of similar extent beginning in the lower part of the body of the uterus and extending through the cervix down into the vaginal wall. The uterus was now rapidly removed by supravaginal amputation by clamping and dividing the broad ligaments, the peritoneum was partly closed over the stump, and a gauze drain passed through the torn side of the cervix into the vagina. Venous transfusion was practised during the operation, and, in addition, two pints of saline were poured into the peritoneal cavity before the abdominal wound was closed. At the end of the operation the patient's condition was much worse than at its commencement, the pulse being very rapid and difficult to count. She passed a bad night, and, though better by the next morning, her pulse-rate was still 140. Within twenty-four hours, however, she began to improve; the following day her pulse-rate had fallen to 116, and thereafter she made a good recovery, the abdominal wound healing by first intention. The gauze drain was removed *per vaginam* on the fourth day.

CASE III.

E. W., aged 25. Her first labour (twins) was normal, and she was attended by a midwife. Her second labour began on the night of January 17, 1909. Throughout January 18 she had some irregular pains

at intervals, and at 8 p.m. she was visited at her house by the resident obstetrical officer of Charing Cross Hospital, who noted that the presentation was a vertex, the head above the brim, the os about the size of half-a-crown, and the membranes intact. The foetal heart was heard to the left of the middle line. On the morning of January 19 the head had come down into the brim, the os was nearly fully dilated, and the membranes were still unruptured, but the pains were feeble and irregular. At about 4 p.m. the membranes ruptured, and at 5.30 strong and regular pains set in, and the student in charge found the head descending well into the pelvic cavity. Strong labour pains continued for about three hours, when the patient became sick, and for a quarter of an hour she was vomiting and retching violently. Immediately after this she became pale and exhausted, the pulse-rate ran up to 100; some large clots escaped from the vagina, and on examination the student was now unable to feel the child's head at all, although the last time he examined he thought it was nearly down to the perineum. He accordingly sent for the resident obstetrical officer, who arrived at 9.45 p.m. He found the patient collapsed, pulse-rate 110; the abdomen was rigid and tender; on examination several clots were found in the vagina, but the head could not be felt.

I saw the patient myself about 10.15 p.m. Her pulse-rate was then 140. The abdominal wall was rigid and tender, rendering palpation difficult. On the right side was a softish mass extending well above the umbilicus, which I took to be the body of the uterus; the foetus lay to the left of this, with the head below. The limbs could not be felt; the trunk was immovable, and could not be separated from the uterus. On introducing the hand into the vagina it passed into the empty uterine cavity lying to the right; the foetal head lay in what appeared to be an extension of the uterine cavity to the left. The fingers passed easily round and above it, and, as no coils of intestine or slippery peritoneal surfaces could be felt, it was concluded that the foetus had passed into the left broad ligament, the rupture being extraperitoneal. The feet lay just above the head, and the child was delivered by podalic version, a little difficulty being experienced in extracting the head through the brim, as the pelvis was flat, the conjugate measuring $3\frac{1}{2}$ in. It was dead, and weighed $7\frac{1}{2}$ lb. The placenta was found still attached to the uterus, and was removed. On reintroducing the hand it passed through a large rent in the left antero-lateral part of the vagina and cervix into the peritoneal cavity, and a good deal of blood-clot was felt among the coils of intestine. There was not much external hæmorrhage.

After delivery the patient's condition improved a good deal, and arrangements were made for transferring her to the hospital, but owing to delay in obtaining an ambulance it was about an hour and a half later when she was admitted. Her condition was then worse; there was extreme pallor, the pulse-rate was 140, and the whole of the left side of the abdomen was dull on percussion. I thought there was no doubt that fresh internal bleeding had occurred while she was being moved, and I therefore decided to open the abdomen at once (three hours after delivery).

On pulling the uterus out of the abdomen the injury was seen to be very extensive. The left broad ligament was torn transversely on its anterior aspect from the uterine border right out to the pelvic wall, so as to involve the infundibulo-pelvic fold; the ovarian artery was torn, and had retracted beneath the peritoneum on the pelvic brim, but was not bleeding freely. An extensive subperitoneal hæmorrhage had formed anteriorly between the uterus and the bladder, crossing the middle line over to the right side. Posteriorly there was no subperitoneal bleeding, but there was about a pint of free blood in the peritoneal cavity. The uterus was removed as rapidly as possible by supravaginal amputation, some difficulty being found in securing the retracted ovarian artery. The right ovary was not removed.

After amputating the uterus no further bleeding points could be seen; the peritoneum was sewn partially over, drainage with gauze being provided for into the vagina; venous transfusion was made during the operation, and the abdomen was afterwards filled with warm saline solution, the patient's condition at the close being very serious and the pulse almost uncountable. In two or three hours, however, her condition had improved greatly, the pulse-rate had fallen to 110, and she dozed until early morning. About 6 a.m. she became restless, and the pulse grew more rapid and feeble. Strychnine was given hypodermically, and two pints of saline injected into the rectum. She improved again, but two hours later complained of severe pain in the chest and died rather suddenly.

The autopsy showed the cause of death to be recurrent hæmorrhage. Several ounces of deeply blood-stained fluid were found in the peritoneal cavity. A good deal of subperitoneal hæmorrhage was found anteriorly and to the left side, and the source of this bleeding was traced to a deep laceration of the posterior wall of the bladder extending through the muscular coat, but not including the mucosa; this injury had been overlooked at the operation.

I think that I am myself to some extent to blame for the unfortunate termination of this case. If I had discovered and sewed up the tear in the bladder, I see no reason whatever why this patient should not have recovered; or even if I had packed the laceration *per vaginam* after closing the abdominal wound, as in my first case, the hæmorrhage from the torn bladder might have been controlled. But a small amount of recurrent bleeding is of course enough to turn the scale in cases as serious as this. She rallied surprisingly well from the operation, and had no further hæmorrhage occurred I think she had quite as good a chance of recovery as the other two cases.

All three of these cases present points of interest in regard to the causation of the rupture, but to these points I do not refer, as this paper is intended to deal only with treatment. Cases I and II were obviously examples of what the Germans call "violent" rupture. Case III was an example of spontaneous rupture towards the end of the second stage of labour. The uterine tissue is being carefully examined, and I hope at some other time to present a report to the Society upon this point.

In considering the general question of the operative treatment of rupture of the uterus, it will be convenient first to review briefly the nature of the risks associated with this accident. The risks are three in number—viz., shock, hæmorrhage, and sepsis.

The amount of shock produced by an extensive laceration, whether complete or incomplete, is always severe, and when associated with free bleeding it becomes profound, and in my experience is only equalled in cases of intraperitoneal flooding from ruptured extra-uterine gestation. But I do not believe that in these cases shock of itself is fatal; it is true that death from rupture may occur in a few hours, but in such cases hæmorrhage, not shock, is the immediate cause of death.

The arterial spasm and depression of circulation produced by shock are probably of service in temporarily arresting bleeding from torn vessels. An improvement in the general condition of the patient due to the subsidence of shock may then give place to relapse from recurrent bleeding, and this improvement, followed by deterioration of the general condition, is an important indication of the recurrence of internal bleeding. I noticed it in two of my cases, and found it an important aid to the diagnosis of internal hæmorrhage.

Hæmorrhage and sepsis are, however, to be regarded as much more formidable risks than shock. The frequency with which serious bleeding

occurs in rupture of the uterus has, I believe, been a good deal underestimated. It is difficult to obtain precise statistical information upon this point, because in so many recorded cases that recover mention of the amount of hæmorrhage is not made. But a good deal of information is now at hand with regard to the proportion of fatal cases due to hæmorrhage, and of operated cases in which severe bleeding was found.

A word or two must be said upon the sources from which the figures which follow have been obtained. In 1901 Klien, of Dresden, published a critical article on uterine rupture of a most elaborate and painstaking character; he succeeded in collating clinical reports of 381 cases from medical literature, most of which were, of course, isolated instances published by individual observers. These he grouped and classified from the point of view of causation, variety, symptoms, treatment, &c., and drew from his study a number of conclusions upon various points. With regard to treatment, it may be said in brief that he considered the mortality of operative interference too high to justify its extended application, while in regard to conservative treatment he totally condemned packing, and strongly recommended vaginal drainage with large rubber tubes passed through the rent. This paper excited considerable attention on the Continent, and in 1903 it was severely criticised by Kolomenkin, of Moscow; from a reconsideration of Klien's cases this writer drew conclusions quite opposed to those of Klien—viz., that operative treatment was much more favourable than conservative treatment. Again, in 1905, Eversmann, of Bonn, subjected Klien's cases to further review, and conclusively showed that packing, which Klien entirely condemned, was far more successful than any other method when properly applied—*i.e.*, in the particular manner recommended by Eversmann.

These circumstances make the onlooker rather shy of Klien's cases and percentages. Kolomenkin advanced a view which I think is quite sound: that, in considering the question of treatment, large numbers of isolated cases treated indiscriminately by skilled and unskilled persons, and amid surroundings favourable and unfavourable, are unreliable; only cases in series from lying-in institutions, treated by men of repute and under favourable conditions, can really be relied upon. Fortunately we now have a considerable number of these series of cases, and I shall endeavour to make use of them exclusively in considering such points as percentages of mortality and recovery.

To return now to the question of the frequency of severe bleeding in rupture of the uterus. Ivanoff in a series of 124 cases occurring

in the Moscow Maternity found that in fifty-three cases—*i.e.*, 42·8 per cent. — hæmorrhage proved fatal. Draghiesco and Cristéanu have reported seventy-seven cases from the Maternity of Bucharest in which 24 per cent. of the mortality was due to hæmorrhage alone. In Munro Kerr's fourteen personal cases there was serious hæmorrhage in three—*i.e.*, 21·4 per cent.—and in Kolomenkin's five cases operated on recently at the Alt-Katharinenspital in Moscow, in four—*i.e.*, 88 per cent.—a large amount of internal bleeding was found. In my own experience of ten cases (not all under my own care) there was severe hæmorrhage in four—*i.e.*, 40 per cent. The figures of mortality quoted of course do not include cases in which there was considerable but not fatal hæmorrhage, and in which death subsequently occurred from sepsis plus the constitutional effects of hæmorrhage; nor do they include cases which recovered and in which considerable bleeding may have taken place.

I think it is not unfair to conclude that dangerous hæmorrhage occurs in over 40 per cent. of cases of severe rupture of the uterus, and in this connexion it must be borne in mind that a degree of hæmorrhage which alone would not imperil life when superadded to severe shock may well prove fatal.

The risks of hæmorrhage, though greater in complete than in incomplete rupture, are common to both varieties. Serious external bleeding seldom occurs in complete rupture, but is not uncommon in the incomplete variety; but the real risk in both is internal bleeding. When the foetus or placenta has escaped from the uterus, a considerable amount of blood is commonly found in the peritoneal cavity, and most of this no doubt comes from the placental site, and is soon controlled by uterine retraction, which is usually efficient in these cases. But one is struck, in reading accounts of laparotomy for uterine rupture, by the frequency with which the operator has encountered spurting vessels or free oozing from the lacerated tissues; in two of my three cases active bleeding of this kind was going on. The only writer I have found to dispute this view of the hæmorrhage is Eversmann, who attributes it entirely to atony of the uterus, arguing that if the uterus is properly retracted hæmorrhage can no more occur from a laceration than from the placental site. When operators find active bleeding going on, he attributes this to relaxation of the uterus under the influence of the anæsthetic. This writer has apparently forgotten that in Cæsarean section a degree of retraction which is sufficient to arrest bleeding from the placental site will not control divided vessels in the incision; it is

the suture of the Cæsarean wound which arrests bleeding, not uterine retraction, and direct control of hæmorrhage is almost as necessary in rupture as in Cæsarean section. Eversmann also ignores entirely the possibility of bleeding coming from extra-uterine vessels. It is practically the universal experience that after delivery, or after escape of the foetus into the peritoneal cavity, a ruptured uterus retracts efficiently, and I think that Eversmann's view may be dismissed from further consideration.

In incomplete rupture the internal bleeding is of course sub-peritoneal. Many cases of this kind are on record. Klien mentions thirty such cases in his series, and 70 per cent. of them proved fatal; in one of these the blood had tracked up to the kidney. Mathieson, in 1889, recorded a case of rupture in which death occurred on the fourteenth day, and at the autopsy a retroperitoneal hæmorrhage extending from the brim of the pelvis to the diaphragm was found. In my third case a considerable subperitoneal hæmorrhage formed in front of the uterus in two or three hours.

The bleeding which immediately follows the rupture usually ceases spontaneously, as Munro Kerr has recently pointed out, although Varnier has recorded a case which terminated fatally in fifteen minutes, and at the autopsy the tear was found to have involved the placental site. But if the patient survives the primary hæmorrhage, there remains the risk of recurrence when the shock produced by the injury has passed off and the force of the circulation is regained. Clearly the risks of recurrent bleeding will also be increased by manipulations practised for delivering the child or placenta *per vaginam* or simply for exploration, and also by the necessary transportation of the patient from her home to the hospital. This point is well illustrated by Ivanoff's figures, for he shows that the great majority of deaths from hæmorrhage occurred not immediately but within from two to twelve hours after rupture—*i.e.*, after repeated hæmorrhages or continuous oozing had occurred.

From these considerations two points become clear: (1) that the necessity of establishing control of hæmorrhage from the laceration must be encountered in a large proportion of cases of rupture of the uterus; and (2) the severe shock which is always present complicates the situation by rendering the diagnosis of internal hæmorrhage obscure, and also by greatly increasing the gravity of operative interference undertaken for controlling it.

There remains the third risk of *sepsis*, and about this only a word or two need be said. In all probability most of the cases which come

under our notice in hospital practice have been already infected. Almost invariably the labour has been prolonged and difficult, and repeated attempts, sometimes unsuccessful, have often been made to deliver by some obstetric method. But, if not actually infected at the time, the presence of an extensive laceration, opening up direct communication between the vagina on the one side and the peritoneal cavity or the pelvic cellular tissue on the other hand, renders a lying-in woman's position one of the gravest possible danger. Many deaths occur from acute uterine septicæmia, many from acute peritonitis, alike in complete and incomplete rupture. It is highly probable that infection, even of a mild type, would prove fatal to a woman whose resistances have been reduced already by shock and hæmorrhage following upon a prolonged labour.

The proportion of cases which succumb to septic infection is variously estimated from 37 per cent. by Kolomenkin to 74 per cent. by Draghiesco and Cristéanu; probably we may with safety say that 50 per cent. of the total mortality is due to this cause.

TREATMENT.

The treatment of uterine rupture must then be governed by the necessity of meeting the immediate risks of shock and hæmorrhage, and the subsequent risk of sepsis. An injury so severe as this must lead to a high mortality, no matter what line of treatment is adopted; the general mortality of cases treated in hospital is estimated by Ivanoff at 81.75 per cent., and by Draghiesco and Cristéanu at 70 per cent., by Lobenstein at 73 per cent., and by von Walla at 64 per cent. Munro Kerr has recently expressed the opinion that it is not more than 50 per cent. to 60 per cent., but this is probably too low. It appears that over large numbers of cases there is not much difference in mortality between complete and incomplete rupture, the figures being 5 per cent. to 10 per cent. higher in the case of the former. It must be remembered that many deaths occur within a few minutes after rupture or delivery; two cases of this kind have occurred at Queen Charlotte's within the last ten years, and many are on record in medical literature. In such cases it is probable that nothing could have averted a fatal issue. Statistics of operative results must accordingly be judged in the light of the grave risks inseparable from the accident.

Expectant or Non-operative Treatment has been shown by experience to be attended by a high mortality when applied to cases of all degrees

of severity. Munro Kerr estimates the mortality attending this method at 90 per cent., Lobenstein at 92 per cent., Kolomenkin at 61 per cent., von Walla at 60 per cent., and Varnier, in 1901, reported eleven cases with ten deaths. The truth appears to be that while suitable and successful in cases of slight or moderate severity, it is quite unsuitable for severe cases, whether complete or incomplete. When only the lower part of the broad ligament has been laid open by a laceration of the vaginal vault and cervix or lower segment large enough to admit the fingers, conservative treatment will usually meet the requirements of the case. I regard two of the cases recorded by the President in 1900 as typical of this class, and I quite agree that if the cavity is douched with an antiseptic and then packed with gauze, or even simply drained with a large rubber tube, the patient has an excellent chance of recovery. Unless there is progressive bleeding I think drainage only should be preferred, as tight packing, by retaining discharges, may favour the occurrence of sepsis. In severe cases of rupture it appears to me that expectant treatment is quite unsuitable, and cannot be expected to yield good results.

It must be remembered with regard to plugging with gauze that nothing like the favourable results obtained by Dr. Spencer and other speakers at the 1900 discussion have been generally met with. Klien has collated sixty-five cases treated in this way, and 52 per cent. of them died; and, further, the results were very nearly as bad in incomplete as in complete rupture (50 per cent. and 56 per cent. respectively). The failure of this method when employed to control bleeding is shown by the fact that in twenty-two cases (some complete, others incomplete) of severe bleeding thus treated fifteen—i.e., 68 per cent.—died. So deeply was Klien impressed with the unfavourable results of this method that, with a vehemence rare in the scientific Teutonic mind, he exclaims "*Also! fort mit der Gaze aus der Behandlung der Uterus-ruptur!*" This opinion is by no means unsupported by others, for more recently Lobenstein has recorded fourteen cases of rupture treated by plugging in the Lying-in Hospital of New York with a mortality of 92 per cent. Draghiesco and Cristéanu recorded six cases, five of which proved fatal (83 per cent.).

For all practical purposes an extensive subperitoneal laceration, opening up the entire broad ligament and large enough, say, to admit the whole hand, is quite as formidable as a laceration laying open the peritoneal cavity; it may be accompanied by dangerous hæmorrhage, both external and concealed, or subperitoneal, and in addition the risks

attending septic infection of pelvic cellular tissue in these cases are little less than of the peritoneum, for autopsy has shown that death from acute peritonitis after incomplete rupture is by no means uncommon. To these considerations must be added the further one that great difficulty often exists in deciding whether a large laceration has opened the peritoneum or not—a difficulty which has been encountered and remarked upon by a number of observers. These reasons, I think, justify us in grouping all extensive lacerations together, whether they are complete or incomplete. In such cases I believe that abdominal section offers the patient the best chance of recovery, for the immediate necessity is to expose fully the site of the injury, in order to control bleeding efficiently. It is immaterial, from the point of view of treatment, whether the foetus has escaped into the peritoneal cavity or not; in this case the necessity of opening the abdomen is obvious, but in my belief it is really the existence of a tear large enough to permit the escape of the foetus which necessitates abdominal section. In these cases I regard expectant treatment, such as plugging the rent with gauze *per vaginam*, as being absolutely unreliable. It cannot possibly control the bleeding efficiently in complete rupture. When the rupture is incomplete it *may* do so in some cases, but will certainly fail if laceration and retraction of vessels of considerable size has occurred; and, further, if tight packing is employed, it must be borne in mind that this will retard instead of promoting that free drainage which is absolutely essential as a precaution against sepsis. Loose packing will not control bleeding, tight packing favours sepsis, and it must be further borne in mind that to leave a large open communication between the vagina and the peritoneum during the puerperium is to invite infection through the lochia, even if this has not already occurred.

The only other possible alternative to abdominal section is a vaginal operation either for suture of the rent or for removal of the uterus. It is somewhat remarkable that vaginal hysterectomy has been little practised in cases of uterine rupture. Kolomenkin mentions three cases with one death, Klien seven cases with four deaths. While there is much to recommend it on account of its simplicity and the slight amount of shock which it produces, the great objection to this operation is that it may fail to control bleeding. It would be quite possible to remove the uterus by this method without reaching or controlling all the bleeding vessels; this would certainly have occurred in my third case, in which the ovarian artery had retracted above the pelvic brim, and, as I have already mentioned, this case is by no means unique. Thus, in Klien's

seven cases of vaginal hysterectomy one death occurred from hæmorrhage uncontrolled by the operation and three from "collapse." I can readily admit, however, that in some cases where the extent of the injury is not very great, and there is no evidence of deep bleeding, vaginal hysterectomy may be resorted to with success.

In the majority of cases of severe rupture, however, I believe that laparotomy offers us the best chance of success. It will, in most instances, be very difficult to exclude deep bleeding owing to the profound shock from which the patient is suffering. Steady deterioration of the pulse and of the general condition, or relapse after a temporary improvement, are the only signs of continued hæmorrhage which are likely to be found. And the difficulty is that the worse the patient, the more urgent is the necessity for fully exposing the site of the injury. Opening the abdomen and securing bleeding points of itself will involve little shock, and only the lightest general anæsthesia is required; indeed, it could be readily done under a local anæsthetic alone. And having got thus far, the operator has three courses open to him: (1) Simply to pack the rent from above and drain by both the upper and lower routes; (2) to remove the uterus and freely drain the pelvis; (3) to suture the rent and freely drain the pelvis.

(1) *Abdominal packing and drainage* by both the upper and lower route is a method of treatment of which I have no personal experience, and which has not been at all widely tried. But a few individual cases have been recorded, and it appears reasonable to think that it may be the best thing to do under certain conditions. Thus if the patient is *in extremis*, and every moment's delay prejudicial to her recovery, a few minutes would suffice to open the abdomen, secure bleeding points, clear the peritoneal cavity, pack the tear with gauze carried down into the vagina, and close the abdomen with a suprapubic drain. In many cases of extensive laceration I do not think bleeding can be properly controlled without removing the uterus, but in others it might be quite practicable.

My second case, for example, might very well have been handled in this manner and, I think, with greater prospect of success. The retention of the uterus may involve risks of sepsis, but under conditions of great immediate urgency this risk may be unavoidable. If the patient rallies, vaginal hysterectomy could be very simply and readily performed twenty-four or forty-eight hours' later.

(2) *Hysterectomy* has the immediate advantages of fully controlling hæmorrhage and of removing a frequent source of septic infection;

these points have been already sufficiently considered. A later and subsidiary advantage is that, in the event of recovery, it obviates the serious risks of repetition of the rupture in a subsequent pregnancy or labour. This point requires further mention.

A good deal of evidence has accumulated during the last few years showing that in cases which recover there is a greatly increased liability to the recurrence of rupture in a subsequent labour. Varnier has traced fifteen cases in which a second rupture occurred in this way, and five of them proved fatal. Cristéanu has recorded a case in which rupture occurred three times, the last proving fatal. Kolomenkin has recorded one, Peham three, Patz one. In the case of Patz it is interesting to note that the first laceration occurred on the anterior, the second on the posterior wall, so that it is not always through the scar that the second rupture occurs. In addition to these, a considerable number of cases of rupture through a Cæsarean section scar have been recorded, but the tendency of the Cæsarean scar to give way is not so great as that of the scar of a healed uterine rupture. The latter compares unfavourably because it is a ragged tear, not a clean-cut incision, and because it is situated mainly in the lower segment which stretches during labour, not in the upper segment which contracts. It is accordingly weaker, and is also subjected to greater strain.

This tendency to repeated rupture is now pretty generally recognized, and a recent British writer, Munro Kerr, goes so far as to advise that, in cases successfully treated by plugging, the uterus should be removed as a precaution at a subsequent operation.

It appears, therefore, that there are sound reasons for removing the uterus; but an important question remains—viz., is the condition of the patient such as to offer a reasonable chance of her recovery. At the 1900 discussion the opinion was freely expressed that abdominal hysterectomy must be of necessity fatal. It is a curious circumstance which may partly explain the general pessimism that, although seventeen cases of ruptured uterus were recorded in the *Transactions of the Obstetrical Society* previous to that date, only one of them recovered—a case recorded in 1878 by Dr. Hickinbotham, of the Hospital for Women, Birmingham. Two only of the cases were treated by laparotomy. In the first, recorded by Swayne, of Bristol, in 1886, suture of the rent was practised; in the second, recorded by Dr. John Phillips, in 1897, the uterus was removed by subtotal hysterectomy.

It must now be admitted that these apprehensions have been to a great extent dissipated. The operation has been performed in a large

number of cases, and we are in a position to determine with a fair amount of accuracy what is the mortality which attends it.

For statistical purposes the most valuable data are those collated by Kolomenkin in 1903. Resisting the temptation to aggregate a large number of isolated cases from various sources, treated under both favourable and unfavourable conditions, he has obtained 140 comparatively recent cases of uterine rupture occurring in series in lying-in institutions, and treated in all cases by men of repute. Of these 140 cases, ninety-seven were treated conservatively—*i.e.*, without operation—and of these the mortality was 61 per cent.; in thirty-three cases the uterus was removed, with a mortality of 36·3 per cent. The larger number of cases obtained indiscriminately from all sources by Klien in 1901 showed sixty-three cases of complete rupture treated by abdominal hysterectomy, with a mortality of 47·6 per cent. But he pointed out that if only cases occurring since 1890 were considered, the mortality of the operation would fall to 37·5 per cent., a figure corresponding closely to Kolomenkin's. Individual instances of recovery from the operation have been frequently recorded during the last few years, but these are of no use for statistical purposes, as we do not know how many failures there may have been in addition. Several short series of cases have been recorded by various writers—Kolomenkin's five cases from Moscow all recovered. Of von Walla's five cases, three died; but one of these was from recurrent hæmorrhage due to slipping of the ligature on the ovarian artery. Varnier in 1901 recorded six cases, with three deaths.

But I think it is clear that the operation is not so formidable as was supposed, and also that, with increasing experience and improved technique, the mortality attending it has fallen and may be expected to fall still further. It must also be remembered that the figures show that hysterectomy compares favourably with conservative methods of treatment in the same skilled hands.

As regards method, the main point being simplicity, the majority of operators will no doubt prefer to adopt the subtotal operation; this can be simply and rapidly performed. A second and almost equally important point is the necessity for free drainage *per vaginam*; owing to the presence of the tear it is very easily done, and as the pelvic peritoneum need not be completely closed, a little time in stitching is thus saved. I see no advantage in performing the total operation in these cases. A third necessity is to counteract shock by practising intra-venous or subcutaneous saline transfusion during the operation, and, if

necessary, afterwards. This I did in all my cases, and I do not think any of them could have been successful without it.

Suture of the rent is a method which, by the conservatively-minded surgeon, would naturally be regarded with favour. But it possesses the two crippling disadvantages of often failing to control bleeding and of leaving an infected uterus to do its work later on. I have myself seen two cases (not under my own care) in which after this operation hæmorrhage recurred and proved fatal. And in another case, upon which I operated myself by this method, the patient died upon the fifth day of acute uterine septicæmia. This experience is not peculiar, for in Kolomenkin's series ten cases were operated upon by suture with a mortality of 80 per cent., the greater part of which was due to sepsis. In Klien's larger series the mortality was 50 per cent., but he has included cases sutured *per vaginam*, so that the figures are not comparable.

Owing to the position of the tear, suturing is often difficult and tedious, and to this circumstance the tendency to recurrent bleeding is probably due. Many cases are so badly torn that accurate apposition of the edges is impossible; whilst bruising of the tissues prejudices the chances of obtaining primary union. Small lacerations, unaccompanied by continued bleeding, and accessible in position, may be successfully dealt with by suture if the aseptic management of the labour can be absolutely relied on.

If the uterus is sutured thus, free vaginal drainage must be provided by a separate incision, or through a portion of the rent. In very few operations for suture has drainage been practised, and this omission may in part account for the fact that the mortality is greater than that of hysterectomy, for this is undoubtedly the case.

In conclusion, what I have said about the treatment of uterine rupture may be summarised thus:—

(1) Incomplete rupture of moderate severity, involving only the lower half of the broad ligament, may be treated expectantly by drainage or packing.

(2) In most cases of extensive rupture, whether complete or incomplete, removal of the uterus is required.

(3) In the less serious cases of extensive rupture, not attended by serious bleeding, vaginal hysterectomy may be performed; but this operation has been practised in comparatively few cases.

(4) Abdominal section for exploration of the injury and arrest of hæmorrhage will, as a rule, be required.

(5) According to circumstances this may be followed (a) by packing and drainage; (b) by subtotal hysterectomy.

(6) Suture of the rent is, in the present state of our knowledge, not to be recommended except in the special circumstances mentioned above.

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A Series of Ten Cases of Complete Rupture of the Uterus.

By A. LIONEL SMITH, M.B.

RUPTURE of the uterus is an accident fortunately rare, and I think that the following cases are worth bringing to the notice of this Society, especially as one resulted in recovery and I was able to examine the woman during a subsequent pregnancy and to treat her in her confinement. Eight of the cases were treated in the General Lying-in Hospital and are all the examples of this accident which have occurred in this hospital during the last twenty years; one occurred in the hospital district, and one I saw elsewhere.

I have to thank Dr. Dakin and Dr. Fairbairn for permission to publish the cases which were treated in the General Lying-in Hospital.

CASE I.

The patient, a married woman, 3-para, aged 24, was admitted to the General Lying-in Hospital in July, 1906. Labour occurred at term. The woman was seen early in the morning by one of the district midwives, who reported that the pains were strong and regular, that the vertex presented in the first position, and that labour appeared to be normal in all respects. She was seen again about four hours later, and was then having very strong pains and looked exhausted. The pulse was 100 per minute. The os was fully dilated, the presenting part high up and not advancing with the pains; there was marked œdema of the vulva; there had been no loss of blood from the vagina. About half an hour later the patient suddenly collapsed, and was immediately brought up to the hospital in a cab. On admission she was extremely collapsed. The pulse was 136 per minute, respirations 26, temperature 100·2° F. The uterus was firmly contracted and very tender. The foetal head was felt to the left above the brim. An attempt was made to deliver with forceps, but, as this failed, the head was perforated and delivered with the cephalotribe. The placenta was expressed.

On examination after delivery, a large rent, easily admitting the hand, was found in the lower uterine segment on the left side. The cavity of the pelvis was crammed with blood-clot. All bleeding had ceased. The rent was neither irrigated, packed, nor drained. The foot

of the bed was slightly raised to prevent, if possible, prolapse of the intestine through the rent. The patient's general condition was extremely grave, the pulse being hardly perceptible at the wrist; she complained of great pain in the lower abdomen, for which a hypodermic injection of $\frac{1}{4}$ gr. of morphia was given. For the next twenty-four hours the condition was extremely critical; it then slowly improved. The bowels were opened on the third day with castor oil. From the third to the tenth day the lochia were slightly offensive and the temperature varied between 98.8° F. and 101° F. The vagina was irrigated twice a day with weak solution of lysol. After the tenth day the convalescence was uneventful.

When examined fourteen days after delivery, the uterus was found lying slightly to the right of the mid-line of the abdomen, the fundus reaching to within 1 in. of the umbilicus. In the left iliac fossa there was an ill-defined mass which was tender on deep pressure. The cervix was high up, far forward behind the pubes, and fixed to the pelvic wall on the left side. There was a deep tear posteriorly to the left. To the left of the cervix, extending outwards to the pelvic wall, there was a tender mass which was continuous with the mass felt in the left lower abdomen. The uterus was fairly well involuted.

The patient was discharged twenty-nine days after the accident; the fundus was still high, reaching to about 2 in. below the umbilicus. The mass to the left of the uterus was much smaller and very much less tender. When examined two months later the fundus could be felt about an inch above the pubes. The uterus was somewhat bulky and *semi-fixed*. There was some indefinite thickening to the left of the pelvis.

The patient was not seen again until November, 1908, seventeen months after her confinement. She was then found to be about seven months pregnant. During the next two months she was examined from time to time, but, as the pregnancy appeared to be normal in all respects, there seemed to be no reason for interference. On February 3 she was admitted to the Hospital in labour at term. She had neglected to come until labour was very advanced. I was unable to make a very satisfactory examination of the abdomen, as the pains were so frequent and violent; but I was able to make out that midway between the pubes and the umbilicus there was a fairly definite ridge which I considered to be the commencement of a retraction ring. The os was fully dilated and the vertex, which was presenting in the second position, was fixed in the pelvic brim and did not advance during the pains. There was considerable hæmorrhage from the vagina. Knowing the

history of the case, I thought it would be most unwise to delay for a moment, so I immediately gave chloroform and delivered with forceps. At birth the infant was in a parlous condition: it was extremely pallid, the cry was very feeble, the respiration shallow and gasping, the heart-beat uncountable and almost inaudible. I was at a loss to account for this condition, but, as the infant appeared to have suffered or to be suffering from hæmorrhage, I decided to infuse some saline solution. I cut the cord below the ligature, separated the umbilical arteries from the vein and tied them separately. An ordinary intravenous infusion cannula was then inserted into the vein and about 5 oz. of normal saline injected. The result was immediate and surprising: in a few minutes the character of the cry had entirely changed and was fairly strong; the heart-beat was slow and much more forcible. I may add that the infant eventually did well, and when it left the hospital its weight was steadily increasing.

The total duration of labour was only four hours. After the delivery I thought it advisable to explore the uterus, partly to remove the placenta and partly to see whether there was any cause for the hæmorrhage which preceded delivery. The placenta was attached to the lower zone of the uterus on the left side and partially covered the cicatrix of the rupture which occurred at the previous labour. It was very adherent and was removed in pieces. I think there is no doubt that it had been torn during labour and that owing to this the child had suffered from hæmorrhage. On the left side extending upwards for about 4 in. from the tear in the cervix, there was a definite groove, at the bottom of which the uterine tissue felt hard and almost cartilaginous. On the opposite side, but occupying a slightly more anterior position, there was a ridge of hard tissue about 3 in. in length, lying in the long axis of the uterus. I think there can be very little doubt that this was the cicatrix of a partial rupture which occurred at the previous confinement and which had been overlooked. The puerperium was uneventful and the patient was discharged on the fifteenth day.

CASE II.

The patient, a primigravida, was a very small, single woman, aged 26. Labour at term commenced on November 2, 1906, when the membranes ruptured; on November 3 there was some loss of blood; on November 4 the pains were very strong; on November 5, at the midwife's suggestion, a doctor was sent for; on November 6 he called a colleague in consultation, and on the 7th a third doctor was called in.

Chloroform was administered and various instruments were used, but delivery could not be effected, so the woman was then sent to the hospital. On admission the patient was found to be extremely ill. Pulse 132, temperature 99° F., respiration 32. Vomiting was frequent, and occasionally she had attacks of hiccough. The abdomen was considerably distended and acutely tender. The uterus was firmly contracted and there was a marked retraction ring at the level of the umbilicus. The perineum was lacerated and there was great œdema of the vulva. The discharge from the vagina was very offensive. The remains of the foetal head could be felt at the pelvic brim, the occiput lying posterior. A small quantity of blood-stained urine was drawn off with the catheter.

Having first cleansed the vagina and vulva as thoroughly as possible, I delivered by means of Winter's cephalotribe without any serious difficulty. Immediately after delivery the placenta was expelled into the vagina and removed. The vagina was now irrigated with saline, all clots removed, and the cervix exposed with a speculum. A large rent in the cervix and lower uterine segment was at once seen, through which coils of intestine were visible. The rent was thoroughly irrigated and lightly packed with gauze after all clots had been removed, and the woman was now practically moribund; she was infused with several pints of saline and the shock treated by stimulation. She rallied slightly, but died seven hours later. After delivery the uterus remained firmly contracted and there was no hæmorrhage. The infant, a male, was decomposing; its remains weighed 4 lb.

The following measurements were taken when the patient was admitted, and were verified after death: Greatest abdominal girth, 31 in.; pubes to umbilicus, $5\frac{1}{2}$ in.; left spine to umbilicus, 7 in.; right spine to umbilicus, $6\frac{1}{2}$ in.; fundus to pubes, 10 in.; girth round pelvis, 29 in.; interspinous, 9 in.; intercrystal, $9\frac{1}{4}$ in.; diagonal conjugate, $2\frac{3}{4}$ in.; true conjugate, 2 in.

Post-mortem examination: The body was that of a small, fairly well nourished woman. The tibiæ showed marked rickety curves. On opening the abdomen about a pint of free blood-stained fluid was found in the peritoneal cavity. There was some slight early peritonitis of a few coils of intestine lying in the pelvis and of some of the omentum in the same neighbourhood. Both ovaries were black with extravasated blood. The hand could easily be passed from the vagina through the rent in the lower uterine segment into the peritoneal cavity. There was an opening in the anterior fornix through which a finger could easily be

passed into the bladder. There was also an opening in front of the cervix and to the right, through which a finger could be passed into a track which led outwards and upwards towards the right iliac fossa; this passage contained a considerable quantity of blood-clot. The uterus was removed. The rupture commenced in the mid-line of the posterior lip of the cervix and extended vertically upwards as far as the retraction ring; at this point it turned to the right. It seems probable that if labour had not been terminated, this tear would have completely encircled the uterus, entirely separating the upper from the lower segment.

CASE III.

The patient, a healthy married woman, aged 37, 5-para, was admitted to the General Lying-in Hospital in 1908. The previous labours were normal. The woman was sent into hospital by a doctor in the country who had made prolonged and repeated attempts to deliver with forceps. Labour commenced at term. When the woman was admitted to hospital the head was lying in the transverse diameter at the brim with the occiput to the left. Pains were frequent and very strong. Pulse 120 per minute. The house physician attempted to deliver with forceps, but unfortunately he did not recognize the extreme gravity of the case, and there was some delay in summoning the visiting physician. On Dr. Fairbairn's arrival, the patient was in a very collapsed condition. The uterus was firmly contracted, the fundus being at the level of the umbilicus. Fœtal parts could be felt immediately beneath the abdominal wall. The abdomen was opened by Dr. Fairbairn about twenty minutes after the rupture occurred. The infant and placenta were extracted, as much blood as possible removed, and the rent in the uterus sutured. A small gauze drain was passed through the lower angle of the wound in the uterus into the vagina. After the operation there was some slight improvement at first, but the patient died two and a half hours later.

Post-mortem examination: There was a small quantity of blood in the peritoneal cavity. There was an oblique rent about 5 in. long in the posterior wall of the lower uterine segment extending from the cervix on the left to near the insertion of the round ligament on the right. The rent did not commence at the os and spread upwards, for there is an entire circle of cervical tissue just above the os. This can plainly be seen in the specimen No. 3. On the left there are several vertical fissures in the peritoneum through which the underlying uterine tissue is visible. The cellular tissue of the left side of the pelvis had

been ploughed up in every direction by extravasated blood which had also tracked down into the left ischio-rectal fossa which contained a large hæmatoma.

CASE IV.

The patient, a married woman, a primigravida, aged 33, was admitted to the General Lying-in Hospital in 1903. The woman had been attended by two practitioners, who had first dilated the os and then attempted to perform version, but who had failed to do so after repeated and prolonged trial.

When admitted to hospital the uterus was in a state of contraction, and a well-marked retraction ring could be both seen and felt a little below the level of the umbilicus. The lie was transverse, the head being in the right iliac fossa, the limbs anterior and the right arm presenting. The liquor amnii had drained away. The perineum was torn into the rectum, the cervix was lacerated in all directions. The body was easily delivered after decapitation, but there was some difficulty with the after-coming head, which had to be perforated and extracted with the cephalotribe. The lacerated vagina and the perineum were repaired as well as possible. The total duration of labour was fourteen hours.

After delivery the woman's condition was certainly no worse than when she was admitted; the uterus was well retracted, there was no hæmorrhage. For the first hour or two she appeared to be doing well, but complained of very great pain in the lower abdomen. When examined five hours after delivery there was considerable distension of the abdomen, which was practically motionless and extremely tender; the patient was obviously dying, but lingered for about twenty hours. No necropsy was permitted, but I examined *per vaginam* after death and discovered a small tear to the right of the uterus, which appeared to have spread upwards from one of the lacerations in the cervix. I managed to draw down a loop of intestine; it was very engorged, and had lost its glistening appearance; there were thick flakes of lymph adhering to it. The diagonal conjugate measured $4\frac{1}{2}$ in.

CASE V.

The patient, a married woman, aged 31, 2-para, was admitted to the General Lying-in Hospital in 1895. She presented marked evidences of rickets, and was only 4 ft. 9 in. high. Her first confinement is described as having been "a terrible time," and she was advised to have labour induced prematurely when she next became pregnant. Her

second confinement was natural; she was attended by a midwife, and no particulars are available. Before admission to hospital a doctor had tried to deliver with forceps, after which he had perforated and also attempted to do version.

When admitted to hospital she was very exhausted; the pulse was 140 per minute; the lie was transverse, one arm, both feet, and the cord were presenting. The duration of labour was twenty-six and a half hours. Under chloroform the after-coming head was delivered with the cephalotribe. The placenta was found to have passed into the peritoneal cavity through a rent in the lower segment of the uterus and the vault of the vagina. The patient was treated by stimulation and saline infusion, but she never rallied, and died six hours after the delivery. At the necropsy a large tear 4 in. in length was found in the vault of the vagina, and also involving the lower segment of the uterus posteriorly. Pelvic measurements: Interspinous, 10 in.; intercrystal $9\frac{1}{2}$ in.; diagonal conjugate, $3\frac{1}{2}$ in.; true conjugate, $2\frac{3}{8}$ in.; transverse, 5 in. The foetus was a male, and the remains weighed 8 lb. 8 oz.

CASE VI.

The patient, a married woman, aged 25, 3-para, was admitted to hospital in 1891, after she had been in labour for fifty hours, and after delivery had been attempted many times by means of forceps. The os was fully dilated, and a hydrocephalic head was presenting above the pelvic brim. The head was perforated and delivered with the cephalotribe. After delivery the uterus was well contracted, but the patient was very collapsed. The catheter drew off blood-stained urine, and a urethro-vaginal fistula was discovered. The patient was treated by stimulation and infused with saline, but died about twelve hours after delivery. At the necropsy a tear of the vaginal vault and the lower uterine segment was discovered. The peritoneal cavity contained a quantity of blood.

CASE VII.

The patient, a married woman, aged 37, 8-para, was sent to the General Lying-in Hospital in 1908, and died almost immediately after arrival. She was attended during labour by a midwife, and delivered of a small macerated foetus. After delivery she is said to have lost about a pint and a half of blood. The labour lasted eighteen hours. After an interval of three hours the pains recommenced, and when the midwife



FIG. 1.

A, the anterior abdominal wall reflected; *B*, omentum; *C*, anterior surface of the uterus;
D, the edge of the rent in the uterus.

examined she found the second child presenting by the shoulder. A doctor was sent for, who for several hours endeavoured first to bring down the head and afterwards a leg, but as he was unable to do either he sent her to the hospital.

Post-mortem examination, six hours after death: The first placenta is protruding from the vagina. The abdomen was opened in the mid-line and the abdominal wall drawn to either side. There was a considerable quantity of blood in the peritoneal cavity. The fœtus and placenta had escaped from the uterus, and were lying among the intestines in the right half of the abdomen. The head occupied the right iliac fossa, and was covered by the placenta. The left arm was prolapsed through the rent in the uterus into the vagina. The uterus was almost completely separated from its attachment to the vagina. There was a large tear commencing in the vertex on the right side, and extending obliquely across the anterior surface of the uterus up the insertion of the round ligament on the left side, thus involving a considerable portion of the upper segment. There was a small rent in a coil of small intestine lying close to the back of the fœtus.

The opportunity of recording permanently the position of the fœtus after it escapes into the peritoneal cavity is one which occurs so rarely that I determined to take a series of photographs of the condition, and so record, not only the relationship of the fœtus to the abdominal viscera, but also the position of the uterus and the tear in it. At this time we were fortunate in having Dr. Basil Hood as house physician, who is not only an artistic and enthusiastic photographer, but also an expert, and it is to him that the credit for producing these beautiful photographs is entirely due. The work was done under the most adverse conditions, and but for his unfailing patience and untiring perseverance I am sure would have resulted in failure. After the photographs had been taken the pelvis and contents were removed and are here to-night. (Figs. 1 and 2.)

There was nothing worthy of note in the other organs of the body.

CASE VIII.

The patient was a married woman, aged 40, a 10-para. The face was puffy, the aspect suggesting Bright's disease. The urine contained albumin. Her last confinement occurred two years previously. Unfortunately there is no history of her previous pregnancies and labours.

When admitted to hospital the membranes were intact, the os was



FIG. 2.

A, the left round ligament ; *B*, the anterior wall of the uterus denuded of its peritoneum ; *C*, the right round ligament ; *D*, the posterior wall of the uterus ; *E* and *F*, the torn edges of the cervix ; *G* and *H*, the anterior superior iliac spines ; *K*, the symphysis pubis.

the size of a two-shilling piece, the vertex was presenting in the first position, and the pains were slight and irregular. Labour commenced some time on November 10, 1896, but the pains were irregular, very feeble, and she slept most of the night until 6 a.m. on November 11; at this time she had three very strong pains, which caused her to shriek. She was examined, and it was noticed that she had lost a few ounces of blood. On vaginal examination the os was still found to be the size of a two-shilling piece, the membranes were intact, and the vertex was still above the brim. The membranes now ruptured spontaneously, and the pains immediately became more frequent and stronger. At 11.30 a.m.—that is, about five hours after she had the first strong pains and when the slight hæmorrhage occurred—she looked extremely ill, and was pallid; the pulse was 100 per minute. The uterus was hard and tender and the os was now fully dilated. The case was taken to be one of concealed hæmorrhage, so she was delivered with forceps at once. After the birth of the head there was only very slight contraction of the uterus, so the body was delivered by traction. Only about 4 oz. of blood was lost; the child was stillborn. The placenta could not be expressed; on examination it was found protruding from the os, and when it was grasped in the hand it slipped away into the peritoneal cavity. The pulse was now 144 per minute, and the patient was extremely collapsed; she died within ten minutes of delivery. The first stage of labour lasted thirty-five hours, the second one hour, and the third fifteen minutes. The child was a male and weighed 9 lb. 12 oz.

The rupture was through the posterior wall of the uterus, but unfortunately the exact position was not stated in the notes.

CASE IX.

The patient was a healthy married woman, aged 39. Her nine previous pregnancies and labours have all been normal, and all the children are alive, including one set of twins.

One of the hospital midwives saw the patient at 11 p.m. The breech was presenting in the third position, the cervix was almost fully dilated, the pains were strong and regular, occurring every three or four minutes. During the next hour the body was delivered without any difficulty, but the arms became extended, and the midwife was unable to deliver the head; she now sent for the nearest doctor, not so much because she was unable to deliver, but because the patient had become somewhat collapsed. The doctor arrived within a few minutes, but on his arrival he found the patient *in extremis*; he brought down an arm without

difficulty, and then was at once able to deliver the head. He removed the placenta from the vagina, and almost immediately the patient died.

Post-mortem examination: The body was that of a well-developed and well-nourished woman. The skin was pale and had a peculiar waxy appearance. On section all the tissues were extremely pale and bloodless. The peritoneal cavity was absolutely filled with blood. After the blood had been removed a large rent was at once seen in the left side of the lower segment of uterus extending outwards through the left broad ligament. The pelvic organs were removed and the uterus opened in the mid-line through the anterior wall. The lower segment was extremely thin, in places being considerably less than $\frac{1}{4}$ in. in thickness. Bandl's ring was very well marked. There was a huge rent, which easily permitted the passage of the closed fist, extending from the left side of the external os below to the retracted upper segment above. The tear extended outwards through a considerable portion of the left broad ligament and through the posterior wall into the peritoneal cavity. The torn end, either of the uterine artery or of one of its main branches, could be seen. The upper segment of the uterus was in places nearly $1\frac{1}{2}$ in. thick. On the right side, just above Bandl's ring, there was a complete rupture which easily admitted the index finger. Around the circumference of the lower margin of the upper segment there were about a dozen fissures which extended deeply into the muscular substance; they all ran in the vertical direction and had the appearance of having been cleanly cut with a knife. All the organs of the body appeared healthy. The amount of blood found in the peritoneal cavity was enormous, being estimated at between four and five pints at least.

Unfortunately, owing to the fact that a coroner's inquest was held, no post-mortem could be obtained until decomposition had set in, and the microscopical examination was not very satisfactory. There was certainly no marked naked-eye change to be seen.

CASE X.

The patient, a married woman, 11-para, stated that for the first six weeks of her pregnancy there had been amenorrhœa, but since then there had been a continual loss of blood. Labour commenced at term. When the patient was first seen she had been in labour a week and was in a very exhausted condition. The pulse was 120 per minute. The membranes were ruptured and the uterus was tonically contracted. The child, which presented by the breech, was dead. The upper end of the vagina was occupied by a huge mass of cervical cancer, which had

opened up the bladder and the urethra. Under chloroform a leg was brought down and the after-coming head perforated. The placenta was expelled into the vagina and was then removed. At the termination of labour the patient was certainly not more collapsed than at the beginning, but she gradually sank and died eleven hours later. At the time there was no reason to suppose that the uterus had ruptured.

Post-mortem examination: There was a rent 3 in. long in the lower segment of the uterus on the left side posteriorly; it evidently commenced in the carcinomatous tissue of the cervix and spread upwards. There had been very slight bleeding into the peritoneal cavity.

Authorities vary greatly in estimating the frequency of the occurrence of rupture of the uterus. Winckel states that it happens once in 666 labours, while Lusk and other observers think it is less frequent, about once in 6,000 labours. This great divergence of opinion can only be due to the different methods of calculation adopted.

During the last twenty years 10,989 women were delivered in the General Lying-in Hospital, and among this number there were eight cases of rupture of the uterus—that is, the accident occurred once in 1,373 deliveries. Such a calculation is obviously no guide to the correct frequency of rupture of the uterus, for in six of these cases the rupture had occurred before admission to the hospital. In only two cases did the rupture occur after the patient had been admitted, and a calculation on this basis gives a proportion of one in 5,494 deliveries, which is probably much more correct than the former.

Rupture of the uterus is stated to occur much more frequently amongst multiparæ than primigravidæ. This series of cases illustrates this fact, for 80 per cent. of the patients were multiparæ and only 20 per cent. primigravidæ.

From an examination of the above cases it appears that there are two distinct and entirely different classes of case in which the uterus is liable to rupture.

The first class, and that to which I particularly wish to draw attention, is that in which the uterine action is at fault, this abnormality not necessarily being associated with any obstruction to delivery, though in one at least of this series there was slight contraction of the pelvis. The abnormal uterine action I refer to consists of an increased excitability of the uterus resulting in a great increase in the frequency and strength of the contractions together with a premature retraction, a

condition exactly similar to that produced by the administration of ergot during labour. Case I is a typical example of this class; the total duration of each of her labours was only about four hours, and yet in this short time, in the first labour, the lower uterine segment had become so thinned that rupture occurred, and in the subsequent confinement the pains were so violent and the retraction so marked that I am sure rupture of the uterus would again have occurred if I had not at once terminated the labour artificially. I believe that cases exhibiting this exaggerated excitability of the uterus are fortunately rare, but I think I have seen a few examples in which labour would have ended disastrously if instrumental aid had not been resorted to.

In the second class, in which are included the majority of the cases I have recorded, rupture of the uterus followed a prolonged and obstructed labour. In some cases it was possibly the result of perforation with an instrument, or was due to some intra-uterine manipulation. In several of these cases there were definite and obvious evidences, such as deformity or dwarfing, easily recognizable during pregnancy, which should have warned the medical attendant that parturition would certainly be attended with difficulty and danger. In most of the cases the duration of labour was very much prolonged, and all the usual signs of obstruction and tonic rigidity of the uterus were present, the uterine rupture being the obvious and inevitable termination to labour unless delivery could be effected artificially. There is little to be said with regard to the treatment of these patients prior to their admittance to the hospital; it is appalling to think that, with all the advantages derived from the introduction of antiseptics and anæsthetics, these poor women were subjected to such brutality owing to incompetence and ignorance.

An analysis of the symptoms presented by these patients shows that in only two did the rupture occur without there having been most, if not all, the usual and easily recognizable signs of obstructed labour present. The one symptom exhibited by every patient, generally to a very marked degree, was collapse. In three of the cases the onset of the collapse was sudden, in the remaining seven it was gradual. I think that the gradual onset of the collapse in the latter cases can partly be accounted for by the fact that at the moment of rupture the patient was under the influence of an anæsthetic, for in the three former cases in which the onset was sudden no anæsthetic had been given. The occurrence of pain when the uterus ruptures will, of course, depend upon whether the patient is anæsthetized or not. In only one of these cases was the

patient conscious of a sudden violent pain and of a sensation of something having given way at the moment the rupture occurred. After the uterus had given way, pain and tenderness over the lower abdomen was a marked symptom in every case.

Subcutaneous emphysema is a symptom which has occasionally been noticed after rupture of the uterus. Considering the prolonged manipulations which both preceded and followed the rupture in some of these cases, it is astonishing that this symptom was not observed once. It has frequently been stated that after rupture of the uterus rhythmical contractions cease. I think that this largely depends upon whether the foetus has escaped from the uterine cavity. If the foetus has escaped, the uterus naturally contracts down; but if it has not escaped, and if the rupture has not been so extensive as to entirely separate the upper segment from the lower, there does not appear to be any reason why the uterine contractions should not continue.

In the two cases in which the foetus escaped into the peritoneal cavity uterine contractions ceased immediately. In the remaining eight cases in which the foetus did not escape from the uterine cavity, in five rhythmical contractions to a greater or less extent continued, in two the uterus was tonically contracted, in one the notes are deficient on this point. In all the cases there was a certain amount of blood lost *per vaginam*, but whether this originated in the rent in the uterus or from the lacerations in the cervix, vagina, or perineum, it is impossible to say, except in one case in which there was no doubt that it came from the tear in the uterus.

The amount of blood found in the peritoneal cavity varied greatly, and was estimated at anything from a little under a pint to almost every drop the body contained. A most important question arises: Did the blood escape at the time of rupture, or within a few minutes afterwards, or was there a steady loss until the patient's death? In at least three of the cases there is no doubt that there was a continued oozing of blood until the patient died.

In eight cases the rupture occurred in the lower uterine segment. In two cases the upper segment was also involved. In one case, not only was the retracted portion of the uterus ruptured as a direct result of the extension of the tear in the lower segment, but there was also another complete tear and several incomplete fissures in the upper segment. The specimen, No. 7, exhibited to-night shows not only the complete rupture of the lower segment, but also on the opposite side an incomplete rupture, the peritoneum alone being involved. In

nine of the cases it was the posterior wall which gave way, the rupture occurring once on the right side, four times on the left, and once in the mid-line; unfortunately, in three of the cases the notes do not definitely state the position of the rent. In only one case did the rupture occur through the anterior wall.

When one examines a series of cases such as this, having a mortality of 90 per cent., one at once asks oneself whether other methods of treatment might not have given better results. On considering the question of treatment one can at once eliminate those cases in which death occurred so rapidly that no time was allowed for anything to be done.

Of the seven cases which remain, in Case I the patient's general condition alone was treated, practically nothing was done locally, yet the woman recovered and has had a full-term child since. This case is particularly interesting, for the woman's condition for several hours after delivery was so serious that I am sure any operative measures whatever would have ended fatally.

In the case of the patient (Case III) in which the foetus escaped into the peritoneal cavity, there is no doubt that the proper treatment was to open the abdomen, remove the foetus, and cleanse the peritoneum. I also think that in this particular case the correct treatment for the rent in the uterus was adopted, as being the shortest proceeding possible—namely, to rapidly suture the wound. In dealing with a patient not so profoundly collapsed possibly a more perfect method might have been adopted, but as it is the patient died, so it is obvious that any more severe operative measure would only have hastened the end.

In Case II, in which the rent was packed with gauze, the patient was far too collapsed to have borne any other treatment. I had obtained a very good view of the edges of the rent and was certain that all hæmorrhage had ceased; I was therefore content to pack the tear lightly.

The patient with cancer of the cervix (Case X), was obviously dying, and possibly, when I dragged the foetus through the mass of cancer, I may have ruptured the uterus. If I had to treat a similar case now I should perform vaginal Cæsarean section.

A very significant fact is that among these patients there was not one who could have borne any operative treatment whatever.

DISCUSSION.

The PRESIDENT (Dr. Herbert Spencer) said the thanks of the Section were due to the authors for the papers they had read upon this important subject. Dr. Lionel Smith's contribution of all the cases at the General Lying-in Hospital during a series of years was especially valuable, and he hoped it would be followed by other series from similar institutions. His paper gave a faithful picture of this terrible accident. In considering the question of treatment it was to be borne in mind that, although rupture sometimes occurred spontaneously and in careful hands, it arose for the most part in bad and dirty midwifery practice, and consequently the tissues were often bruised and infected, and many would die however they were treated. More than once he had seen cases suffering from severe septicæmia before the rupture was discovered. In order to demonstrate the local conditions met with he showed ten specimens of rupture from the Museum of University College Hospital. In two of the cases abdominal hysterectomy had been performed, with a fatal result from shock in each case. Only one of the ten cases was a suitable case for supravaginal hysterectomy; in all the others the tear involved the cervix or vagina, the tissues of which were usually bruised, and in two cases was complicated with extensive subperitoneal hæmatoma extending up to the kidney. Total hysterectomy was indicated in those cases which required the removal of the uterus; but those patients were usually too much affected by shock to stand removal of the uterus by the abdominal route. He thought there should be no difficulty in controlling hæmorrhage after vaginal hysterectomy, and that Dr. Eden had been too much influenced by his case where the ovarian artery had been torn across and had retracted out of reach from the vagina. This could only take place when the outer part of the broad ligament had been torn across, a rare condition, which would usually call for abdominal section. He did not think Dr. Eden had made out a case for abdominal supravaginal hysterectomy as a routine method of treatment in complete cases of rupture of the uterus, and still less for cases in which the peritoneum was not involved. He thought that a small abdominal incision might be made to ascertain in doubtful cases if the peritoneum had been opened, but that in complete non-infected cases, in which the broad ligaments had not been extensively torn, suture of the peritoneum (as recommended by Leopold) and gauze drainage would be preferable to removal of the uterus. He did not know on what grounds Dr. Eden styled treatment by gauze-packing "unscientific and unsurgical"; it had, at any rate, saved many lives and uteri. His objection that the gauze did not stop hæmorrhage and passed deeply into the peritoneal cavity only applied to an improper application. The Master of the Rotunda, in publishing two successful cases of gauze-packing for rupture, calls especial attention to this point.¹ Since publishing his four cases

¹ *Journ. of Obstet. and Gyn. of the Brit. Emp.*, May, 1900.

(all of which recovered) he had seen a case of rupture of the cervix in which secondary hæmorrhage from a branch of the uterine artery was stopped by gauze-packing; but unfortunately a recurrence of the hæmorrhage some time after the removal of the gauze led to the death of the patient. While appreciating the value of Dr. Eden's paper, and recognizing that many improvements had been made in hysterectomy since his own paper was published nine years ago, he thought that any routine adoption of the operation for really severe cases of rupture of the uterus would increase the mortality from this terrible accident.

Dr. W. E. FOTHERGILL (Manchester) showed five specimens of ruptured uterus, two of which were lent by Dr. W. K. Walls, of Manchester:—

No. 1.—A small uterus ruptured on the right side nearly up to the Fallopian tube during the delivery of a seven-months fœtus. Patient transferred five miles to St. Mary's Hospital; severe hæmorrhage; abdominal panhysterectomy. Recovery. (August 28, 1906.)

No. 2.—A large uterus with disorganized lower uterine segment, from a patient admitted to St. Mary's Hospital as a brow presentation after failure to deliver by forceps. Child's breech and body in abdominal cavity, the head, arms, and feet occupying the lower uterine segment, and torn right broad ligament. Hæmatoma of vagina and vulva. Abdominal panhysterectomy. Recovery. (February 23, 1908.)

No. 3.—A large uterus with a complete rupture on the right side. The patient was delivered by craniotomy, twelve hours after several failures to deliver with forceps. She had been in labour forty-eight hours when admitted to hospital. The exact nature of rupture not diagnosed before delivery. Abdominal panhysterectomy followed by death the same day. (1908.)

No. 4.—Uterus in which a large complete rupture occurred during the second stage of labour, and was discovered by medical attendant when attempting manual extraction of the placenta, which he could not find. After admission to St. Mary's Hospital abdominal section by Dr. Walls, who recovered the placenta from the abdominal cavity (left hypochondrium). Abdominal panhysterectomy. Recovery. (1905.)

No. 5.—Non-pregnant uterus (post-mortem specimen) with several feet of small intestine. Patient, who was supposed to be aborting, was curetted; and was thereafter admitted to St. Mary's Hospital with 72 in. of small intestine protruding through the cervix. Abdominal section; resection of nearly 80 in. of gut; end-to-end anastomosis and suture of rupture in uterus by Dr. Walls. Death suddenly on fifth day with no symptoms of peritonitis or of obstruction.

Dr. Fothergill said that he did not wish to make any reference to the minor cases of rupture of the uterus which he had seen. It was difficult to estimate the real gravity of minor tears; the majority probably were not diagnosed, the patients dying or recovering according as they became infected or did not. Again, specimen No. 5 threw no light on the treatment. He had, however, seen four cases of serious rupture apart from the four from which

the specimens were now shown. In one of these other four cases gauze-packing was the treatment employed, while in the other three abdominal section was done and the tears were sutured. None of these four patients survived. He had therefore personal knowledge of eight cases of serious rupture of the uterus during parturition. Three of the patients lived, while five died. The three who lived were treated by abdominal hysterectomy. Again, of the four patients treated by hysterectomy three lived.

Dr. J. B. HELLIER (Leeds) sent the following account of a specimen of ruptured uterus from a case treated by abdominal section:¹ I first saw the patient when she was admitted into the Leeds General Infirmary, at midnight, on January 24, 1909. She did not come from the extern maternity department. She was a married woman, aged 22; three previous confinements. She was in a desperately bad condition, blanched and collapsed, and with a piece of omentum protruding from the vulva, and the pulse was 160. I am not in a position to give a detailed history of what had happened before admission, but I was told that there had been a transverse presentation with an arm in the vagina, that the patient had been left for many hours without interference and had finally been delivered by version, and that she had lost a good deal of blood. She was in a very dirty state on admission, and came from a poor house. The portion of omentum at the vulva was rather dry and very dirty, and I decided not to try to return the omentum and then plug the uterus, but to proceed to abdominal section. Saline intravenous infusion was commenced simultaneously with the operation. On opening the abdomen some free blood was found in the peritoneal cavity, but not very much. There was a large tear extending across the anterior uterine wall just at the level of the uterovesical fold of peritoneum and up the left side of the uterus. The rent was about $3\frac{1}{2}$ in. long, the flap of the uterine wall thus formed was retracted upwards, and the hand could easily be passed into the womb. The peritoneum was extensively stripped from the anterior uterine wall, and the left broad ligament was torn and ecchymosed and blood was exuded into the left parametrium. The omentum passed down into the vagina, but there was no prolapse of bowel. A large portion of omentum was now removed and the remaining portion of the uterine wall was cut through at the level of the tear, so as to complete supravaginal amputation. Owing to the bad state of the patient the operation was made as short as possible. The uterine stump was lightly drawn together with one or two catgut sutures and the peritoneum was closed over the stump and the rent in the broad ligament repaired. A central drainage tube was left in the peritoneal cavity through the vagina. The appendages were removed with the body of the uterus. The vessels were tied with catgut and the abdomen closed in the usual way. The pulse improved during the operation and the patient rallied from the shock and did fairly well for forty-eight hours. Then temperature, pulse, and respiration rose quickly, septic symptoms developed with signs of pneumonia, and she died on the sixth day.

¹ The specimen was kindly shown and the account read by Dr. Eden.

On post-mortem examination very extensive septic lesions were found. There was extensive general peritonitis with matting of the intestines. There was suppurative nephritis in both kidneys with small pyæmic foci, the lower lobe of the left lung was nearly solid and pieces sank in water, and there were parenchymatous changes in the liver. It will be seen that the patient rallied from the shock and then succumbed to severe septic poisoning. I fear that this is the history of many cases of uterine rupture. Had the patient been less collapsed I should have preferred to perform panhysterectomy, for I think that better drainage is obtained in that way and more infected tissue removed; but I do not think the result would have been different in the present case. The specimen shown is the body of the uterus removed as just described. On the left side the rent is seen extending upwards, and in this rent the trunk of the uterine artery is laid bare, but not torn across. The partial stripping of the peritoneum is also seen.

Dr. W. S. A. GRIFFITH stated that he had not personally attended a case of rupture of the uterus, although for more than fifteen years he had had over 2,000 cases a year to supervise. He had formerly, at the Obstetrical Society, exhibited a specimen illustrating the rare condition present in one of Dr. Lionel Smith's cases, where there was enormous hypertrophy of the muscular wall of the uterus. The record that they had heard was, as usual, for the most part a tragic record of disaster due to the grossest mismanagement and ignorance on the part of the attendants, though it was clear that this accident did occasionally occur independently of ignorance and mismanagement, as in three cases recorded one evening at the Obstetrical Society in which spontaneous rupture occurred after apparently uncomplicated breech labours.

Dr. HUBERT ROBERTS thought the papers read that evening were of great value and had added very much to our knowledge of the subject. Dr. Roberts's experience was small, but he wished to relate four cases of rupture of the uterus which had come under his care. The first was one which was admitted to the Samaritan Free Hospital in 1899 with a severe tear of the lower uterine segment during delivery of a hydrocephalic head which was not diagnosed till after the breech was born. There was great collapse and the rent was treated by plugging. The patient recovered. The second case was a 4-para, admitted to the Queen Charlotte's Hospital in 1904, with an impacted transverse presentation and about seven months pregnant. Repeated efforts had been made to deliver her outside the hospital. Some hours after her delivery she became collapsed, and, on examination under an anæsthetic, the uterus was found to be ruptured on the right side. The abdomen was opened and contained much free blood; also an enormous subperitoneal hæmatoma existed between the bladder and the lower uterus segment. The uterus itself was torn upwards on the right side for nearly its entire length. It was decided to suture the rent, and this was closed as in Cæsarean section with interrupted fishing-gut sutures. The lower part of the rupture was drained by gauze into the vagina.

The patient eventually did very well. Some interest attaches to this case, as she returned to the hospital in November, 1908, pregnant again at full time. Labour came on naturally, and she was delivered without mishap, owing to the careful treatment of the senior resident medical officer, Dr. Bannister, who sat up with her all night. The labour was somewhat painful, but no trouble occurred in the region of the former tear. The third case also occurred at Queen Charlotte's Hospital in 1907, the uterus being torn during version for a transverse presentation. No rupture of the uterus was suspected at the time, but the patient soon showed evidences of shock and internal bleeding, and it was decided to open the abdomen at once. An extensive rupture was found in the right side of the uterus extending nearly to the insertion of the right Fallopian tube, the right broad ligament being occupied with a large mass of clot. The rupture was sutured much in the same way as in the second case related, and gauze-packing inserted into the lower angle of the tear and into the vagina. Intravenous saline transfusion was performed during the whole operation. The patient lived thirty-six hours. The fourth case was one of placenta prævia, ruptures being probably caused by elastic traction on a Champetier de Ribes bag during delivery. The patient had lost a great deal of blood during the fortnight before labour came on. Version was also performed. Almost immediately after retraction the patient became collapsed and died within half an hour, in spite of all efforts to restore her. Dr. Roberts remarked that these four cases proved little, but one was of interest in that she was delivered naturally of a live child four years after her uterus had been sewn up. As to treatment, Dr. Roberts thought no hard and fast rule could be adopted. Packing simply with antiseptic precautions must still be used in certain cases—*e.g.*, those in which the rupture was incomplete or the patient's condition so grave that abdominal section was out of the question. Dr. Roberts quite agreed with Dr. Eden that if the uterus was ruptured, and the patient's condition admitted it, the abdomen should be opened; also that hæmorrhage was probably the cause of the death in most cases. As to the question of removal of the uterus, Dr. Roberts was of opinion that preservation of the uterus, closure of the rent by suture, with drainage by gauze into the vagina, would yield equally good results as removal of the uterus.

Dr. DRUMMOND ROBINSON stated that he had only had five cases of ruptured uterus under his care. One patient died shortly after the rupture had occurred. In two cases the rupture was discovered after the delivery of the child. Case A: A large tear in the anterior part of the lower uterine segment, through which a long tag of peritoneum protruded, was discovered after delivery by forceps. The patient was very collapsed, but eventually recovered and has since given birth to more than one child. The rupture was packed with iodoform gauze. Case B: A large rupture into the left broad ligament was discovered after delivery by podalic version in a case of transverse presentation. Gauze-packing was employed and the patient made a good recovery. In two cases the fetus was still in the uterus; abdominal hysterectomy (subtotal) was performed, and both patients died. Case A (a case of contracted pelvis): A

complete rupture of considerable size was produced in the anterior uterine wall by attempts to induce labour prematurely by digital dilatation of the cervix. The os was the size of a two-shilling piece; there were practically no pains. The patient was very collapsed, and died twelve hours after abdominal hysterectomy (subtotal). Case B (a case of transverse presentation in which labour had been going on for forty-eight hours): The arm was prolapsed; the os admitted four fingers with difficulty; the child's neck was high up and could only just be touched; there was a large incomplete transverse rupture in the anterior part of the lower uterine segment; the uterine contents were very putrid. Abdominal hysterectomy (subtotal) was performed, and the patient, after recovering from the shock of the operation, died on the tenth day from sepsis. The speaker was surprised that others had not alluded to this class of rupture in which the foetus was still in the uterus and delivery *per vaginam* was contra-indicated. He thought that in cases such as he had described there was no option but to perform abdominal hysterectomy.

Dr. J. S. FAIRBAIRN said he could speak only from a small experience of about six cases, in which, however, the three methods of plugging, suture of the rent, and abdominal hysterectomy had been tried. He thought that there was a place for all these forms of treatment, and that what should be aimed at was some means of recognizing the cases suitable for each method. He had had three cases in which no operative treatment was done, two of which made as straightforward a recovery as any uncomplicated labour case. In one of them there was a rent through the posterior wall of the uterus which admitted three fingers, but there was no bleeding, and all that was required was a gauze plug to prevent intestinal prolapse. In another case from the St. Thomas's Hospital district there was an extensive laceration into the left broad ligament, large enough to admit the whole hand, but not communicating with the peritoneal cavity. On the advice of Dr. Cullingworth, who saw the patient, nothing at all was done and she made an uninterrupted recovery. A third case in which plugging was tried died in twenty-four hours. In the case in which hysterectomy was done, the child, which had been lying transversely, had escaped into the left broad ligament and the placenta had been extruded into the peritoneal cavity. Both child and placenta had been delivered by the outside practitioner before the patient was admitted. She was suffering profoundly from shock and was bleeding, so the abdomen was opened. The posterior layer of the broad ligament was torn into shreds, the ureter remaining as a bridge, and the uterine rent was so ragged that no other course than hysterectomy was practicable. This patient died a few hours later, as also did one mentioned by Dr. Lionel Smith, in whom partial suture of the rent and drainage was tried. These few cases exhibited such differences in symptoms that no one form of treatment would have been suitable in all.

Dr. EDEN, in reply, said that in his remarks the President indicated that he (Dr. Eden) had perhaps been unduly influenced in his attitude towards vaginal hysterectomy by the third case, in which retraction of the ovarian artery

occurred. Having recently read the accounts of a good many operations for rupture, he could assure the Section that this case was by no means unique. In extensive laceration considerable displacement of torn vessels had been noted by others. He thought also that the cases collated by Klien supported his view that this operation was an uncertain method of controlling bleeding, for out of seven cases of vaginal hysterectomy for rupture, one died of hæmorrhage and three of "collapse." He thought Dr. Lionel Smith had taken up a wrong attitude when he said that not one of the cases he recorded could have borne any operative procedure. While the patient remained alive it was not too late to operate for internal hæmorrhages. With the aid of intravenous transfusion he believed that even desperately bad cases might be saved by operating to control bleeding. No case could have been more unfavourable than his first case, in which the patient had been bleeding internally for fourteen hours before operation, yet a successful result was fortunately obtained. Dr. Lionel Smith's series of cases illustrated very well the disastrous results of non-interference, and it was hardly conceivable that these results could have been worse if operative treatment had been adopted. He was much interested in Dr. Roberts's series of cases, but he could not agree that suturing the rent was to be generally recommended, although he believed that, for certain cases in which the injury was small and accessible, it might succeed if the patient escaped septic infection. As cases of rupture were more frequently operated upon we should learn more about them, and in time Dr. Fairbairn's wish might be realized, that clear indications could be laid down as to what cases should be treated respectively by plugging, suture, or hysterectomy.

Dr. LIONEL SMITH said, in reply, he wished it to be clearly understood that he had not employed packing to arrest hæmorrhage. The laceration was lightly packed to provide drainage and to prevent prolapse of the intestine. He thought that case No. 8 was an example of the class referred to by Dr. Drummond Robinson. The uterus undoubtedly ruptured at the moment the woman had the violent pains, for from that time onwards she became progressively collapsed. At the moment the rupture occurred the os was the size of a two-shilling piece. In regard to what Dr. Eden said, he still thought that what he had said was correct, and that the patients were too ill to have borne any operative treatment.

Obstetrical and Gynæcological Section.

June 10, 1909.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

Bilateral Ovarian Dermoid Tumours, one of which obstructed Labour on Two Occasions.

By C. E. PURSLOW, M.D.

THE patient from whom these tumours were removed was first seen by Dr. Purslow on August 28, 1903, in consultation with Mr. Bernstein, of Birmingham. She was then in labour with her first child, and the passage of the fœtus was obstructed by a tumour in the pouch of Douglas; under an anæsthetic the tumour was pushed up and a living female child delivered by forceps. She was advised to come into hospital for operation after the puerperium, but did not do so. On July 26, 1906, she was again in labour, and there was obstruction; on this occasion she was sent into the Queen's Hospital by Mr. Bernstein. She was placed under anæsthesia and the tumour pushed up above the pelvic brim, though with more difficulty than before. A living male child, weighing 7½ lb., was delivered by version, the os not being sufficiently dilated for forceps. Both children have thriven and are well. She again declined operation for the removal of the tumour; however, in February, 1909, she came to the hospital to have the tumour removed. She said that since the last confinement she had been subject to occasional attacks of abdominal pain and vomiting, which sent her to bed for three or four days; for some months previous to admission the abdomen had been enlarging.

On admission, there was a central semi-cystic tumour reaching to the umbilicus, and on vaginal examination a swelling could be felt in the pouch of Douglas. The abdomen was opened, and the larger cyst which

presented in the incision was found to be free from adhesion and to be growing in the left ovary. This was removed, and further investigation showed that the right ovary was also enlarged to the size of a fist and was densely adherent to the back of the uterus and the sides of the pouch of Douglas; it was removed with difficulty. Both cysts were found to be dermoids, containing sebaceous matter and hair; it seemed probable that the right one was the one which had caused trouble, and that it had inflamed and become adherent in consequence of the violence to which it had been subjected. The patient made a good recovery.

Carcinoma of the Cervix Uteri with Metastatic Deposits in the Mucosa of the Uterine Body.

By WILLIAM J. GOW, M.D.

THE specimen shows an extensive infiltrating and ulcerating cancerous growth involving the whole length of the cervical canal. It has not spread beyond the os externum, which is not unduly open. The vaginal aspect of the cervix is apparently normal. The cervix is greatly expanded and enlarged by the growth. At the fundus of the uterus, growing from the mucous membrane, is a firm, prominent, rather warty nodule of cancerous growth. The mucous membrane above the os internum is healthy both in its naked-eye appearance and in its microscopical characters. Several fibroid tumours, interstitial and subperitoneal, exist. This specimen was removed by operation on December 13, 1907, from a multiparous woman, aged 50. Panhysterectomy by the abdominal route without removing any portion of the vagina was the operation adopted. Convalescence was normal, and the patient at the present time is quite free from any signs of recurrence.

In March, 1906, the patient was found to have a rapidly growing abdominal tumour, and free fluid developed in the peritoneal cavity. On April 5, 1906, this tumour was removed, and proved to be a soft, solid tumour of the right ovary, the size of two fists. In a report signed by Dr. Cuthbert Lockyer this was said to be an adenoma of the ovary, possibly malignant. The patient completely recovered from this operation and remained quite well for more than eighteen months, when she began to be troubled with irregular and at times free bleeding. This led to the discovery of the cancerous growth

in the cervix, which was removed as above described. Cancer of the cervix may sometimes spread by direct continuity into the uterine body, but the development of an apparently independent nodule of cancer in the mucosa of the fundus without involvement of the intervening tissue is rare. Dr. Amand Routh describes a case of independent cancer of the body and cervix uteri¹ in which, in a woman aged 56, an epitheliomatous mass growing from the posterior lip of the cervix was found to be associated with cancer of the lining membrane of the uterus. No description of the microscopical appearances of these growths is given.

In the work of Winter and Ruge, entitled "Gynæcological Diagnosis," an illustration is given of a condition almost identical with my specimen. It is from a specimen in the Königsberg Museum, and is described as "Ulcerating cervical carcinoma behind the closed os externum of a multipara with metastases in the body." Whether this condition should be described as an independent growth in the cervix and body, or whether the growth in the body should be regarded as a metastatic deposit from the cervix, is not quite clear. In my own case and in those above mentioned the disease in the cervix was far more advanced than that in the body, so that at any rate there is reason to suppose that it first appeared in the former situation. In Winter and Ruge's case, as in my own, the patient was a multipara, and the os externum was closed.

Dr. T. G. Stevens, who kindly examined the growth in the two situations, states that in the cervix the growth presents the appearance of a typical malignant adenoma, whilst that in the body has the appearance of a spheroidal celled carcinoma.

Report of the Pathology Committee.—The committee is of opinion that both growths are carcinomata, and that probably the growth in the cervix is the primary one.

DISCUSSION.

Dr. T. G. STEVENS said the importance of the specimen lay in its possible relation to the ovarian tumour. He agreed that the growth of the cervix was in all probability primary and of slow development, and regarded the growth at the fundus as a metastasis, and the ovarian tumour, notwithstanding its very rapid development, as probably also secondary to the cervical carcinoma. The cervical carcinoma was a typical malignant adenoma, and had invaded the

¹ *Trans. Obstet. Soc. Lond.* (1896), 1897, xxxviii, p. 100.

300 Bland-Sutton: *Red Degeneration of Uterine Fibroid*

whole thickness of the wall of the cervix. Although the fundal growth was a solid growth and more closely resembled a spheroidal celled carcinoma, this was not an objection to the metastatic view, for by a process of metaplasia it is not uncommon for a tubular carcinoma to become a growth with solid epithelial masses. The fundal growth also clearly invaded the endometrium, but was not actually derived from it.

Dr. AMAND ROUTH remarked that in 1896 he had shown a similar specimen of cancer, originating separately in the uterine body and cervix, in a 9-para, aged 56. It was about the time when cancer of the cervix was being treated by panhysterectomy, instead of supravaginal amputation of the cervix, and he had shown the specimen to emphasize the advantage of the more radical operation.

On a Uterine Fibroid in the state of Red Degeneration containing *Staphylococcus pyogenes aureus*.

By J. BLAND-SUTTON, F.R.C.S.

PROFESSOR LORRAIN SMITH and Dr. Fletcher Shaw have recently published a paper¹ containing an account of a pathological and bacteriological examination of four uterine fibroids in the condition known as red degeneration. Each specimen was associated with pregnancy. These observers express the opinion that this striking change in uterine fibroids is due to thrombosis of the vessels in the tumours. In two of the fibroids they isolated micro-organisms—*e.g.*, *Staphylococcus* in one, and a *Diplococcus* in another. The patients with these tumours exhibited toxic symptoms. Since 1901, when I first became acquainted with this red change, I placed several fibroids in which it was well marked into the hands of bacteriologists with the hope that a micro-organism would be detected which could be held responsible for the degeneration. The results were so persistently negative that the search was abandoned. On learning that micro-organisms had been detected in connexion with red degeneration, I had the next specimen which came to hand examined bacteriologically, with a surprising result. The case is in some respects exceptional.

A primigravida, aged 30, two months advanced in pregnancy, had been to London for the purpose of consulting a doctor, who, after an

¹ "The Pathology of the Red Degeneration of Uterine Myomata," *Journ. of Obstet. and Gyn. of the Brit. Emp.*, Lond., April, 1909, xv, p. 225.

examination, expressed himself satisfied with her condition. On the return journey the woman was seized with such sudden and acute pain that she left the train at an intermediate station and placed herself under the care of a doctor whom she knew. He found a large and tender swelling on the right side of the abdomen; this, in conjunction with the history, led him to the opinion that the patient was the victim of tubal pregnancy which had burst. In this belief Dr. McCarthy summoned me to see the woman. I saw her twenty-four hours after the onset of the symptoms, and found a large tumour, probably a fibroid, occupying the right half of the belly, and reaching as high as the liver. I considered that some change had taken place in this tumour consequent on the pregnancy. It was also possible that it might be an ovarian cyst which had twisted its pedicle. The tumour was very tender; the patient had a pulse-rate of 112 per minute, and her temperature was 100° F. On opening the abdomen the tumour proved to be a large subserous fibroid with a broad stalk, the uterus was gravid, and, as it contained several fibroids the size of golf balls, it was removed. On examining the big fibroid in the course of the operation I detected an area of softening about 5 cm. in diameter. It appeared to be acutely inflamed, and there were flakes of lymph over this soft area. I noticed this peculiar necrotic patch as soon as the tumour was withdrawn from the abdomen, and took especial care to avoid contaminating my gloves with it, but in spite of this care one of the sutures caused a stitch abscess. The patient recovered.

As soon as the operation was completed I had the uterus carefully packed in sterilized waterproof material, and on reaching London conveyed it direct to the bacteriological laboratory for investigation. When the large fibroid was bisected we found a pyriform patch of red-softening equal in size to one-third the total area of the tumour. The remainder of the fibroid was very hard, but the degenerate area had become so soft that the finger could be pushed into it with ease. Mr. Somerville Hastings undertook the bacteriological examination of this "infarcted area." Portions were abstracted with the usual precautions, and he succeeded in obtaining in pure culture *Staphylococcus pyogenes aureus*. The precise methods employed by him are stated in his report appended to this paper. Mr. Hastings also made a careful examination of the tissues from this degenerated tissue, and found the vessels thrombosed, and in some situations the clot has undergone partial organization. The interstitial fibroids exhibited the red change in streaks, but no micro-organisms were detected in them. Although staphylococci

were detected in the large tumour in the case under consideration, I am bound to express the opinion, derived from a long study and opportunities of examining a large number of red fibroids, that this change is not due to micro-organisms, but to mechanical interference with the circulation. The sharply defined area of red-softening in the large tumour resembled an infarction, and, if this be the case, the presence of staphylococci is in all probability an epiphenomenon.

Two other fibroids with red degeneration, one from a gravid and the other from a non-gravid uterus, have come to hand since the tumour which is the subject of this communication was investigated, but we failed to find micro-organisms in their tissues after the most painstaking bacteriological examination.

NOTE ON A DEGENERATED RED "FIBROID."

By SOMERVILLE HASTINGS, M.S.

On February 10 a uterus containing a degenerating subserous fibroid was received from Mr. Bland-Sutton. With a piece of iron heated to redness a linear sear some 3 or 4 in. long was made on the peritoneal covering of the fibroid. A scalpel sterilized by prolonged boiling was then taken and an incision made deeply into the tissue of the fibroid by cutting along the seared area. The cut edges were retracted, and two small pieces of tissue, about the size of peas, were removed by forceps and scissors, also sterilized by boiling, from the centre of the fibroid and dropped into the two tubes of ordinary peptone broth. These were incubated at 37° C., and the next day the staphylococci were obtained from both tubes. The staphylococcus gave the staining and cultural reactions of *Staphylococcus pyogenes aureus*, and the cultures were shown to be pure by being plated out.

Tumour of the Ovary. ? Adenofibroma or Endothelioma.

By G. F. DARWALL SMITH, F.R.C.S.

THE patient from whom this tumour was removed was a married woman, aged 36, who had had three children and no miscarriages. She was admitted into St. George's Hospital on June 27, 1907, under

Dr. Dakin, complaining of increased frequency of menstruation, of pain low down on the right side of the abdomen, and of some yellow vaginal discharge. The pain and the discharge had been noticed for two years in each case; the hæmorrhage, which was merely an increase in frequency without any change in duration or quantity, dated from five years before. She had been seen as an out-patient fifteen months before admission and had been treated medically for the hæmorrhage, with temporary success. On admission a rounded solid tumour was felt in the right posterior quarter of the pelvis, which was removed through the abdomen in the usual way. There was no free fluid in the peritoneal cavity, and convalescence was uneventful. At the operation the left ovary appeared to be normal and was left *in situ*. The tumour had not appreciably increased in size since the time that she was seen first, fifteen months before operation.

Description of Specimen.—I am extremely sorry not to be able to show the actual tumour. It was put aside very carefully in order that it might be mounted as a museum specimen, so carefully indeed that a diligent search has not succeeded in bringing it to light. The tumour was almost spherical and about the size of a clenched fist. The tissue of the ovary could be seen with the naked eye to be stretched as a capsule over the substance of the tumour proper and could be separated easily from it. The cut surface of the tumour looked exactly like an ordinary ovarian fibroma, and nothing unusual was suspected until the microscopic sections were examined.

Description of Microscopic Appearances.—Microscopic examination confirms the impression that the ovary was stretched over the tumour as a capsule. Between the ovarian tissue and the substance of the tumour proper is a loose and very vascular layer of fibrous tissue. The tumour itself, which is clearly marked off from the latter layer, consists for the most part of a dense fibrous stroma in which little or no muscle tissue can be seen. Scattered about in this stroma are numerous strands of cells, which somewhat resemble those of glandular epithelium. The smaller strands are composed of solid masses of these cells. In the larger strands the central cells are seen to have become vacuolated and broken down, to such an extent that some of the strands are merely represented by spaces without any epithelial lining at all. This degeneration is in many cases accompanied by the formation of colloid material, which in its earlier stages appears in the form of pearl-like bodies surrounded by the cells of the growth. The strands of cells are clearly defined from the fibrous stroma in which they lie,

and show no signs of infiltrating it in the manner of a malignant growth.

The diagnosis seems to lie between endothelioma and adenofibroma. This kind of degeneration is, I believe, very apt to occur in endotheliomata, and, if the growth is an endothelioma, it is obviously one of very



FIG. 1.

View under very low power. *A*, ovarian tissue; *B*, intervening connective tissue (very vascular); *C*, tumour substance.

low malignancy, since the tumour remained practically unaltered in size for fifteen months. If, on the other hand, it is an adenofibroma, it is not very easy to say whence the adenomatous part of the growth has originated. I should be very grateful for the opinion of



FIG. 2.

View under low power. *A*, vascular connective tissue intervening between ovarian tissue and tumour substance; *B*, tumour substance.

the Section as to the correct diagnosis. I am much indebted to Dr. Dakin for allowing me to report the case, and to Dr. Harold Spitta for the microphotographs, which were taken in the Peirse-Duncombe laboratory at St. George's Hospital.

Report of Pathology Committee.—The committee are unable to express an opinion as to the nature of this tumour. They find no histological evidence of malignancy. In their opinion the diagnosis lies between an adenofibroma of Wolffian origin and an endothelioma.



FIG. 3.

View, under high power, of tumour substance.

Dr. GRIFFITH mentioned that he had removed a similar tumour of the right ovary with similar microscopical endothelial characters two years ago (June, 1907), and that he had been strongly urged by the relatives to give an opinion as to its malignancy, but he felt unable to do so, and up to the present time there was no evidence of recurrence and the patient was in perfect health. The left ovary was small and healthy and not removed.

Fœtal Chondrodystrophia as a Cause of Brow Presentation and Dystocia.

By KEDARNATH DAS, M.D.

AMONGST the causes of brow presentation and of consequent dystocia, no mention is made of fœtal chondrodystrophia in any obstetric text-book. I have had two such cases, short notes of which are appended below. The neck, in these fœtuses, is extremely short. The configuration of the head is such that the occipital and sincipital arms of the head lever are almost equal. The head, also, is set squarely upon the shoulders in a sort of military attitude of attention, turned upside down. The short neck, and the peculiar shape and position of the head, thus enable the latter to maintain its position of unstable equilibrium between flexion and extension, with a resultant persistent brow presentation. The shape of the head is an antenatal condition, as I found by observing the fœtus which was delivered by Cæsarian section, and also the living individuals with this pathological condition.

If it were possible, then, to diagnose this condition of the fœtus before delivery, it would greatly influence the obstetrician in the treatment of these cases. The diagnosis is not impossible. We may not be absolutely certain, but at all events we may make a fair guess. The head in these cases is always large in proportion to the body. In cases of brow presentation, then, with large head, if the trunk and limbs of the fœtus appear to take comparatively less space in the uterus than might be expected from the size of the head, we may fairly assume that it is not a large normal fœtus. A large hydrocephalic head can easily be eliminated by its usual characteristics. If we thus eliminate a normal fœtus and a hydrocephalic fœtus, chondrodystrophia will be left. Cases usually present themselves after the second stage of labour has gone on for some time, and delivery by natural efforts has failed, and it is then that we are called on to diagnose.

This fœtal disease does not prove incompatible with postnatal life. Although some fœtuses succumb a few hours after birth, many survive and reach the adult state. Are we justified, then, in resorting to destructive operations in obstructed delivery due to a chondrodystrophic fœtus? I would answer in the affirmative. We would only have the chance of diagnosing these cases when labour has become pathological, and we would not be justified in putting the mother's life to any extra risk to save a pathological fœtus with an uncertain postnatal life.

In my first case I performed craniotomy, and in the second Cæsarean section. In the latter case, if I had even a suspicion that the fœtus was chondrodystrophic, I would certainly have resorted to craniotomy. Of course I was misled by the history of the case, and thus did a conservative Cæsarean instead of a hysterectomy.



FIG. 1.

CASE I.

S., aged 15, a Bengali primipara, had been in labour for over thirty-six hours before I saw her. Several attempts to deliver by the forceps

were made by a local practitioner without success. On arrival I found the vulva swollen and a big caput succedaneum pretty low in the pelvis. Her temperature was high and pulse rapid. I at once perforated the head and delivered the foetus (chondrodystrophic) without much trouble.



FIG. 2.

The site of perforation can be well seen in the diagram (fig. 1). The puerperium was complicated by septic troubles, but she recovered with a very small vesico-vaginal fistula, which was closed later on.

CASE II.

S., aged 30, 1-para, was admitted into the Campbell Hospital at 6 p.m., on May 13, 1906. She was brought in by friends, who could not give any history except that she had been in labour about twenty-four hours. On admission, the pains were very strong, uterus tender, retraction ring well marked, a little above the symphysis pubis. Fœtal heart sounds were well marked. Head was above the brim, and the brow presenting. Her general condition was fair. She was put under chloroform and a thorough vaginal examination was made. The pelvis did not seem to be capacious. The presenting part was fixed at the brim. As the fœtal heart sounds were well audible, I decided to perform Cæsarian section. I did a Säger at once. The fœtus, chondrodystrophic, with a very big and ossified head, was delivered asphyxiated. She was removed to bed in a good condition, with a pulse of 80 and a temperature of 97° F. On May 14, 1906, the temperature rose to 102·6° F. at noon, but otherwise she was in a fair condition. On the morning of May 15, however, she complained of pain in the abdomen with tympanites. The temperature did not go above 99·2° F. on this day, but in the evening the pulse-rate went up to 144. On May 16 she passed three loose stools during the day. The pulse ranged between 140 and 152. The highest temperature was 101·6° F. early in the morning. Restlessness with retching supervened. Antistreptococcic serum was injected liberally without any impression. She died at 8 p.m.

The day after the operation a sister of the patient called and gave the following further information. (1) That the membranes ruptured about twelve hours before her admission to the hospital; (2) that a midwife (unqualified) was called in the morning and she "tried to deliver her" but failed, and she then left the patient, advising her to go to a hospital; (3) that her first labour was instrumental (forceps). This information was received too late.

The PRESIDENT (Dr. Herbert Spencer) said that he did not think that the posterior arm of the "head lever" was ever as long as the anterior arm in face or brow presentations. Nor did he think that it was right to perform craniotomy for chondrodystrophia. Even in cases of hydrocephalus aspiration was more suitable than perforation, as it did not necessarily sacrifice the child.

A Case of Rudimentary Uterus Didelphys, with Ectopia of each Uterine Body in an Inguinal Hernia Sac ; with some Remarks on the Development of the Female Genital Organs.

By W. BLAIR BELL, M.D.

OF the many deformities of the uterus due to developmental errors, uterus didelphys appears to be one of the rarest. In addition, of the known cases the majority have occurred in children (frequently stillborn) who have been afflicted with other deformities [10]. The malformation arises, of course, from the failure of the Müllerian ducts to fuse during the third month of foetal life. Partial fusion which results in a septate or cornuate uterus is fairly common, and cases of this kind frequently come under the observation of gynæcologists. The complete separation of the two halves of the uterus, with the resulting uterus didelphys, is an exaggerated condition of the other forms, but the actual causal factors determining this state of affairs have not been fully worked out [1]. For this reason it is important that all cases of uterus didelphys should be reported, and especially those cases which show unusual features that may possibly have a bearing upon the pathology of the condition. The following case was sent to me by Dr. Morris, of Old Colwyn, to whose interest in the case I am indebted for the opportunity of dealing with it.

L. R., aged 20, had suffered from double inguinal hernia all her life. Lately there had been considerable increase in the size of the herniæ, and attacks of abdominal pain were not infrequent. It was during one of these attacks that the advice of Dr. Morris was sought. On examination the patient was found to be a well-grown, well-developed and intelligent girl. She had never menstruated, nor had she felt any monthly molimina. The breasts were large, and the external genitals were covered with hair and normal in appearance. There was a total absence of vagina, but there was a circular fringe suggesting a hymen, around a dimple not more than $\frac{1}{8}$ in. deep in the situation of the vaginal orifice. *Per rectum* nothing could be felt of any of the pelvic genital organs. There was a large inguinal hernia on each side. Bowel descended into the sacs, but was easily reducible. A hard, irreducible lump, felt in the right sac, was thought to be

omentum. On the left side a similar hard, irreducible lump was felt, which was also considered to be omentum, and a small, round, reducible body was made out to be the left ovary. Total absence of uterus and vagina, with double inguinal hernia and hernia of the left ovary, was the diagnosis arrived at.

Operation was performed on August 24, 1908. A sub-umbilical median incision was first made, in order to investigate the state of affairs. With the patient in the Trendelenburg position a good view of the whole pelvis was easily obtained. One saw no sign at all of any genital organs in the pelvis proper. The peritoneum formed a smooth and uninterrupted recto-vesical pouch. Above the true pelvis on either side a large hernial orifice was seen at the site of each internal abdominal ring. These readily admitted the index finger. Issuing from the right opening was the Fallopian tube, underneath which lay the right ovary, somewhat cystic, close to the lower border of the ring. On the left side only the fimbriated extremity of the Fallopian tube of that side protruded from the hernial opening. There was no omentum entering the canals. This state of affairs at once gave one a hint as to the contents of the hernial sacs. An incision was now made over the left inguinal canal and the sac dissected out in the usual way; some difficulty was experienced in freeing the underneath or deep surface. When this had been accomplished the sac was opened and found to contain the left uterine body with ovary and tube attached (*see fig. 1, A*). The whole was ligated at the neck of the sac and a radical cure effected in the ordinary way. The same proceedings were then carried out on the right side, and the sac—also firmly attached on the deep surface—removed with the contained uterine body (*see fig. 1, B*). In this case, however, the tube and ovary after ligation were freed from their connexion with the uterus at the point F (*fig. 1, B*) and allowed to remain in the abdomen just inside the hernial orifice. A radical cure of the hernia was then carried out on this side also. Finally the cysts were dissected out of the right ovary and the central incision closed. It should be mentioned that the cervixes of the uterine bodies ended blindly on each side at the hernial orifice, blending with the peritoneum. The patient made an excellent recovery, and returned home at the end of three weeks.

A histological examination of the right uterine body shows that it is almost entirely composed of fibrous tissue. The sections were very difficult to cut owing to the extreme hardness of the specimen, consequently a somewhat poor photomicrograph was obtained (*fig. 2*). In *fig. 3* is shown a section of the left ovary, in which corpora lutea

are to be seen, indicating the functional activity of the organ. This interesting case presented, then, the very rare condition of rudimentary uterus didelphys in an adult, with *ectopia* of both uterine bodies in hernial sacs. The uterine bodies were imperforate, but the Fallopian tubes were patent.

Of uterus didelphys itself there is not much to be said, for the literature contains many references to this condition, and the classical paper of Giles [6] contains many interesting cases—twenty-nine in all, four of which were discovered on the post-mortem table. In almost all of these cases, however, the uterus was functional, and in none was there any hernia or *ectopia* present. This is interesting

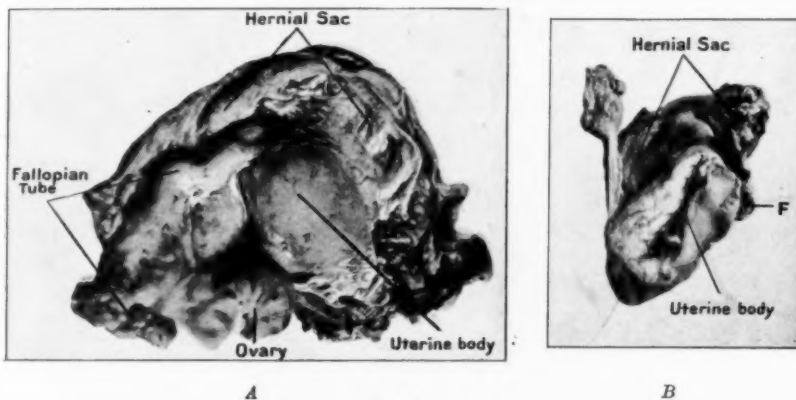


FIG. 1.

Specimens removed: A, from the left inguinal canal; B, from the right inguinal canal.

in connexion with the statement of Kussmaul (already referred to) that the condition is rarely found in adults, and that of Hilgenreiner [8], quoted by Cranwell [5], who points out the frequency of other congenital malformations which may produce early death. It follows therefore that, generally speaking, those women who reach adult life with a uterus didelphys rarely possess other congenital malformations, and that in them the uterine bodies are functional.

In regard to inguinal hernia of the uterus, Cranwell [5] has recently published a very valuable paper on this subject. In it he discusses a case of his own and forty-five other cases recorded in the literature.

Of these forty-six cases, he says that thirty-two can be definitely stated to be congenital. Six of the cases occurred in young children, six in men, and several were in pseudo-hermaphrodites. Two of the cases, No. 19 (Roux) [12] and No. 28 (Werth) [13], are identical with mine. That is to say, in each case there was a rudimentary uterus didelphys with each uterine body firmly incorporated with an inguinal hernia sac—a condition I have ventured to call *ectopia* in contradistinction to hernia. These two cases and mine seem to be the only three cases of the sort on record.

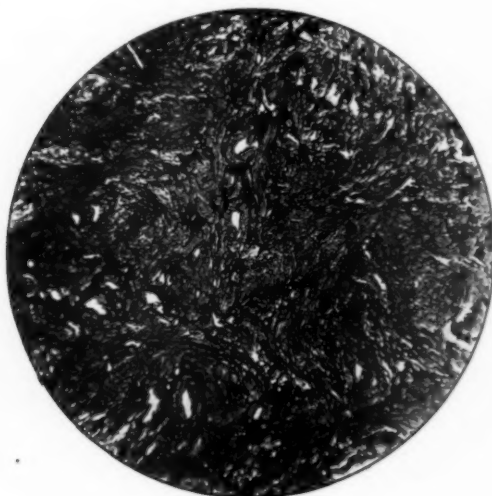


FIG. 2.

Section of rudimentary uterine body from right hernial sac.

Herniæ of the uterus in other regions are extremely rare, apart from umbilical hernia of the gravid uterus—a condition which can hardly be placed in the same category with the other forms, since such a state of affairs is, strictly speaking, an acquired eventration, rather than a hernia in the ordinary sense, and probably only occurs in multipara. Brunner [3], however, has collected two cases of crural hernia of the uterus and one of obturator hernia. These cases, like those of Roux [12], Werth [13], and myself, are of great value from a developmental standpoint.

There are those who state, with much evidence in favour of their argument, that all inguinal (and possibly other parietal) herniæ are either truly congenital from the first, in that a patent canal exists, into which intestine has been able to descend from infancy, or that a potential passage persists as the result of failure of the canal to become obliterated. Any slight strain may subsequently cause this to be opened up by the descent of intestine into it. This is a point of much importance, but it is contested by others who, while admitting that the congenital origin of many herniæ is undoubted, believe that



FIG. 3.

Section of left ovary. Three corpora lutea, marked *C*, can be seen.

the majority of cases are due to strain alone. The truth probably lies half-way between these conflicting opinions, and herniæ occur either through congenital openings or through those imperfectly closed in the process of development, or where the resisting surface is less than elsewhere. This weaker resisting surface usually occurs, of course, at the site of the congenital orifices.

In regard to herniæ of the female genital organs, we need only consider those cases in which some congenital defect leads to the condition found, for accidental herniæ, such as those of the gravid uterus

through the middle line, are of no great interest or complexity of causation. Of the congenital forms we find:—

(1) Persistence of the canal of Nuck, or other congenital canal, into which the hernia of ovary, ovary and tube, or ovary, tube, and uterus may occur. It is, indeed, of frequent occurrence to find ovaries and Fallopian tubes in the inguinal hernial sacs of females. Carmichael [4] states that such is the case in from 25 per cent. to 30 per cent. of all the cases of inguinal hernia operated upon in young females. Hernia of the uterus, however, is very rare.

(2) A condition of *ectopia* of the genital organs. In these cases the genital organs, or some part of them, are incorporated with the peritoneum forming the sac. Apparently two forms may occur: (a) Where the ovary becomes attached and drags the uterus with it; (b) where the firmest attachment is to the uterus, as in my case.

In regard to the first class—where there is as the predisposing cause merely a patent canal of Nuck—we must regard the hernia of ovaries and tubes, or even of the uterus, as a secondary and more or less accidental occurrence, while recognizing the predisposing factors in the patent canal and attachment of the round ligament.

It is, however, from the cases which occur in the second class that we should derive information of value in regard to the normal processes of development. Yet how difficult of interpretation much of it is! How often are the findings in some of these abnormalities out of keeping with much that is laid down as certain in these matters!

It may be worth while, therefore, to take some of the points presented by this case and see how far they support or contradict the accredited views of development. According to Keith [9], the round ligaments are developed within the inguinal fold of peritoneum, which extends backwards to the groin from the Wolffian ridge, by the development of non-striated muscular tissue; and that the piercing force, by which the abdominal wall is penetrated, is the result of "the inertia of its own growth." I must say that I think the whole subject of the "descent" of the genital organs is somewhat "wropt in mystery," and I confess to being unable to understand any of the explanations offered—if such hazy descriptions can be called "explanations." There are two points which, I think, require clearing up. The first is how the distal (pubic) attachment of the round ligament is obtained, and the second is what is the guiding force which brings the Müllerian ducts together to form the united "genital cord."

In regard to the second point, clearly the peritoneum must be closely

adherent to the underlying muscular fibres whether the cord follows the ordinary route through the inguinal canal, or passes through the obturator foramen, or follows any other direction. Since, however, the fibres which go to form the round ligament do not cross the middle line, any pull there may be by the inertia of their growth and that of the peritoneum must tend to keep the Müllerian ducts apart rather than to bring them together; and, again, the shorter these ligaments are the more likelihood is there that a uterus didelphys will be formed. It has already been shown that the round ligaments are not necessarily attached to the uterus, for in a case previously described by me [2] there was no uterus, and each round ligament was attached to the lower pole of the ovary (clearly showing that the ligament of the ovary was continuous originally with the round ligament) and passed through the obturator foramen on each side. It seems probable, therefore, that the Müllerian ducts are drawn together in part by that muscular layer (also subperitoneal) which forms the utero-sacral ligaments, and also by those fibres which decussate in the middle line and form the external muscular coat of the uterus (*see fig. 4*). The round ligaments tend to keep the Müllerian ducts apart, but are in normal cases overcome by the more powerful action of the interlocking fibres which form the external muscular layer of the fused Müllerian ducts, as already stated, the whole genital cord being kept down in the pelvis, which is growing around these structures, by the utero-sacral muscular fibres; that is to say, in regard to the round ligaments, that they are bundles of muscular fibres which have become hypertrophied by the strain put upon them in the process of the union of the Müllerian ducts and the general "descent" of the organs. In the male the comparative atrophy of the Müllerian ducts and overlying subperitoneal muscular fibres leaves the testes free to be "drawn" into the inguinal canal. In the case under notice evidently the interlocking muscular fibres did not come into action, and the Müllerian ducts, being fixed by their shortened mesenteries, remained attached in the original position of the "inguinal fold," and no round ligaments were developed.

Next, in regard to the vagina and hymen, it has already been pointed out that in this case there was a fringe of hymen, but no vagina at all. It has been shown by Berry Hart [7] that the vagina is formed by the downgrowth of cells which force their way from the lower end of the Müllerian or Wolffian ducts—from which is not quite certain—to the urogenital sinus. In the case here recorded there was no sign of anything beyond the cervixes fused in the hernial sacs, so that we may

presume that a vagina can only be formed when the Müllerian ducts come in relation with certain mesoblastic structures in the neighbourhood of the urogenital sinus. This fact is also in favour of the Müllerian origin of the upper portion of the vagina.

In regard to the urogenital sinus itself, this seems to vary very much in extent. In the case here recorded there was no trace of a vaginal portion of the original urogenital sinus; on the other hand,

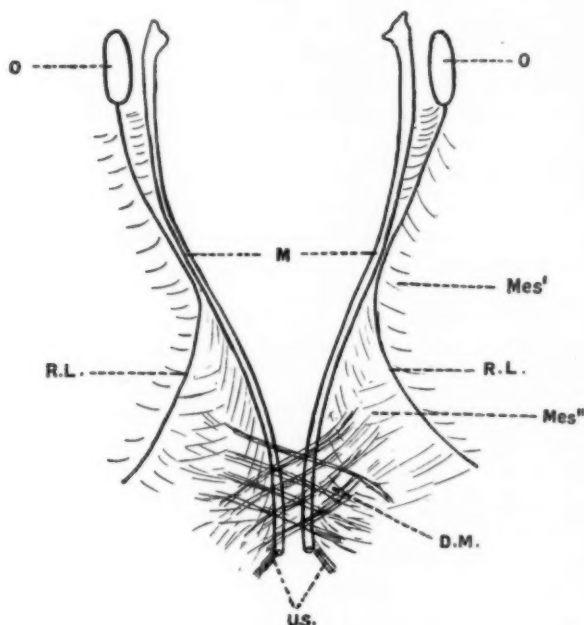


FIG. 4.

Diagram to illustrate the junction of the Müllerian ducts. O, ovary; M, Müllerian ducts; Mes' Mes'', genital mesentery; R.L., round ligament; D.M., decussating subperitoneal muscular fibres; U.S., utero-sacral subperitoneal muscular fibres.

I have also seen recently two women who had distinct urogenital pouches $1\frac{1}{2}$ in. to 2 in. in depth, fringed with hymens, with total absence of an upper portion of vagina and uterus. In these cases the urogenital pouches had all the appearance in size and structure of the

lower part of the normal vagina. In the other case I have mentioned, previously reported by me [2], there was a very shallow blind depression with absence of clitoris, labia minora, and hymen. It is apparent, then, that the vagina may be wholly formed by the mesoblastic down-growth from the lower ends of the Müllerian ducts opening just inside the hymen or ending blindly and producing the so-called "imperforate hymen"; or—and probably most commonly—by the perforation of these processes into a urogenital pouch which extends upwards for $1\frac{1}{2}$ in. to 2 in. within the hymen.

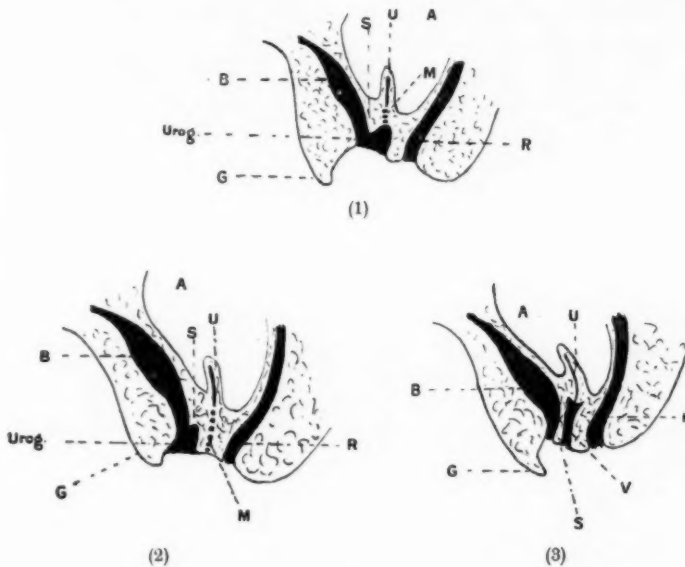


FIG. 5.

Normal methods of development of the vagina. A, abdominal cavity; B, bladder; G, genital tubercle (clitoris); M, mesoblastic downgrowths from the ends of (?) Müllerian ducts; R, rectum; S, downgrowing vesico-vaginal septum; Urog., urogenital sinus, or pouch; U, uterus; V, vagina.

In fig. 5 (1) is seen a diagrammatic representation of the normal way in which the mesoblastic downgrowths from the lower ends of the Müllerian ducts (which unite to form the uterus, U) perform their part in the formation of the vagina. On reaching the summit of the

urogenital sinus (*Urog*) they perforate into this space behind the septum (*S*) which, growing down, separates the bladder (*B*) and the urethra from the vagina, and thus forms the vesico-vaginal septum. In fig. 5 (2) the alternative method of development is shown, in which the posterior portion of the urogenital pouch is not formed, the septal down-growth (*S*) closing in the whole sinus except that which forms the base of the bladder and urethra. In this case the mesoblastic downgrowths from the Müllerian ducts have further to go to reach the surface behind the urethra. Fig. 5 (3) represents the normal result which occurs in both of these cases.

The absence of uterus or *ectopia* of the Müllerian ducts makes no difference to the fate of the urogenital sinus.

In fig. 6 (1) and (2) is seen the mode of formation of the lower end of the vagina which is frequently found in cases of absence of uterus. Fig. 6 (3) and (4) illustrates what occurs when this vaginal pouch is absent. It will be seen at once that the same conditions obtain in regard to the formation of a urogenito-vaginal pouch whether there are Müllerian downgrowths or not.

In regard to the origin of the hymen there are two views: First, that it is the ragged edge which marks the perforation into the urogenital sinus of the down-growing "vaginal cords" (Keith and others). Secondly, there are those who believe with Pozzi [11] that the hymen has purely a vulval origin, and arises in foetal life, apart from the vagina, around the vulval orifice of the urogenital sinus. My own observations are entirely in accord with Pozzi's view. As already related, the hymen was recognizable in this case, although there was no sign of a vagina nor of a urogenital pouch. Further, in the two other cases mentioned above, which I have had an opportunity of examining recently, there was in each case a well-marked hymen around the urogenital pouches which had not been perforated by a vagina, and I have also seen the so-called "imperforate hymen," in which the hymen was demonstrable stretched out on the bulging vagina, a condition to which attention was, I think, first called by Mathews Duncan.

The main conclusions, then, that I have come to, after a consideration of the above case and of three other cases of absence of the uterus and vagina which I have seen, are:—

(1) That uterus didelphys (and *ectopia* of the uterine bodies) is due to the absence of fusion of the Müllerian ducts, caused by the deficient action of the decussating subperitoneal muscular fibres which normally form the external coat of the uterus; and also, in part

(especially where *ectopia* coexists), by the deficient formation of utero-sacral muscular fibres. As a contributory factor a short genital mesentery plays an important part.

(2) The vagina is formed (a) by the downgrowth of mesoblastic tissue which perforates the summit of the vaginal portion of the urogenital sinus; or (b) by the perforation by this downgrowth of the superficial depression below the urethra.

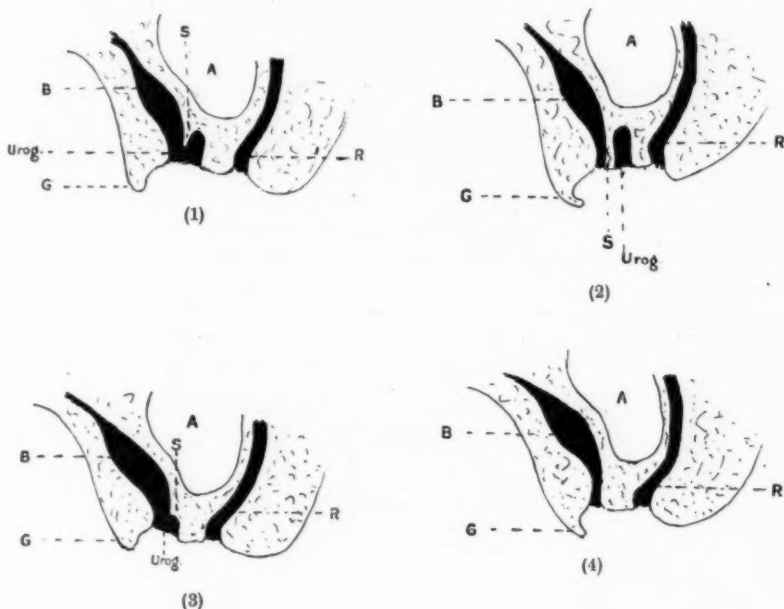


FIG. 6.

Fate of the urogenital sinus in the absence of the uterus and mesoblastic downgrowths to form the vagina, as in fig. 5. A, abdominal cavity; B, bladder; G, genital tubercle (clitoris); R, rectum; S, downgrowing vesico-vaginal septum; Urog., urogenital sinus, or pouch.

(3) The hymen is of vulval origin, as pointed out by Pozzi, and exists in the absence of a vagina, whether there is a urogenito-vaginal pouch or not.

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- [12] ROUX. "Vice de conformation par aberration inguinale de deux moitiés uterines," *Cong. français de Chir.*, Par., 1891, v, p. 497.
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DISCUSSION.

Dr. CUTHBERT LOCKYER thought the thanks of the Section were due to Dr. Blair Bell for his interesting demonstration of a rare congenital pelvic condition. He agreed with the speaker that complete uterus didelphys was rarely found in adult women, and also that hernia of the uterine body was of rare occurrence, especially as regards its displacement in the canal of Nuck. Dr. Lockyer drew attention to the fact that in compiling a bibliography on the subject of uterus didelphys, Dr. Blair Bell had omitted two cases published in the *Transactions of the Obstetrical Society of London*, 1906, xlviii, pp. 75-82. In one of these cases the right ovary, the right half of the bifid uterus, and the right Fallopian tube were all contained in a hernial sac of a woman aged 27. In the other case the left ovary and left half of the uterus were displaced, and

lay within the canal of Nuck, in a patient, aged 28. In Dr. Lockyer's opinion the mention of these cases in the bibliographical summary at the end of Dr. Bell's paper would have enhanced rather than have detracted from its value. Dr. Lockyer drew Dr. Blair Bell's attention to Robert Meyer's paper, "Zür Entstehung des doppelten Uterus,"¹ in which that author described a case of uterus didelphys in a foetus at the eighth month of life. In this case the two horns were laterally secured by abnormally hypertrophied round ligaments, which the author regarded as the primary congenital abnormality which had led to non-union of the two halves of the uterine body. The condition was uncomplicated by pelvic inflammation, and, owing to the tender age of the subject, it formed valuable information as to what can occur in the very early months of development. The cases published in the *Transactions of the Obstetrical Society of London*, already referred to, occurred in the hospital practice of Dr. Amand Routh and Mr. Waterhouse, and by the courtesy of these gentlemen Dr. Lockyer had examined and reported on the conditions found. In both instances Robert Meyer's view as to the causal relationship between congenital hypertrophy of the round ligaments and uterus didelphys seemed to be supported. Neither in Dr. Routh's specimen nor in that of Mr. Waterhouse were the structures which lay in the canal of Nuck at all altered or influenced by inflammatory adhesions and thickening of the hernial sac, as appeared to be the case in Dr. Bell's specimens. The result was that the material investigated by Dr. Bell was not so satisfactory for the purpose of drawing conclusions as to the state of the round ligaments. Dr. Blair Bell had said that "no round ligaments were developed." Dr. Lockyer submitted that as the two uterine bodies were adherent to the thickened sac by their anterior surfaces this contention could not be proved, and that, as far as the round ligaments were concerned, the specimens proved nothing. As there was nothing in the present case of Dr. Bell's which could be offered as proof that these ligaments were absent, Dr. Lockyer saw no reason to change his views on the causation of uterus didelphys.

Dr. AMAND ROUTH said that as Dr. Lockyer had alluded to his case of rudimentary uterus didelphys, where he had removed the pelvic generative organs for true epilepsy, he thought the Section would like to know that three years had elapsed since the operation, and that the woman, now aged 31, had entirely ceased having fits and had made great physical and intellectual progress.

Dr. BLAIR BELL, in reply to Dr. Lockyer, said he was well aware of the case that was under the care of Dr. Routh. There were, however, considerable differences between the case recorded in his (Dr. Blair Bell's) paper and the one mentioned. The latter was a case of hystero-epilepsy with a rudimentary

¹ *Zeitschr. f. Geburts., Stuttg.*, 1898, xxxviii, p. 16.

bicornute uterus. Unfortunately he had not had time in his paper to discuss the many cases of bicornute uteri, herniated or otherwise, which had been recorded. Since the question of epilepsy had been brought up, he would like to point out that a year ago he read a paper before the section on the relationship of the calcium metabolism to menstruation; in it he advised the use of calcium lactate in hystero-epilepsy. It was gratifying, therefore, to observe that alienists had, in the last few months, found that certain cases of epilepsy could be almost specifically treated with calcium lactate. Dr. Blair Bell thought that the fact that the round ligaments had been found to be hypertrophied in cases in which Müllerian ducts had failed to fuse supported rather than depreciated the value of his views. Excessive hypertrophy indicated that the round ligaments had been able to overcome the action of the decussating fibres by a process of hypertrophy beyond the normal. In this connexion it was pointed out that hypertrophy of the round ligaments was generally seen in the early stages of *prolapsus uteri*. What he wished to make clear in regard to the evidence of his own specimen was that with complete *ectopia* the round ligament was not present, except in so far as an ovarian ligament existed; and that the term *ectopia* implied the incorporation of the Müllerian derivatives with the peritoneum of the canal of Nuck. This, he thought, was an original developmental condition, and not due to inflammatory processes, as suggested by Dr. Lockyer.

Primary Chorionepithelioma of the Ovary.

By JOHN S. FAIRBAIRN, M.B.

THE case which I am bringing before the Section to-night as one of primary chorionepithelioma of the ovary came under my notice over two years ago. I have allowed this time to elapse before reporting it in order to be able to give something of the after-history of the patient, and perhaps I would have waited still longer had there not recently appeared two very similar cases from Professor Doederlein's Klinik, which has determined me not to delay further. Unfortunately, the greater part of the notes of the case have been lost, but the details of the patient's clinical history were filled in afterwards by questioning her again, and the specimen, with microscope slides, was preserved, and is shown this evening. I will begin with an account of the case.

The patient was a married woman, aged 25. She was married at the age of 20, and had had three children and one miscarriage between the first and second child. The last child was born in February, 1905, when there was some difficulty at the confinement, the placenta having to be removed by hand. Three weeks afterwards she went into the infirmary for "white leg." The child was nursed for twelve months, and, after weaning, menstruation recommenced. The first period was normal, the next one followed three weeks later, and from then there was a continuous slight loss nearly every day, so that the patient could keep no proper count of the occurrence of the periods. At no time was this loss excessive or such as to suggest to her that she might have had a miscarriage. This state of affairs continued till November, when the loss ceased after an attack of influenza with general pains. When she began to get about after this illness the pains became localized in the left side and lower part of the abdomen, and she suffered a good deal from sickness. About a fortnight before Christmas she noticed a tender lump in the left side of the abdomen, and the medical man whom she consulted advised her to come to the hospital. She was admitted to St. Thomas's on December 28, 1906. On examination a tender, hard, elastic swelling was found on the left side of the abdomen reaching up to the level of the umbilicus. On bimanual examination it did not appear to be connected with the uterus, and was thought to be an ovarian cyst with a twisted pedicle.

Operation January 3, 1907: On opening the abdomen the tumour was seen to be covered with a thin white capsule, through which showed the dark bluish-red colour of the tumour substance. It was very adherent, especially about the level of the pelvic brim, and burst during its separation, and allowed of the escape of material so like blood-clot that it at once suggested the possibility of an ectopic pregnancy. When isolated it was found to be a growth of the left ovary, but without any twisting of the pedicle. It was removed and the ovary of the other side also, partly because it was involved in the adhesions and partly as a prophylactic measure in view of the evidently malignant character of the primary growth.

The patient made a good recovery and has remained well up to the time when she was last seen, about five months ago. She has had complete amenorrhœa since the operation, and except for flushes and heats for a few months has had no abnormal symptoms. There has been no uterine hæmorrhage or discharge of any kind. The tumour is of about the size of a small cocoa-nut, measuring $4\frac{1}{2}$ in. by 3 in. It is ragged and torn owing to its having ruptured during removal. The surface is nodular and is covered with the stretched-out tunica albuginea of the ovary. The cut edge of the broad ligament can be seen, but no trace of the Fallopian tube is recognisable. On section it is seen to be made up of a deep red hæmorrhagic mass covered with a thin capsule of ovarian tissue. The deep red colour of the cut surface is interspersed with lighter areas, giving it a mottled appearance, and there are fibrous-like strands running through it.

Microscopically the greater part of the tumour is found to be made up of blood-clot, fibrin and necrotic tissue. The chorionepitheliomatous structure is seen best in portions taken from just under the capsule. The sections show the remains of the ovarian tissue with follicles and corpora albicantia, and between it and the main mass of clot and fibrin are syncytial masses with small polyhedral cells. The syncytium is especially well developed and forms on the surface of the blood-clot an irregular meshwork of vacuolated protoplasm. There are numerous nuclei of varying shapes, mainly oval or crescentic, and differing considerably in the amount of stain they have taken up. For the most part they have stained deeply. Small compressed masses of polyhedral cells are seen below the syncytium and running in between its strands. Here and there are isolated mononucleated cells. Among the masses of blood-clot are collections of inflammatory leucocytes, mainly of the polymorphonuclear type. In this cellular portion there is a large

amount of blood, partly clotted and partly free in sinuses between the bands of syncytium. No proper stroma or blood-vessels are present except in the ovarian tissue remaining on the surface of the growth. No traces of teratomatous elements were found in the portions examined.

The case is described as one of primary chorionepithelioma of ovary, and I will therefore begin by noting the points in justification of the



Section of growth ($\times 65$) showing chorionepitheliomatous structure. *A*, blood clot, fibrin and necrotic tissue; *B*, syncytium; *C*, small polyhedral cells; *D*, spaces containing blood.

title I have adopted. The naked-eye appearance of the tumour is such as to suggest its chorionepitheliomatous nature, though I will confess that at the time of its removal it did not strike me as such, and that it was not until I had received Dr. Dudgeon's report of the microscopic examination that I recognized how closely the hæmorrhagic structure of the body of the tumour corresponded to the ordinary chorionepithelioma.

Having begun the case from the clinical side, and never having had a thought of chorionepithelioma, it is not surprising that the pathological nature of an ovarian tumour of so unusual and unexpected a kind was overlooked in spite of its characteristic appearance. The moment I heard the report from the Clinical Laboratory, I could only wonder that no thought of chorionepithelioma had crossed my mind. The structure of the growth is made perfectly plain by a study of the microscope sections. The syncytial masses showing marked vacuolation form a large part of the cellular elements, and with the small polyhedral cells, mononuclear and multinuclear isolated cells, and the huge masses of blood-clot and necrotic tissue, present the typical histological characters of chorionepithelioma.

The other part of the title which has to be justified is the word "primary." Although the patient had been pregnant four times, there is no history to indicate anything of the nature of a cystic mole or of chorionepithelioma elsewhere. The uterus and vagina were apparently free, and the fact that no other growths have occurred, though over two years have elapsed since the operation, seems to be sufficient evidence that the growth was one solely and primarily affecting the left ovary. At the same time it is quite evident that the tumour cannot have been of anything like so malignant a nature as is usual with such growths, as it had extensive adhesions to the posterior abdominal wall at the level of the pelvic brim, and, being of a soft and friable nature, ruptured during removal, so that a certain amount of the clot-like material was extruded into the abdominal cavity, and yet no immediate recurrence took place. It is, however, well known that there are remarkable variations in the malignancy of these tumours. Teacher called attention to this in his valuable monograph on the subject, and mentions that Pick described his first case of primary disease in the vagina as "chorionepithelioma benignum." Among the favourable cases collected by Teacher, attention may be drawn to that of Cazin-Segond, in which there was a primary focus in the posterior wall of the uterus and a metastasis in the right ovary, and yet the patient was free from recurrence three years after operation. A full discussion of the malignancy and prognosis in tumours of this nature will be found in Teacher's paper. Several instances of primary chorionepithelioma of the ovary have been described, most of them having arisen in connexion with teratomata of that organ. Some years ago Pick collected seven, some cystic and some solid, which he very carefully investigated and described. Michel described a case as one of carcinoma of the ovary, with secondary chorionepitheliomatous

deposits in the abdominal cavity, which involved him in a lively controversy with Pick, who maintained that this case was similar to those described by him. Schmaus also recorded a case like that of Michel.

Two papers on primary chorionepithelioma of the ovary have appeared recently, and I have chiefly relied on them for information on the subject. These are an inaugural dissertation from Tübingen by Assmuth, and a paper in the *Archiv für Gynakologie*, 1908 (lxxxv, p. 415) by Iwase. Both these papers contain the same case from the Tübingen Klinik, but Iwase has a second from the Munich Frauenklinik. Both cases were operated on by Doederlein. Iwase considers that the ovary is by far the most unusual site for extra-uterine chorionepithelioma, and he and Assmuth are agreed in considering that the only case previously recorded which can be considered on all fours with theirs is one of Kleinhan's, which possibly may have arisen in a left-sided ovarian pregnancy.

In the first of these cases of Doederlein's the patient was a woman, aged 31, who had had two normal confinements, and complained of abdominal pain, especially in the right side, and of vaginal hæmorrhage. The last menstrual period had occurred the beginning of April, 1907, and at the end of May severe hæmorrhage had begun, which continued with short intermissions up to the time of her admission to the Klinik on July 17. Three tumours were found in the vaginal wall: the largest, the size of a hen's egg, was just under the urethra; the other two, the size of a hazel nut, were on the posterior wall. The uterus was normal, but to the right was a round movable tumour of the size of an apple. The diagnosis of extra-uterine pregnancy was made, the importance of the vaginal tumours being overlooked. On opening the abdomen a tumour of bluish-red colour was discovered involving the right ovary, of the size of two fists and free from adhesions. This was removed. The left appendages were normal. The vaginal tumours were also removed, the largest one under the urethra being too diffuse to permit of radical extirpation. Curettage was also done, but showed nothing beyond the usual "endometritis glandularis." The vaginal growths rapidly recurred, and the patient was discharged from hospital with signs of metastases in the lungs, and died within a month of the operation. There was no post-mortem examination. The section of the tumour showed numerous blood masses between which ran fibrous bands, thus giving it an appearance very like that of the placenta. The vaginal tumours showed a similar structure. Microscopic examination showed that the main

part of the tumour was made up of necrotic tissue and blood fibrin, among which were broken-down tumour cells and blood corpuscles. The structure of the new growth was best seen near the periphery, between the necrotic tissue and the remains of the ovarian tissue. It presented the usual syncytial meshwork with Langhans' cells. Serial sections through the tube gave no sign of tumour elements, and the uterine mucosa removed by curetting showed no evidence of decidual reaction. The nodules from the vagina had a similar structure to that of the main growth.

The second (Munich) case was that of a woman, aged 42, who was admitted to the University Frauenklinik on October 9, 1907, complaining of severe abdominal pain of six weeks' duration. She had had eleven pregnancies, one of them ending in an abortion at the third month, and in the last two labours the placenta had been adherent and was removed manually. Otherwise there was nothing unusual in her history. Menstruation had been quite regular up to June, 1907, and after then it had ceased. For three weeks before admission there had been a slight loss, amounting only to a few drops of blood. On examination the uterus was found to the right of the middle line and of the size of a goose's egg; on the left, connected with it and inseparable from it, was a hard tumour the size of a man's head. On October 15, and again on October 17, there was a slight loss of blood. Operation was performed on October 21 by Professor Doederlein. On opening the abdomen a bluish-black tumour was seen reaching up to the umbilicus. On section it exhibited hæmorrhagic characteristics, which suggested its being a chorionepithelioma following a tubal gestation; and therefore an extensive operation analogous to Wertheim's was done, and the uterus and its appendages, with the parametrial tissue and the upper third of the vagina, were freely removed. The patient was discharged on November 9. The tumour of the left ovary was of the size of a child's head, and round, nodular, and of a friable consistence. On section it showed a placenta-like structure. The tube appeared normal. The uterus was somewhat enlarged, but, except for an area on the posterior wall to which the ovarian tumour had been adherent, showed nothing abnormal. The portion of the tumour adherent to it did not involve the uterine wall. The right ovary contained a small metastatic nodule the size of a hazel-nut and of the same hæmorrhagic structure as the main tumour. The tube of this side also was quite normal. The microscopic examination was much the same as in the first case. The main part of the tumour was made up of

masses of blood-clot with necrotic debris, and the cellular portions were made up of syncytial elements and Langhans' cells. The metastasis in the right ovary showed the same structure as the primary tumour. Both tubes were free from tumour elements. The uterus was also free from tumour cells, and its mucosa showed no trace of decidual change.

In discussing his two cases Iwase considers the vaginal nodules as secondary metastatic growths in what is the favourite situation for such metastatic growths in the case of chorionepithelioma, but the nodule in the right ovary in the second case he considers as occurring in a very unusual site. In discussing the occurrence of chorionepithelioma outside the area of the implantation of the ovum he mentions three possible explanations. According to Schmorl we may have to do with a case of circumscribed chorionepithelioma, or cystic mole, which has been completely extruded from the uterus without leaving any trace in the uterus itself, but which, through transportation of the tumour elements, has given rise to a growth at the site where they have settled, so that it appears to be primary. Pick and others do not consider the previous occurrence of a circumscribed chorionepithelioma or cystic mole as essential, as they consider that the transportation of villi from the normal placenta is sufficient to explain the formation of the growth at the site of transplantation. The third view is that the tumour may have arisen as a metastasis from a teratoma with chorionepitheliomatous parts. In the case of ovarian chorionepithelioma there are two further possibilities. It may have arisen from an ovarian pregnancy or from a teratoma of the ovary as in the case of similar tumours in the testicle. Iwase considers that, although it is impossible to speak with certainty as to the presence of a teratoma, such is extremely unlikely, as in none of the many sections examined was any sarcomatous or other malignant degeneration found, nor were there any traces of unusual tissue elements. Again, with regard to the possibility of its arising directly from a pregnancy, very careful examination of the genital organs produced no evidence of any changes due to gestation, so that although menstruation was absent once in the first case and three times in the second case, and in both cases some irregular bleeding accompanied the abdominal pains, too much reliance must not be placed on these clinical symptoms in the absence of microscopical evidence of pregnancy.

The case of Kleinhans, which Assmuth and Iwase consider as the only one hitherto recorded like theirs, was a hæmorrhagic tumour of the left ovary, which histologically presented all the signs of

chorionepithelioma. It was partly surrounded by a capsule which contained ovarian tissue. The tubal mucosa was intact, but there were some small tumour nodules in the wall. The patient died shortly after the operation, and at the post-mortem metastases were found in the lungs and in the vagina. The uterus and the adnexa of the other side were free. Kleinhans was not satisfied that there had been a preceding ovarian pregnancy. There is not sufficient clinical history given to make the case of any value to me in this paper.

The two cases of Doederlein's are extremely interesting as forming a very close parallel with the one I have just recorded. They all occurred in multiparous women in the child-bearing period of life (ages 25 to 42), and without any history of a preceding cystic mole. There was some disturbance of the menstrual function, but abdominal pain was the marked symptom in them all. The tumours were similar in their characteristics, not large, of a dark bluish-red colour, and encapsuled in the remains of the ovary. The cross section had the same hæmorrhagic appearance, which was compared by Iwase to placenta. Microscopically they presented the usual characters of chorionepithelioma, and, judging from the descriptions of the two other cases, the tumour elements were best observed beneath the capsule and between it and the mass of blood-clot and necrotic tissue, as described in my case. In none of them was there any evidence of teratomatous structures or of antecedent gestation. In Doederlein's cases there were metastases, in the vagina in one case and in the ovary in the other, but none in my case. Rapid recurrence followed the operation in Doederlein's first case, and his second case is too recent to speak of definitely; in my case there is no recurrence up to the present.

These cases appear to me to be quite different from those recorded by Pick. Apart from the absence of positive evidence of an origin from teratomatous elements, attention may be directed to the following points of difference: Among Pick's cases were several in young girls, and altogether the age incidence was much younger than in the series quoted above. One patient was aged 9, and the others were aged 16, 17, 21, 24, 30, and 36 respectively. Except one (the patient aged 21) all were nulliparous women. Michel's case also was in a nullipara of 16. The extremely malignant character of the growth in Pick's cases is shown by the fact that of the six cases, in which a record of the result is given, four patients died within a few months of operation, one a year after, and one only is mentioned as being alive for over a year.

The definite evidence of teratomatous structures in these cases, the

more mixed nature of the tumour elements, the occurrence of the tumours in young girls before the child-bearing age and in multiparous women make it unlikely that the tumours under consideration in this paper are of a like origin, quite apart from the fact that no evidence of teratoma is forthcoming. Further than this, any discussion as to the origin of these growths of the ovary must be purely speculative. There is no evidence of a preceding ovarian pregnancy, and, considering the rarity of this variety of ectopic gestation, such an explanation would involve the assumption of the occurrence of a very rare sequel of pregnancy on the top of an extremely rare form of pregnancy. In the meantime I would accept Pick's view as to their possible origin from the transported villi of a normal placenta, for the patients in whom these growths occurred were multiparæ, and, curiously, in two of them manual removal of the placenta was practised in the labours preceding the commencement of symptoms.

Full references to the literature of the subject will be found in Teacher's monograph¹ and in the papers mentioned.

Report of Pathology Committee.—The committee agree that this tumour is a chorionepithelioma, and find no evidence of teratomatous structure.

Dr. VICTOR BONNEY was much interested in Dr. Fairbairn's specimen. Whilst it was impossible to exclude absolutely a gestational origin, it was much more likely that it belonged to the class of teratomatous chorionepitheliomata. The speaker had, in a paper published in the *Transactions of the Pathological Society of London* (lviii), collected all the recorded instances of this condition up to that time (1907). Dr. Fairbairn's specimen was comparable with that recorded by Michel, in which a dark-red tumour of the left ovary was removed from an undoubted virgin, aged 16, who subsequently died with chorionepitheliomatous metastases. Michel published the case as a carcinoma of the ovary of a chorionepitheliomatous type, but Pick subsequently criticised this conclusion and declared the tumour to be a teratomatous chorionepithelioma. Schmaus had reported a case which was probably of a similar nature, whilst Kleinhans' specimen, to which Dr. Fairbairn had referred, was published antecedent to Schlagenhauer's paper and before anything was known or thought of concerning teratomatous chorionepitheliomata. The two types of ovarian teratomatous chorionepitheliomata—namely, the pure growth like the present specimen and those recorded by Kleinhans and Michel, and the mixed teratomatous type like those of Pick, to which reference had been made, and of which the speaker had slides—were exactly paralleled by the testicular and the much

¹ *Journ. of Obstet. and Gyn. of the Brit. Emp., Lond., 1903, iv, p. 1.*

rarer extra-genital varieties of the disease. Thus, while Scott and Longcope had published a case of a pure chorionepithelioma of a retained testicle with metastases of a similar nature, yet the bulk of the recorded testicular growths were of the mixed type. In regard to the extra-genital variety of the disease, the unique specimen described by Ritchie was a perfect example of the mixed type, while he (Dr. Bonney) had recorded a case in which an elderly man died of a very large primary tumour of the great omentum with hepatic metastases, the structure of both of which was purely chorionepitheliomatous. Boström had published a similar case, while M. Askanazy had exhibited what was probably the most remarkable case of all—namely, a primary growth of pure nature arising in the pineal gland of a young man. He (Dr. Bonney) possessed slides of this case, which showed a most typical chorionepitheliomatous structure. These pure teratomatous chorionepitheliomata could arise in two possible ways: Firstly, from a cell seclusion representing all the layers of the blastocyst or the ectodermal layer only, and in which the elements of the trophic ectoderm had been produced in such excess as to obscure the other derivatives; secondly, from a pure seclusion from the trophic ectoderm, in which case the product of the subsequent growth of the seclusion would be chorionepitheliomatous tissue only. The first possibility was the more likely.

**Labour Obstructed by a Fibroid Tumour; Cæsarean Section;
Supravaginal Hysterectomy.**

By C. E. PURSLOW, M.D.

THE account of this case has been previously published (*Lancet*, 1909, i, p. 1598).

Obstetrical and Gynæcological Section.

July 8, 1909.

Dr. HERBERT SPENCER, President of the Section, in the Chair.

Cancer of the Cervix diagnosed early by the Family Doctor : Patient well ten years after High Amputation.

By HERBERT R. SPENCER, M.D.

ON March 22, 1899, I saw a widow lady, aged 62, in consultation with her medical attendant, the late Mr. Hugo Daniell, who had discovered a growth on the patient's cervix which he believed to be cancer, in the following circumstances: He had been attending the lady for an attack of influenza, and the patient casually remarked to him that she had a little leucorrhœal discharge. On questioning her he found that this discharge had been noticed for the last five or six months, was occasionally tinged with red, but had never contained obvious blood. There had been no pain. Mr. Daniell at once suggested that he should make a vaginal examination, to which the patient, who thought the discharge was of no consequence, demurred, but consented on the urgent representation of its importance by the medical attendant, who was the friend as well as doctor of the family.

I found the patient to be of magnificent physique, over 5 ft. 10 in. in height and weighing 16 st., the mother of two daughters, splendidly developed women nearly 6 ft. high, and of a son measuring 6 ft. 2 in. These children (who were not unusually large at birth) had been born naturally without any difficulty. The patient had also had two miscarriages. No operation had ever been performed on the uterus. Menstruation, which was never regular or excessive in amount, began at the age of 14 and ceased at the age of 50. There was no family

history of cancer or tumour, but the patient is the only survivor of twelve children, almost all the others having died of phthisis.

On the cervix was a lobulated growth (fig. 1), typically cancerous, breaking down and bleeding readily on examination. The uterus was movable and the ligaments were unaffected.

The patient was removed to a nursing home, and next day, March 23, 1899, I removed the cervix by the high amputation with the Pacquelin cautery. Douglas's pouch was opened rather freely and the hole was plugged with iodoform gauze. Owing to the vascularity of the parts I had to use five pairs of forceps to stop hæmorrhage. These were removed on the third day and the gauze a few days later. The specimen exhibited has the shape of a cone, the base of which is the portio



FIG. 1.

Squamous carcinoma of the cervix. Sketch of cervix (natural size) after being ten years in spirit. A wedge has been removed from the anterior lip.

vaginalis, which measures $1\frac{3}{4}$ in. by $1\frac{5}{8}$ in.; the truncated apex is in the region of the internal os at a height of $2\frac{1}{2}$ in. above the edge of the posterior lip. Surrounding the external os, but especially posteriorly and to the left, is a lobulated growth, shown in the figure. On slitting up the cervical canal, in addition to the masses of growth at the end of the portio, finer finger-shaped and club-shaped processes are seen to depend from the cervical canal as high up as 1 in. below the line of amputation; the upper inch is free from growth.

Examination of a microscopic section (fig. 2) from the situation indicated in the drawing shows that the growth is a typical squamous

carcinoma showing the formation of cell-nests, with abundant small round-cell infiltration of the stroma. The tissue of the cervix unaffected by the growth is unusually vascular. The specimen and the microscopic section are exhibited.

After the operation the recovery of the patient was uneventful. On April 16 the vaginal wound had almost closed, and the patient got up and left the home a few days later, feeling quite well. I have seen and heard from her on several occasions. She has never had the slightest pelvic pain or discomfort since the operation, and is in excellent health at the present time at the age of 72½. I examined her on June 24,



FIG. 2.

1909, more than ten years after the operation, and found the scar sound and the body of the uterus senile, freely movable without pain or tenderness; there was no prolapse of uterus, bladder, or rectum.

The case is an instance of the excellent results obtained by high amputation of the cervix with the cautery in early cases of squamous carcinoma of the portio vaginalis; but I publish it in memory of the deceased general practitioner, to whose insistence on an examination to ascertain the cause of the discharge the patient owes her life, and as an example for other general practitioners to imitate.

Two Uteri removed by Wertheim's Method for Cancer of the Cervix apparently only moderately advanced: Infiltrated Iliac Glands removed, only discovered in the course of the Operation.

By ARTHUR H. N. LEWERS, M.D.

DR. LEWERS said the specimens shown were from the twenty-ninth and thirtieth cases in which he had performed Wertheim's hysterectomy.

CASE XXIX.

C. P., a married woman, aged 50, was admitted into the London Hospital on May 11, 1909. She had had seven children, the last eleven years previously, and five miscarriages. The menopause had occurred at the age of 47, and she "saw nothing" for two years. Since April, 1908, she had complained of bleeding on coitus, and also pain in the left side and back on coitus. She had been admitted in September, 1908, and the cervix was dilated and curetting performed by Dr. Andrews, who at that time found no sign of malignancy. Dr. Lewers examined her under an anæsthetic on May 13, 1909. There was little or nothing seen abnormal on inspection of the vaginal portion, but on passing a curette into the cervical canal typical carcinomatous tissue was removed, chiefly from the anterior wall. After removing as much as possible with the curette, the cavity was cauterised. At this time under the anæsthetic a very careful examination was made; the uterus was found very fairly movable, and no enlarged glands could be felt.

On May 28 Wertheim's hysterectomy was performed. In the course of the operation two enlarged lymphatic glands were found on the right side between the external and internal iliac arteries, and closely apposed to the iliac vein; but the wall of the vein was not involved by the growth. On the left side one enlarged gland was found in a similar position. These glands were removed, and sections showed that they contained a secondary deposit similar to that found in the cervical canal, which was a columnar-celled tubular carcinoma. The patient made a good recovery.

CASE XXX.

E. B., a married woman, aged 42, was admitted into the London Hospital under his care on May 15, 1909. She had had two children,

the last six years previously, and no miscarriages. Menstruation had been regular till five months before admission. Since then it had occurred at intervals of five to eight weeks, and had not been excessive. For the four weeks prior to her admission she had been losing blood daily.

May 20: Examined under anæsthesia. The uterus was freely movable; the posterior lip of the cervix appeared normal, but the anterior lip was the seat of a malignant growth, which had spread slightly on to the vaginal wall in the left lateral fornix. No glands could be felt. The growth was curetted and cauterized. Sections of it subsequently showed it to be a squamous carcinoma.

June 4, 1909: Wertheim's hysterectomy was performed. One iliac gland was found enlarged on the left side, and was removed; no other enlarged glands could be found. Sections of the gland removed showed deposits of squamous carcinoma. In this case the body of the uterus was uniformly enlarged to a size equal to that of a uterus two months pregnant. The endometrium was polypoid and fleshy, and sections of it showed no evidence of malignancy. The patient made a good recovery.

REMARKS.

These two cases emphasize the point that carcinomatous infiltration may be present in the corresponding lymphatic glands in relatively early cases of carcinoma of the cervix, and the consequent superiority of any operation, such as Wertheim's, that admits of diseased glands being removed, if necessary, at the same time as the uterus. Case XXIX is interesting also because only eight months before the operation the patient had been in hospital and very thoroughly examined, the cervix even being dilated, without any evidence of malignant disease being detected. In this case certainly vaginal hysterectomy might have been performed. If this had been the treatment no benefit could have resulted, and the infiltrated glands would have necessarily remained undiscovered. Occasionally the diseased lymphatic glands can be felt on bimanual examination, especially when the patient is under anæsthesia, but in these two cases the glands could not be felt.

DISCUSSION.

Dr. DAUBER remarked that it should be borne in mind that carcinomata of the cervix were not all equally malignant, the squamous-celled fungating carcinoma of the vaginal portion of the cervix being of a much lower order of malignancy than the columnar-celled carcinoma arising in the glands

lining the cervical canal. In the squamous-celled variety growth generally exceeded ulceration, and both processes advanced relatively slowly, whereas in the columnar-celled carcinomata growth and ulceration proceeded almost *pari passu*, and both processes were extremely rapid. The so-called "cauliflower excrescence" might sometimes nearly fill the vagina before invading the vaginal walls or the parametrium to any appreciable extent, while infiltration of extra-uterine tissue and glandular infection occurred early in the columnar-celled variety. At least, such was his personal experience, and for this reason he thought it inadvisable to treat all cases by any one routine method. It was, he felt sure, within the experience of many members of the Section to have removed carcinomata of the cervix either by vaginal hysterectomy or by supravaginal amputation of the cervix, and to have had some of their patients survive for long periods and to be alive and well at the present day, although many years had elapsed since operation, as in the case reported by the President. He might mention one case of his own. Over fourteen years ago he had removed the cervix supravaginally in a patient with early carcinoma of the cervix. This patient was alive and well to-day and free from all apprehension of recurrence. It was somewhat the custom to question the diagnosis when a patient survived so long. He could only say that such eminent gynaecologists as Dr. Champneys, Dr. Galabin, and Mr. Bland-Sutton had, independently of each other, all considered the case one of early carcinoma at the time. A microscopical examination was made of the excised cervix by a skilled pathologist, and so typical of early carcinoma did Dr. Galabin apparently consider it that he asked him (Dr. Dauber) if he might retain the slides for his own use in class demonstration as an example of early carcinoma. To this he readily agreed, and possibly the sections were still in existence. Thus he thought this case might be fairly considered to have been one of carcinoma. He asked if any better result than in the President's and this case—and they did not stand alone—could be obtained even by Wertheim's method. To assume that nothing less than Wertheim's operation was adequate treatment in every operable case of cervical cancer was in his opinion too sweeping a generalization. He had a high regard for the operation, and thought it marked a great advance in dealing with certain cases; but it had come into vogue too recently for anyone to dogmatize about it at present. It would be *sub judice* for some years to come yet, in his opinion. But what he rather wished to suggest was that in an early case of cervical carcinoma a differential diagnosis should be made between the squamous-celled more or less sprouting carcinoma and the columnar-celled and more rapidly ulcerating kind, and that both varieties should not be treated alike as if belonging to the same category of malignancy. If belonging to the former and less malignant class, a supravaginal cervical amputation might be performed tentatively—a comparatively simple procedure—and the case closely watched subsequently; for, in his experience, recurrence—if recurrence took place—was invariably local, metastases without local recurrence being unknown to him in these cases. Then, should recurrence take place, it would be always possible to do a Wertheim's operation, the previous operation not prejudicing such a

procedure in the least. At present he was not quite prepared to accept the view of Wertheim's operation being the only right treatment in each and every operable case of cancer of the cervix without exception.

Dr. AMAND ROUTH had looked up his statistics for cases of carcinoma of the cervix treated by the old operation of supravaginal amputation of the cervix performed between 1888 and 1898. Out of the fifteen cases operated on only two are known to have survived two years. The first case, aged 28, was operated on in 1888 for what was declared to be adeno-epithelioma of the cervix. In 1899 this patient brought her younger sister with a similar sprouting cervical growth, which was declared to be a benign adenoma. It is possible, therefore, that the original growth was also benign. The other case was well seven and a half years after operation, but had had retention of menstrual blood as a result of stenosis cervicis following the use of the cautery at the operation.

Dr. RUSSELL ANDREWS said that in the earliest case of carcinoma of the cervix that he had seen, a squamous carcinoma in a young woman, he had removed the uterus by Wertheim's method, and found a carcinomatous gland 3 in. from the cervix. The growth was so early that it might have been considered to be a favourable case for high amputation or vaginal hysterectomy, but by neither of these methods could the gland have been removed. Dr. Andrews saw Dr. Lewers's first patient in September, 1908, and admitted her into hospital as being probably a case of carcinoma, but after dilatation under an anæsthetic he could find no evidence of malignant disease. When he saw her again in May, 1909, he could feel carcinomatous tissue high up in the cervical canal. The only way in which it could have been diagnosed in September would have been by scraping the interior of the cervical canal with a sharp spoon.

Dr. BLACKER thought the possible occurrence of obstructive dysmenorrhœa a very great objection to the performance of supravaginal amputation of the cervix, and this complication seemed to follow the use of the cautery more often than that of the knife. He would like to ask the President if this was not his experience. He himself had seen a most disastrous case of this kind. The patient, a lady with early carcinoma of the cervix, after supravaginal amputation developed very marked dysmenorrhœa. The pain was so severe that she consulted another gynæcologist, who recommended some operative interference. An attempt to dilate up the opening into the vault of the vagina failed, and it was decided to perform abdominal hysterectomy. This was accordingly done, but unfortunately the patient died of cardiac failure. In this instance the fact that he (Dr. Blacker) had performed supravaginal amputation of the cervix was undoubtedly the indirect cause of the patient's death.

Dr. CUTHBERT LOCKYER stated that in the thirty-four cases of carcinoma of the cervix operated upon by him there had been two early examples of the disease associated with malignant glands adherent to the iliac vessels. The glands were not diagnosable at the time of preliminary cauterization of the growth under anæsthesia one week before the carrying out of Wertheim's

hysterectomy. These cases provided a strong argument in favour of adopting Wertheim's technique for the early case as well as for the more advanced. Dr. Lewers's experience in respect of early glandular infection by cancer only bore out the observations of Professor Wertheim himself.

The PRESIDENT (Dr. Herbert Spencer) said it was with the object of showing the importance of early diagnosis by the general practitioner that he had published this case. He had had other cases similarly treated which remained free from recurrence. Of the first twenty-five cases of cancer of the cervix treated by him at University College Hospital (six by vaginal hysterectomy and nineteen by high amputation) no patient died from the operation, and at least five of the cases remained well for five years, three of them (cases of high amputation) for eleven years. With regard to Wertheim's operation, he regarded it as the best operation for cancer of the cervix in suitable cases; but that was not saying that vaginal operations should never be done. Early cases of squamous carcinoma of the portio could be safely treated by the vagina, and the extended abdominal operation was not suitable for elderly, feeble, or very fat patients. With regard to the presence of infected glands, it was well known that in early cases of squamous carcinoma of the portio the presence of cancerous glands was rare, and was declared by some observers not to be met with in elderly patients such as the case he published.

Dr. LEWERS (in reference to the President's specimen) said that he had had very good results in early cases of cancer of the cervix treated by the high amputation. In May, 1907, he had shown before the Obstetrical Society of London a specimen removed by that operation from a patient in March, 1887, the patient still remaining quite well and free from recurrence. At the present time, however, he had given up that operation. In patients in whom menstruation continued, it was often followed by a troublesome form of obstructive dysmenorrhœa. He believed that, at the present time, in patients who could stand a severe operation, Wertheim's hysterectomy was the proper treatment. In feeble patients, and in those over 60 years of age, he considered Wertheim's hysterectomy too severe a procedure; and for such cases, if the disease seemed limited to the uterus, he performed vaginal hysterectomy. The mortality of Wertheim's operation rapidly declined as the operator gained experience. In his last twenty cases he had only had two deaths.

Pregnancy in a Rudimentary Horn of a Bicornute Uterus.

By ABERNETHY WILLETT, M.D.

C. K., AGED 19, was admitted into St. Bartholomew's Hospital on May 5, 1908, suffering from intense abdominal pain and collapse. Menstruation was normal and regular till December, 1907, since when it had been absent. The patient, a primigravida, had been in perfect

health until 6 p.m. on May 5, when she was seized with sudden and intense abdominal pain, so severe as to cause her to faint when walking in the street. She was picked up in this condition by the police and brought direct to the hospital.

On admission she was found to be intensely anæmic, with all the symptoms and signs of severe internal hæmorrhage. Her abdomen was distended and very tender, with shifting dullness in both flanks. A centrally situated tumour rising 2 in. above the navel was found, in which foetal small parts could be distinguished. *Per vaginam* nothing abnormal was detected; no blood was found in the vagina. The patient died fifteen minutes after admission.

Post-mortem examination: All organs were anæmic, but otherwise healthy. The uterus is enlarged to about the size of a 2½ months' gestation (the specimen has shrunk a good deal in the process of hardening). The left appendages are normal. On the right side there is a gestation sac the upper part of which has gradually thinned and permitted the escape of the foetus in its membranes. On the inner aspect of the rent the placenta is seen, which in the recent state was covered with adherent blood-clot and was evidently the source of the hæmorrhage. The giving way of the sac appears to have been a gradual process, as a strand of omentum is seen adherent to the rent. The foetus is of about five months' gestation. From the relation of the round and ovarian ligaments to the gestation sac it is plain that the pregnancy is in a rudimentary horn of a bicornute uterus.

A microscopical section was taken from the isthmus of tissue joining the gestation sac to the uterus. No communication between the two could be detected.

DISCUSSION.

Dr. AMAND ROUTH was somewhat puzzled to know how conception could have occurred if the author's view of the specimen was right. The gestation sac was entirely unconnected with the vagina. Neither ovary appeared to show a corpus luteum; yet it was evident that conception must have taken place, either by the migration of a fertilized ovum from the left ovary across the pelvis and up the undeveloped right Fallopian tube into the immature uterine cornu, or else by the migration of the spermatozoa from the left tube across the pelvis to a ripened ovum from the right ovary.

Mr. ALBAN DORAN referred to his own case of rupture of a cornual sac, reported by himself in the ninth volume of the *Journal of Obstetrics and Gynæcology of the British Empire*, and described at full length by Dr. Cuthbert Lockyer in his monograph on "The Pathology of Cornual Gestation" in the

tenth volume of the same journal, because it illustrated two facts of clinical interest associated with cornual pregnancy—namely, the relation of the gravid cornual sac to the cervix as definable by palpation, and the occurrence of gestation in the normal cornu after removal of the sac. This patient was seized with abdominal pains three weeks after weaning her first child, then six months old. Dr. Lionel Smith defined a soft body in the middle line resembling a pregnant uterus, but he noted that the thinning of the supravaginal portion of the cervix was marked to an extreme degree. The patient had an attack of collapse, and Mr. Doran operated. He found the ruptured gravid (right) cornu attached by a thin band to the left cornu and cervix. It was this band which had felt like an abnormally thinned cervix when palpated. This operation was performed in September, 1904. The patient afterwards became pregnant three times. A child was born spontaneously at term in September, 1905, and another was delivered at term in March, 1907, the breech presenting. Lastly, the patient was delivered prematurely in October, 1908, of a living child with a deep constriction in the left leg caused by a band of adhesion. There was also oligohydramnion. The case was reported by Mr. Blackstone in the *Lancet* for March 13, 1909. Mr. Doran referred to another instance of uterus unicornis where the patient, after being twice pregnant, developed a fibroid in the rudimentary cornu, which he removed. Two years after the operation the patient was delivered at term of a fine, healthy male child.

A Case of Sarcoma of the Uterus.

By EARDLEY HOLLAND, M.D.

(For JOHN PHILLIPS, M.D.)

THE patient, aged 53, had had eight children and three miscarriages. She was admitted into King's College Hospital in September, 1908, under the care of Dr. John Phillips. To within eighteen months of her admission she was quite well. The first symptom to appear was rather profuse uterine hæmorrhage, alternating with an offensive discharge. In March, 1908, she passed a "fibrous tumour" from the uterus, and was confined to bed for ten weeks. From an examination under an anæsthetic at this time other "fibrous tumours" of the uterus were diagnosed by her medical attendant. In June of the same year the hæmorrhage became more profuse, and she underwent an operation for removal of a "fibroid polypus": no pathological examination of this polypus was deemed necessary. Subsequent to this the hæmorrhage, which for a time had ceased, again made its appearance, and she passed another tumour "as large as a duck's egg." Latterly the discharge has not contained much blood, but has been most offensive. She has had no

other illnesses worthy of mention, and in her family history there is nothing of note.

On her admission into King's College Hospital she was distinctly weak and ill. Her temperature was intermittent, and showed daily variations between 98° F. and 103° F. There was an offensive blood-stained discharge from the uterus. At this time she was examined under an anæsthetic by Dr. Hugh Playfair, who made the following notes: "There is a hard, rounded, fairly movable swelling rising out of the pelvis and reaching 2½ in. above the symphysis pubis. The cervical canal is widely dilated and funnel-shaped, the wide end of the funnel being upwards. This expansion is due to the presence of one large polypus within the uterus, the lower end of which projects into the cervical canal, and also to some other flat mushroom-shaped growths projecting from the endometrium. Growing from the front of the body of the uterus and projecting to the right is a mass which is evidently continuous with the uterus, moving with it and projecting above the pelvic brim to the right of the middle line. The uterus as a whole is movable in all directions. The sound passes 4½ in., and its withdrawal is followed by bright hæmorrhage." Dr. Playfair removed some of the polypoid growths for microscopical examination. They proved to be sarcoma.

Shortly after this Dr. Phillips operated by the abdominal route and removed the whole uterus and upper part of the vagina, together with the broad ligaments and their contents as widely as possible. Her subsequent progress whilst in hospital was uneventful. The wound healed by first intention, and she left in apparently good health twenty-eight days after the operation.

Her present medical attendant, in answer to an inquiry, has written: "The patient died last June (1909). She had recurrences in the pelvic glands and deposits in the liver, and also in the lungs, producing hæmoptysis. She kept to her work (schoolmistress) up to a month of her death."

The specimen shown consists of a considerably enlarged uterus, together with the broad ligaments, tubes, and ovaries. The uterus measures 4½ in. in length, 3½ in. from side to side in the widest part, and 3 in. from before backwards. The enlargement is fairly regular in all directions, but there is especially a bulging forwards of the whole anterior wall and irregular enlargements at each cornu, due to the presence of nodules between the layers of that part of the broad ligaments adjacent to the uterus. The Fallopian tubes pass above and

behind the masses in the broad ligaments; the ovaries are large, but otherwise unchanged. The uterus has been bisected antero-posteriorly; its cavity is much enlarged and filled with a polypoid mass composed of soft, yellowish-white tissue springing from nearly the whole extent of the anterior wall (fig. 1). This white tissue infiltrates for a considerable

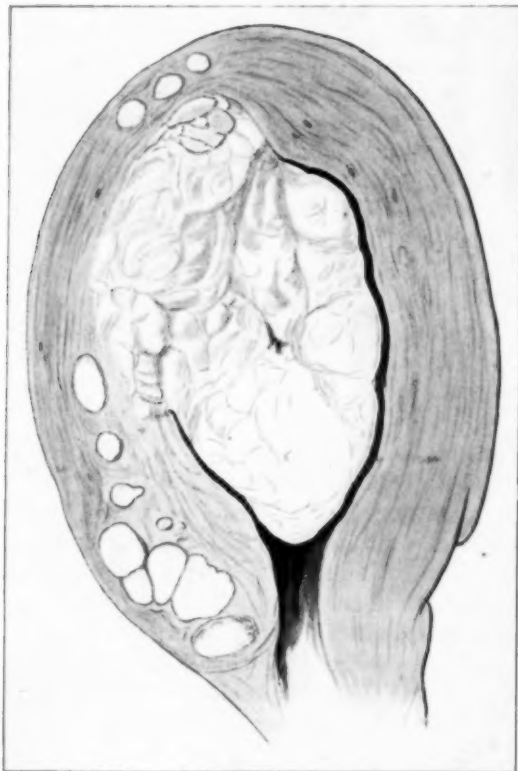


FIG. 1.

Sarcoma of the uterus.

distance the anterior wall, which is in consequence much increased in thickness. At first this infiltration is fairly general, but as the peritoneal surface is approached it takes the form of clusters of white nodules, some of which are tinged yellow; nearer still to the peritoneal surface the



FIG. 2.

Section through the intra-uterine growth, showing degeneration of stroma and giant-cells ($\times 300$).



FIG. 3.

Section through broad ligament, showing plugs of sarcoma filling the blood-vessels ($\times 5$).

nodules become smaller and more isolated amongst healthy uterine tissue. The posterior wall is free, and the peritoneal surface of the uterus is intact everywhere. The irregular masses between the layers of the broad ligament at the uterine cornua are due to the same white tissue, which has wormed its way into the blood-vessels, and has filled them to a very considerable extent. The Fallopian tubes are unaltered. This white substance proves on microscopical examination to be sarcoma. Sections were made from different parts—from the main polypoid growth, from the whole thickness of the anterior uterine wall in several places, from the broad ligaments, and from the ovary.

The sarcoma is of the irregular large-celled variety, round and spindle cells being present in varying proportions in different places. Very striking is the presence of large multi-nucleated cells. In certain areas the stroma has undergone some form of degeneration, apparently myxomatous; this appears as areas of homogeneous, faintly-staining stroma, containing very few cells. It is in these areas that the large multi-nucleated cells are especially abundant. The growth in the blood-vessels of the broad ligaments is histologically identical with the main growth. The ovaries are free from sarcoma. No trace of fibro-myomatous tumours was found anywhere.

The case is one of diffuse sarcoma of the uterus, probably arising from the interglandular connective tissue. The origin from sarcomatous degeneration of a fibro-myoma may be excluded; at all events, no trace of a fibro-myoma, however small, could be detected by a systematic examination. It is unfortunate that no pathological report on the "fibrous tumours" passed from the uterus is available. It is highly probable that these tumours were not "fibrous" but sarcomatous.

Report of Pathology Committee.—"The tumour is a mixed-celled sarcoma of the uterus."

DISCUSSION.

Dr. RUSSELL ANDREWS said that he had seen a somewhat similar case, in which two polypi, said to be fibroid polypi, were removed from a patient, there being an interval of a year between the two operations. The second polypus was found to be sarcomatous; the first, although very cellular, was pronounced innocent by a pathologist. The uterus was removed and found to contain a large sarcomatous mass bulging into the cavity.

Dr. LOCKYER said he was particularly interested in the polypoidal nature of the sarcomatous growth. He (Dr. Lockyer) had recorded a case, by the kind permission of Dr. Amand Routh, in which four or five polypi had been removed. The last polypoid growth was removed by Dr. Routh in the Charing Cross

Hospital, and Dr. Lockyer, after examination, reported it as a very necrotic and hæmorrhagic fibroid polypus. However, soon after the removal of this growth the uterus rapidly enlarged and was removed by panhysterectomy, when it was found to contain a mixed giant-celled sarcoma with a secondary deposit on its peritoneal coat. At the time of showing the specimen the patient was alive and well, but she had since died—*i.e.*, within a year of the operation—with metastatic growths all over the body. The obvious moral was that every case of so-called fibrous polypus should be microscopically examined, and doubtful cases kept under observation in the expectancy of recurrence.

A Case of Non-ovarian Pelvic Dermoid Tumour.

By FRANCES IVENS, M.S.

I venture to bring this case forward, as I think it is one of some rarity. Dermoid tumours originating in the pelvic connective tissue, non-ovarian in origin, are apparently of infrequent occurrence, and I have only been able to find twenty-nine cases reported.

As far back as 1859 Birkett [1], in *Guy's Hospital Reports*, described a dermoid cyst, the size of a walnut, situated in the connective tissue between the rectum and sacrum. A later case occurring in this country was reported by Page [7], of Newcastle, in 1891—*viz.*, "A large extraperitoneal dermoid cyst successfully removed by an incision across the perineum, midway between the anus and coccyx." Emmet [4], in 1884, brought before the Gynæcological Society of New York an instance of a dermoid tumour developing in the connective tissue of the left fornix, which he diagnosed as an ovarian tumour and removed by laparotomy. Emmet mentioned that it was the only case of the kind which had come under his notice. In 1906 Dr. Karl Reinecke [8] added to the cases already described and collected by Sänger [10], Rosthorn [9], and Skütsch [11], twenty-four in number, four others reported by Von Abel [13], Gottschalk [6], Steffek [12], and Bröse [2], and one occurring in his own practice. Reinecke pointed out the necessity for distinguishing ovarian dermoids closely connected by inflammatory adhesions with the pelvic connective tissue from those arising primarily beneath the peritoneum, ætiologically distinct, and entirely unconnected with the ovary. Reinecke classifies these twenty-nine cases into seven groups: (1) seven situated between the rectum and sacrum; (2) five in the connective tissue of the left fornix; (3) four in the connective tissue of the right fornix; (4) five behind the rectum

but extending into the left fornix; (5) two behind the rectum but extending into the right fornix; (6) three on the upper surface of the posterior vaginal septum; (7) three in the connective tissue of the broad ligament. In commenting on the extreme rarity of these tumours, Gebhard [5] mentions their predilection for the left half of the pelvis, and points out their tendency to be of somewhat simpler structure than the highly complex ovarian dermoids, hair and teeth being of infrequent occurrence. All the recorded cases occurred in women, with one exception, where de Quervain [3] first incised and then removed a large retroperitoneal dermoid from a man aged 58.

The tumours have been of varying size, occasionally forming large abdominal swellings, often not giving symptoms until after puberty. Their origin is uncertain. The tumours belong to the simpler varieties of dermoids, and do not resemble in structure the sacro-coccygeal tumour, but probably arise from the post-anal gut. That they are unconnected with the ovary is fully proved. Symptoms have been chiefly caused by pressure on the adjacent pelvic organs, giving painful defæcation and difficult micturition; pain on sitting has been noticed, and occasionally dysmenorrhœa and menorrhagia; repeated attacks of pelvic inflammation have usually occurred. The diagnosis has not been clear, and in some cases has only been determined by exploratory puncture or laparotomy. In the treatment, simple incision with drainage has been found to be unsatisfactory. In one case the vagina was incised eight times for what was believed to be a pelvic abscess, a vaginal fistula remaining. The cyst was finally extirpated by Bröse. Complete removal is the only method likely to be successful, and to effect this various routes have been tried. Sängner was in favour of a perineal incision, and Page [7] made an incision between the anus and coccyx. For a tumour situated above the recto-vaginal septum Reinecke [8] performed a posterior colpotomy. Emmet adopted the abdominal route. In a case recorded by Von Biernacki [11], associated with pregnancy and difficult labour, the tumour was punctured through the rectum, the patient recovering after a prolonged illness.

The following case occurred in a young woman, C. E., aged 26, married three years, nullipara. Menstruation was regular every month, not excessive, and lasted three to five days. There was no dysmenorrhœa or intermenstrual discharge. The patient, who came to the Liverpool Stanley Hospital complaining of severe pelvic pain, was sent on to the Gynæcological Department by Dr. Owen, and I first saw her on March 28, 1908. She was a healthy-looking woman, but was

evidently suffering acutely. She could not comfortably sit down, and said the pain was worse on defæcation. No history of a previous attack was given, but on close inquiry later I ascertained that for seven years similar attacks had occurred at intervals, associated with painful defæcation and rectal discharge. She had been told at several hospitals that she had a fistula, and three and a half years before had been an inmate of Mill Road Infirmary for a fortnight. Dr. Nathan Raw kindly supplied me with the following report of the patient, then unmarried. The history was given that she had had pain, more or less severe, at intervals, for three years before admission. When she came into the hospital she complained of pain in the rectum, and said she had passed a good deal of matter the day before. Examination at that time appears to have been negative, and, as she seemed to be quite well and there were no symptoms, nothing was done.

On examining the patient there was tenderness in the left iliac fossa. A bimanual examination, owing to the muscular rigidity and the pain caused, was necessarily rather incomplete, but determined the presence of a fixed fluctuating swelling, the size of a goose egg, high up in the left fornix, to the left of, and in front of, the rectum. The uterus was small, retroverted, and matted up with the appendages. I believed the case to be one of pyosalpinx, or ovarian abscess, possibly of tuberculous origin, and advised the patient to come into hospital, which she was very ready to do. While waiting a few days for a bed the district nurse reported that the patient had passed some thick matter by the rectum, rather like condensed milk, and was feeling easier.

On April 6 I performed laparotomy, making no attempt to reach the swelling by the vagina, as it was high up, and the pouch of Douglas was occupied by the retroverted uterus and adherent appendages. I found the swelling lay entirely behind the peritoneum, which was stretched smoothly over a cyst lying partly in front and to the left of the rectum. It was quite unconnected with the uterus or appendages. I incised the peritoneum and enucleated the tumour from its bed; it shelled out easily, without rupture, as it had a thick wall. There was very slight venous oozing. No opening into the rectum could be seen, though there was a small, dark, softened area in the tumour wall, where it had been in close contact. The cellular-tissue space was mopped out with warm saline solution and the peritoneal edges brought together with a few catgut stitches. I closed the abdomen without drainage. This I afterwards regretted when, on closer examination of the tumour later, I found that on slight pressure thick, buttery material of an apple-green

colour and intensely offensive odour exuded from the softened area in the wall. I felt that the nurse's history was probably correct, that the tumour had partly discharged into the rectum, and had in this manner become infected.

The specimen removed is a thick-walled, single-chambered cyst, partially septate. At one spot the wall is thinner. Microscopically, the wall shows an outer layer of dense, fibrous tissue, lined by granulation tissue, from which all traces of epithelium have disappeared. The cyst contained greasy material, which on cooling became of the consistency of butter, of a bright green colour similar to that produced by *Bacillus pyocyaneus*. Microscopically, cholesterin crystals and debris were seen. Cultures were sterile, and no organisms could be seen in coverslip preparations made from the cyst contents.

After operation the patient was unusually restless, and Mr. Sanderson (the house surgeon) gave a small dose of morphia. The following day the pulse was rapid and feeble, and vomiting continued until, on the third evening, the patient became very ill. The pulse, 160, was almost imperceptible, and she lay in a semi-comatose condition. There was no abdominal pain or distension, and the temperature was normal. The symptoms suggested a toxæmia. After a dose of calomel had been given, two pints of saline were administered intravenously, and a little improvement followed. Diarrhœa began the following morning; the stools, first blood-stained, became bright green in colour. Several pints of saline were injected into the cellular tissue beneath the breasts with marked benefit. Stimulants were given and the saline repeated. The patient gradually improved, the diarrhœa passing off in a few days and the wound healing by first intention. She left the hospital within three weeks, and when I saw her some months later was in very good health and free from pain.

I think the chief points of interest in this case are its unusual nature and the difficulty of diagnosis. With regard to the treatment adopted, although the patient recovered, she had a very narrow escape. Had I realized earlier that the contents of the cyst were infected I should have taken the precaution of draining the cellular tissue. That the tumour was of dermoid nature is, I think, probable from the character of its contents, as I know of no other tumour which would contain liquid grease. It is not unusual in inflamed dermoids for all traces of the original epithelial lining to disappear.

Report of Pathology Committee.—"The sections do not contain any element indicating that the cyst is of dermoid origin. The cyst-wall

appears to be made up of a thick layer of connective tissue undergoing inflammatory change."

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- [13] VON ABEL. *Berl. klin. Wochenschr.*, 1893, xxx, p. 47.

DISCUSSION.

Dr. HERMAN had, in a clinical lecture published in the *Clinical Journal* of August 22, 1900, p. 277, recorded a case of a pelvic dermoid, not ovarian, which suppurated while the patient was recovering from an uncomplicated ovariectomy. In a paper on "The Suppuration and Discharge into Mucous Cavities of Pelvic Dermoid Cysts," published in vol. xxvii of the *Transactions of the Obstetrical Society of London*, 1885, p. 254, he had collected a number of cases, some of which, he thought, were certainly not ovarian.

Mr. ALBAN DORAN desired to know Miss Ivens' opinion about the origin of the dermoid. It was not probable that the dermoid elements arose from displaced ovarian tissue, but it seemed very likely to be the homologue of the dermoid cysts occasionally found in the abdomen of men, as in Ord's case recorded in vol. lxiii of the *Medico-Chirurgical Transactions*, where the tumour was closely related to the bladder and rectum.

Dr. CHAMPNEYS said that he had seen one similar case. The tumour lay between Douglas's pouch and the sacrum, was about the size of an orange, and had no apparent connection with either ovary. It came into notice first during labour, caused considerable obstruction, and the patient died of sepsis.

The PRESIDENT (Dr. Herbert Spencer) asked whether it was possible that the tumour was a degenerated fibroid. He had met with a case in which he enucleated a tumour *per vaginam* from Douglas's pouch. The pultaceous contents led him to think it was a dermoid. He had to open the abdomen for oozing and found that it was a fibroid which had grown from the back of the lower segment. Examination of the tumour with the microscope showed it to be a cystic fibromyoma. He had several times observed the solidification of the contents of degenerated fibroids (from clotting). This, he thought, might be mistaken for the setting of the liquid fat of a dermoid.

Miss IVENS regretted that she had not seen the paper by Dr. Herman "On the Suppuration and Discharge into Mucous Cavities of Dermoid Cysts of the Pelvis," and mentioned that her communication referred entirely to retro-peritoneal dermoids. In answer to Mr. Doran's question respecting the origin of the cyst, Miss Ivens said she believed it to be derived from foetal remnants, probably the post-anal gut or projection of the hind-gut behind the proctodeal depression. Replying to the President's question as to the presence of hair in the cyst, she said that none could be seen. The diagnosis rested on the greasy character of the contents, which, though liquid on removal, solidified to the consistency of butter on cooling. After repeated attacks of suppuration, extending over a period of seven years, it was unlikely that the original lining of the cyst could be demonstrated.

President's Valedictory Address.

By HERBERT SPENCER, M.D.

IT is a custom honoured by long observance that on quitting office the President should deliver a valedictory address. Like all old customs, it no doubt has its value, though I am at the present moment the one who least appreciates it; but, as your Council has expressed a wish that it should be continued, I propose to give you to-night a short account of the work accomplished in the two years during which I have had the honour of presiding over your meetings. In former times it was usual to commence the address with a full account of those who had died during the presidency. The Council of the Royal Society of Medicine has, however, decided not to publish obituary notices of deceased members, but to confine the *Proceedings* to scientific matter. One reason for this decision will be seen at once in the fact that a Fellow or Member may belong to many Sections and, if each Section were to publish an obituary notice, the size of the volumes would be seriously increased. At the same time it is to me a matter of personal regret that this decision has been necessary, for some of the most valuable contributions to the history and biography of medicine have been originally made to the transactions of learned societies, and I look forward to the time when a historical section shall form part of the Royal Society of Medicine and undertake the task of recording the life-history of the most distinguished of its deceased members.

Since the formation of the Section death has removed twenty-one members from our midst, amongst whom Dr. Barnes, Dr. Cullingworth, and Dr. Horrocks have held the position of President of the Obstetrical Society of London, and one, Dr. C. H. F. Routh, was a founder of the Obstetrical Society and a President and Honorary Fellow of the British Gynæcological Society. Dr. Robert Barnes died at the age of 89. He will always be remembered by obstetricians for his work on "Obstetrical Operations," for his vigorous intellect and personality, for his independent spirit, and for the blow he struck in defence of the dignity of midwifery at a time when it was treated with less respect than it is to-day. Dr. C. H. F. Routh died at the advanced age of 87. He conferred a benefit for all time on British obstetrics as the introducer into this country of the great work of Semmelweis on "Puerperal Fever." Dr. Peter Horrocks died at the early age of 55, carried off by that disease which he and all members of this Section have striven, and are striving, to cure. Dr. C. J. Cullingworth died at the age of 66. A full account of his life's work has appeared in the *Journal of Obstetrics and Gynæcology of the British Empire*, but I cannot let the occasion pass without alluding to the work he did not only for obstetrics and gynæcology in their scientific aspects, but also for the Obstetrical Society of London and for this Section since the union of the two Societies. Always the soul of honesty and honour, always devoted to the public good, he spent much time in public work of which no record is kept and in little acts of kindness of which your President has been the recipient on many occasions. He maintained his interest in the work of the Section up till the end, frequently took part in our discussions, and added greatly to their value by the communications which his large experience of operative work allowed him to make. As President, I could wish that more of the senior members of our Section would contribute to our discussions from their experience in the same frank manner as Dr. Cullingworth did.

Altogether our Section has lost twenty-one members by death, and twenty-five members have been elected. The Section now numbers 724 members and is the largest Section in the Royal Society of Medicine. Since its formation 126 contributions have been made to its *Proceedings*, in the form of papers or short communications read and specimens exhibited.

I will follow the example of some of my predecessors, and arrange the contributions according to subject-matter rather than chronologically. After each writer's name is added the volume and page at which the

communication appears, so that this address will form a subject-index to the work of the Section, and may (in this respect) prove of some use to members, especially as the index at the end of the volumes of the *Proceedings* is not separate from that of the other Sections. And, first, I will deal with the subjects of obstetrical interest.

CÆSAREAN HYSTERECTOMY.

Four communications have been brought before the Section dealing with Cæsarean hysterectomy, or the removal of the uterus after Cæsarean section. Dr. Amand Routh (i, 1) read the first of these, which described a case in which the author had successfully performed the operation on account of traumatic atresia of the vagina. The paper was accompanied by a table of thirty cases in which the operation had been performed for stenosis, and of two cases in which conservative Cæsarean section had been performed; this table will prove of much value to future writers on the subject.

Dr. Kedarnath Das (i, 188) sent a contribution from India on three cases in which the operation had been performed for stenosis of the vagina; two of the mothers and two of the children survived. In discussing the paper Dr. Amand Routh stated that the mortality of Cæsarean hysterectomy for vaginal atresia was 21 per cent.

Your President (ii, 74) described a specimen which had been removed by total abdominal hysterectomy following Cæsarean section. The uterus was retroflexed to a right angle by a fibroid adherent in Douglas's pouch. The child was dead before the operation; the mother recovered. Attention was drawn to the thinning of the anterior wall of the uterus in these cases, and to the necessity for interference at an early stage of labour.

Dr. C. E. Purslow (ii, 334) showed a somewhat similar specimen successfully treated by supravaginal amputation; but, as the case had previously been published, the title only appears in the *Proceedings*.

PREGNANCY COMPLICATED BY FIBROIDS.

In addition to the two cases just mentioned, the Section has had brought before it ten communications dealing with fibroids complicating pregnancy. Mr. Bland-Sutton (i, 17) showed a uterus pregnant four months with a cervical fibroid in a state of red degeneration, and read a note on a case of red degeneration associated with *Staphylococcus pyogenes aureus* (ii, 300), and Dr. Purslow (i, 42) a similar uterus at the third month.

Dr. Dauber (i, 132 ; ii, 101) showed two uteri at the third and fourth months of pregnancy. Dr. F. J. McCann (i, 315) showed a uterus, pregnant $4\frac{1}{2}$ months, with a cervical fibroid in a state of red degeneration, from a patient aged 25. All these cases were operated on by supravaginal hysterectomy and recovered.

Dr. Herbert Williamson (i, 73) showed a $17\frac{1}{2}$ lb. fibroid enucleated from the uterus of a patient in the seventh lunar month of pregnancy, and Dr. W. C. Swayne (i, 129) a fibroid enucleated at the fourth month of pregnancy. In both of these cases premature labour followed. Dr. Swayne, at the same time, read notes of a case in which he had enucleated a fibroid at the fifth month ; the pregnancy continued after the operation for at least two months, when the patient was lost sight of.

Dr. Fairbairn (ii, 103) described a very remarkable case in which pregnancy continued and a living child was born at term after a fibroid (1.15 kilo in weight) and a large part of the anterior wall of the uterus had been removed at the third month of pregnancy. All these cases of myomectomy recovered.

A rare, and as far as I know unique, danger of subperitoneal fibroids was illustrated by a case described by Mr. W. D. Spanton (ii, 87) in which intestinal obstruction occurred from nipping of intestine in the sulcus between two subperitoneal tumours. The intestinal obstruction had existed for eight days, and peritonitis was present at the time of operation, so it is not surprising that the case terminated fatally.

These records express the views, sometimes divergent, of various authorities upon the dangers and the mode of treatment of pregnancy complicated by fibroids. A study of them, and of the discussions to which they gave rise, will enable a sound judgment to be formed as to the best treatment to adopt in the interest of the mother and child. The records and discussion show that in this country and, as pointed out by our President-elect, in France and Germany obstetricians are generally in favour of non-interference in the great majority of cases of pregnancy complicated by fibroids ; that the conservative operation of myomectomy may be performed with little risk to the mother, but with considerable risk to the pregnancy, and should therefore only be undertaken for definite indications ; that an endeavour should be made, when operation is indicated, to perform it at a time when there is a chance of survival of the child, and that all cases of pregnancy complicated by fibroids need careful supervision in order that accidents, which sometimes ensue, may be promptly dealt with.

PUBIOTOMY.

Dr. T. Wilson (i, 19) read a paper on pubiotomy, with notes of an illustrative case, in which the operation had been successfully performed on a patient whose diagonal conjugate measured $4\frac{3}{8}$ in., who had had five children at term delivered alive with forceps. A good account of the operation was given and the author expressed himself in its favour. From the discussion it appeared that the operation had not been performed in London, and only on ten occasions in this country. Attention was drawn to some dangers and drawbacks of the operation, and to the advantages of the alternative operations of induction of labour and Cæsarean section.

DIFFICULT LABOUR.

Dr. H. Russell Andrews and Dr. R. Drummond Maxwell (i, 138) described a case in which it was found to be impossible to deliver a dead child through a fairly roomy pelvis even after cephalotripsy had been performed and a large variety of instruments had been employed during an hour and a half. The child was removed by Cæsarean section and total hysterectomy was then performed. The patient succumbed in twenty-four hours.

The authors discussed the cause of the difficulty, which lay partly in the macerated condition of the large foetus and partly in the close application of the uterus to the trunk. The question was asked whether it would not have been better to leave the case to natural delivery after evisceration of the foetus. A sympathetic discussion followed the reading of the paper.

Dr. J. Evan Spicer (ii, 1) read a paper on "Dystocia due to Distension of the Urinary Bladder in the Foetus," and gave a careful description of the foetus, an excellent bibliography, and discussed the question of the secretion of urine during foetal life.

ECTOPIC PREGNANCY.

The subject of ectopic pregnancy has been brought before the Section on eight occasions.

Dr. J. S. Fairbairn (i, 31) described a case in which death followed rupture of an early tubal gestation within a few hours.

Dr. James Oliver (i, 68) showed a tube which had been pregnant, the ovary being transformed into two thin-walled cysts.

Dr. Lockyer (i, 248) showed a specimen of pregnancy in the wall of a tubal pus sac.

Dr. Munro Kerr (i, 268) described a case of ovarian pregnancy associated with intra-uterine pregnancy. The case was well described and illustrated, and adds another to the list of instances of this rare form of ectopic pregnancy, in the recognition of which British gynæcologists have taken a large share.

Dr. Macnaughton-Jones (ii, 91) described a very early case of tubal pregnancy.

Mr. Herbert Paterson (ii, 117) described a case operated on during the sixth month of pregnancy, and Mr. Alban Doran (ii, 120) one operated on at the end of the fifth month of pregnancy. In both these cases the foetus was alive at the time of operation, and in both the mothers survived.

Dr. H. Russell Andrews (ii, 228) showed a specimen of twin pregnancy in a Fallopian tube, of which only twenty-one cases had been previously recorded.

ECLAMPSIA.

Dr. N. C. Carver and Dr. J. S. Fairbairn (i, 79) read a paper on "Hæmorrhage into the Pons Varolii as the Immediate Cause of Death in Eclampsia," with illustrative cases and a special list of references. Amongst those discussing the paper no one seemed to have seen a case of pontine hæmorrhage, though several speakers had met with hæmorrhage into other portions of the brain. Dr. Lionel Smith mentioned three cases, in one of which he had been able to make observations on the heart sounds of the foetus, which lived *in utero* for twenty-three minutes after the death of the mother.

PLACENTA.

Dr. W. L. Dixon and Dr. F. E. Taylor (i, 11) gave a demonstration on the physiological action of the placenta, as evidenced by the action of an alcoholic extract of the organ which raised blood-pressure and possessed the properties of adrenalin.

Dr. Amand Routh (i, 127) described a specimen of placenta diffusa.

Dr. T. J. T. McHattie (i, 314) described a case of placenta prævia, with delivery of the complete gestation sac at the twenty-eighth week.

EMBRYOLOGY AND MALDEVELOPMENT.

Dr. John H. Teacher (i, 264) gave a demonstration of an extremely young human ovum (the Teacher-Bryce ovum), probably only thirteen or fourteen days after fertilization, the blastocyst being, in the opinion

of the authors, the youngest stage hitherto recorded of the human ovum. A brief list of the authors' conclusions appears in the *Proceedings*, but the full account of the ovum has since been published in a beautiful monograph.

Dr. Blair Bell (ii, 311) read a paper on a case of rudimentary uterus didelphys with ectopia of each uterine body in an inguinal canal, and Dr. Abernethy Willett (ii, 342) showed a specimen of bicornute uterus.

INVERSION OF THE UTERUS.

Dr. F. W. W. Haultain (i, 279) read a paper on abdominal hysterectomy for chronic uterine inversion, with a record of three cases in which the operation had been performed with success. The paper gave rise to a spirited discussion, in which it was admitted that the author's operation was ingenious, but very rarely required, four out of the five speakers having always been able to reduce chronic inversion by Aveling's repositor, and the fifth speaker also being in favour of reposition. It was pointed out that Aveling's repositor should be employed with the precautions insisted on by its inventor.

RUPTURE OF THE UTERUS.

The important subject of rupture of the uterus was brought before the Section in papers by Dr. Eden (ii, 253) and Dr. Lionel Smith (ii, 274). Dr. Eden described three cases in which supravaginal hysterectomy had been performed; two of the cases recovered. He recommended this operation in all severe cases of rupture, whether complete or incomplete. Dr. Eden condemned gauze packing, and advocated subtotal hysterectomy, pointing out the advantages in controlling hæmorrhages and preventing sepsis. Dr. Lionel Smith recorded ten cases occurring in twenty years amongst 10,989 women delivered at the General Lying-in Hospital; nine of the cases died. He pointed out that none of the patients was in a state to bear any surgical operation. Of the eight speakers who took part in the discussion of these papers Dr. Griffiths announced that he had not attended a case of rupture of the uterus. Ten cases treated by hysterectomy were mentioned by various speakers. Of these, seven (70 per cent.) died; of five treated by subtotal hysterectomy every one died (100 per cent.); of ten treated by gauze packing only two died (20 per cent.). The conclusion which may be drawn from the papers and the discussion is that rupture of the uterus may be treated by evacuation of the blood and gauze packing,

by suture, or by hysterectomy, according to the special circumstances of the case; that hysterectomy is a very severe operation for patients suffering from shock and hæmorrhage, but when it is indicated the whole organ should be removed.

ACCIDENTAL HÆMORRHAGE.

A fatal case of accidental hæmorrhage was reported by Mr. W. D. Keyworth (ii, 107). There was intraperitoneal bleeding, which was at first thought to have come from the lumen of the Fallopian tubes, but which the Pathology Committee were of opinion was due to the pressure exerted on the uterus whilst the vagina was tightly packed; extensive extravasation of blood was found in both broad ligaments and in the mesosalpinx.

FŒTUS AND INFANT.

Mr. H. S. Singleton (i, 158) showed a fœtus with an encephalocele, and Dr. Herbert Williamson and Dr. E. L. Holland (i, 161) read a paper on intra-uterine death of the fœtus, with observations upon the importance of the examination of the fœtal tissues for the presence of *Spirochæta pallida*. An excellent account of the spirochæta, the method of staining, and a demonstration under the microscope were given by the authors.

Mr. Reginald Jamison (i, 323) showed a section of the cervix of an infant, showing ectropion of the cervical mucosa.

Dr. Kedarnath Das showed a specimen of gastroschisis (i, 323) and read a short communication on chondrodystrophia (ii, 307).

MENSTRUATION.

Dr. W. Blair Bell (i, 291) read a paper on menstruation and its relationship to the calcium metabolism.

PUERPERIUM.

Dr. Arnold W. Lea and Dr. F. J. Sidebotham (ii, 127) read a paper on the bacteria of the puerperal uterus, with special reference to the presence of hæmolytic streptococci. Their paper was accompanied by a table and a list of papers dealing with the subject, and was terminated by a series of conclusions of practical importance.

Dr. Nepean Longridge (i, 193) read a communication on creatinin excretion in lying-in women, of which only a brief abstract appears in the *Proceedings*.

TOXIC VOMITING OF PREGNANCY.

Dr. R. Drummond Maxwell (ii, 236) described a fatal case of toxic vomiting of pregnancy, with microscopic sections of the liver and kidneys.

PERINEUM.

Dr. R. H. Paramore (ii, 171) read a paper which appears in abstract in the *Proceedings* on the rôle of the perineal body during labour and the conduction of delivery in relation thereto.

VESICULAR MOLE.

The Section has had before it two papers on Vesicular Mole—one by Dr. H. Russell Andrews (ii, 196) on four cases of ovarian cyst in association with vesicular mole, and the other by Dr. G. Blacker (ii, 202) on a case of hydatidiform mole with albuminuria and kidney of pregnancy in which sudden death occurred. Dr. Blacker's paper was beautifully illustrated by a drawing of the uterus with the ovum *in situ*. An interesting discussion took place upon ovarian cysts in association with vesicular mole.

INSTRUMENTS.

An improved pelvis with attached foetal skull for demonstrating the mechanism of labour was shown by Dr. H. Macnaughton-Jones (i, 41), and a modified de Ribes' bag made entirely of rubber was shown by Dr. Victor Bonney (ii, 182).

I will now mention the gynæcological contributions which have been brought before the Section.

CANCER OF THE CERVIX OF THE UTERUS.

Your President (i, 31) showed a specimen (with microscopic sections) of cancer of the cervix, removed by high amputation with the cautery from a patient aged 26, who remained free from recurrence after six and a half years; also a similar specimen (ii, 335) in which, as a result of early diagnosis by a general practitioner, the patient remained free from recurrence for ten years.

Dr. Hubert Roberts (i, 61) showed a uterus removed by Wertheim's method, and Dr. A. H. N. Lewers (i, 100 and ii, 338) showed three uteri removed by the same method; in the last two of these, although the cancer appeared to be only moderately advanced, the iliac glands were infiltrated and were removed.

Mrs. Stanley Boyd (ii, 166) read notes on the late results and post-mortem findings in a case of vesico-vaginal fistula following abdominal panhysterectomy for carcinoma of the cervix. It was referred to the Pathology Committee to report on the case, but as the sections of the original growth could not be found, and the alleged fibrous tissue surrounding the ureter was not examined microscopically, the Committee were unable to express any opinion as to the nature of the case. Dr. Gow (ii, 298) showed a specimen of malignant adenoma of the cervix, associated with adeno-carcinoma of the body.

CANCER OF THE BODY OF THE UTERUS.

Four contributions were made on this subject: one by your President (i, 119) on a case of caseating carcinoma of the body, the uterus weighing 1 lb. 1½ oz.; the second by Dr. A. H. N. Lewers (ii, 40) in which the patient recovered and was free from recurrence fifteen years after vaginal hysterectomy; in the third case, shown by Mrs. Scharlieb (ii, 43), and in the fourth, by Dr. H. Macnaughton-Jones (i, 155), the cancer was associated with fibroids, which in the last case were invaded by the cancer.

CANCER OF THE VAGINA.

One case of carcinoma of the vagina has been brought before the Section—a primary growth, removed by Dr. Russell Andrews (ii, 248), together with the whole of the vagina and uterus from below. The patient remained well eighteen months after the operation.

CANCER OF THE OVARY.

Four communications on carcinoma of the ovary were laid before the Section. Dr. J. D. Malcolm (i, 236) read notes of one in which there was also cancer of the sigmoid flexure; the uterus, ovaries, and a portion of the sigmoid flexure were removed; recurrence took place. Dr. Eden (i, 243) described a case of bilateral papillary carcinoma and a case (ii, 98) of unilateral carcinoma of the ovary with a papillary tumour of the endometrium, which the exhibitor regarded as innocent, but which the Pathology Committee thought was a columnar-celled carcinoma. Mr. E. W. Hey Groves (ii, 147) also showed a carcinomatous ovary removed from a girl, aged 16, who was well six months after operation.

SARCOMA OF THE CERVIX.

A case of this rare disease was described by Dr. Louisa Garrett Anderson (i, 153); it was removed by vaginal hysterectomy from a girl, aged 16, who recovered, and was in a satisfactory condition two months after operation. Dr. F. J. McCann (i, 253) showed a specimen of grape-like sarcoma of the cervix, which recurred a few months after vaginal hysterectomy.

Dr. C. E. Purslow (ii, 81) also showed a giant-celled sarcoma of the cervix, removed by vaginal hysterectomy; the patient died of recurrence about twelve months later.

SARCOMA OF BODY OF THE UTERUS.

A case of sarcoma of the uterus from a patient, aged 43, was shown by Dr. Lockyer (i, 245): the Pathology Committee reported that it was a mixed-celled sarcoma.

A case was also brought before the Section by Mr. Bland-Sutton (i, 233) as a sarcoma of the uterus: the Pathology Committee, however, were of opinion that it was a fibromyoma.

Another case shown by Dr. Louisa Garrett Anderson (ii, 152) as a sarcoma was submitted to the Pathology Committee, who pronounced it to be a fibromyoma.

The last case was exhibited by Dr. Eardley Holland for Dr. John Phillips (ii, 344).

CHORIONEPITHELIOMA.

Only one case of chorionepithelioma of the uterus has been brought before the Section, by Dr. Eden (i, 13). The patient was aged 26. The uterus and appendages were removed by the abdomen, and secondary vaginal growths were removed about a week later. The patient died after fifteen days, and secondary growths were found in the lungs, liver, and brain. It was said by Dr. Eden to be the only case of chorionepithelioma which had been met with at the Chelsea Hospital for Women.

Dr. Fairbairn read a paper on primary chorionepithelioma of the ovary (ii, 325).

SARCOMA OF OVARY.

Dr. Gifford Nash (ii, 225) showed a specimen of ovarian sarcoma; but the Pathology Committee, to whom it was referred, found the

tissues too necrotic to enable them to form an opinion as to its nature.

A sarcomatous ovarian tumour (perithelioma) co-existing with carcinoma of the uterus, and treated by ovariectomy, and hysterectomy, was shown by Dr. Lewers (i, 156).

SARCOMA OF MESOSALPINX.

A growth which was at first thought to be a sarcoma of the Fallopian tube, but which was found, on examination by the Pathology Committee, to be in the mesosalpinx, was shown by Dr. F. J. McCann (ii, 183).

MELANOTIC TUMOUR OF VULVA.

A specimen of this rare disease, removed from a patient aged 73, was shown by Dr. Eardley Holland (i, 124) : six months later the patient was free from recurrence. The cases were shown to be extremely malignant, only one patient out of twenty-two operated on having remained well for over five years.

FIBROID TUMOURS OF THE UTERUS, &c.

In addition to the twelve cases already mentioned as complicating pregnancy, ten communications dealt with fibromyoma.

Dr. W. C. Swayne (i, 151) showed a fibromyoma undergoing sarcomatous degeneration. Mrs. Scharlieb (ii, 45) read a paper on a second hundred cases of operation for fibromyoma, with special reference to their degeneration and local complications. Mr. Bland-Sutton (ii, 95) showed a case of fibroids in both tubes of a bicornute uterus. Dr. Lewers (ii, 148) showed a cystic subperitoneal fibroid with unusual relations, and (ii, 150) a large retroperitoneal cervical fibroid, and (ii, 229) a fibroid spontaneously expelled seven and a half weeks after delivery. Mrs. Willey (ii, 156) read an important paper, which she modestly called "some notes," on the histology of the smaller fibromyomata. Your President (ii, 177) showed a venous aneurysm on a uterine fibroid. Dr. F. E. Taylor (ii, 180) showed a uterus with two interstitial fibromyomata, one showing red degeneration, the other normal. Mr. Doran and Dr. Lockyer (ii, 25) described two remarkable cases of uterine fibroids showing peritheliomatous changes, in which there was a long period of immunity from recurrence after operation. A diffuse adenomyoma of the uterus was described by Mr. Bland-Sutton (ii, 113). A calcified fibroid ("womb-stone") was shown for Mr. J. Preston Maxwell

by Dr. Abernethy Willett (i, 228). A fibroid of the pelvis was shown by Dr. Walter Tate (i, 7), and a fibroid of the paravaginal tissue by Dr. F. J. McCann (i, 321).

OVARIAN TUMOUR.

Twelve communications dealt with tumour of the ovary. Four of the tumours were dermoids.

Mr. Alban Doran's case (i, 35) had been retained two years in the pelvis after obstructing labour. Dr. Randall and Mr. Lawrence's case (i, 105) had secondary cysts in the omentum, resulting from puncture of the primary cyst. An abstract was given of all the similar cases which had been published. Dr. Macnaughton-Jones's case (ii, 94) was complicated by an extreme displacement of the kidney. Dr. Rivers Pollock (ii, 223) showed bilateral dermoids from a lady, aged 92, who had had six children.

Dr. Lewers (i, 69) showed a sarcomatous ovarian tumour co-existing with carcinoma of the uterus.

Dr. Hedley (i, 96) showed bilateral hæmorrhagic ovarian cysts.

Ovarian fibroids were shown by Mrs. Scharlieb (ii, 41) and by the President (ii, 231), who read notes on two cases of ovarian fibroid complicating pregnancy: in one of these, in which the fibroids were bilateral, the tumour was enucleated from an ovary, and the patient subsequently became pregnant and had a living child.

Mr. G. Darwall Smith showed a specimen of adeno-fibroma (? endometrioma) of the ovary (ii, 302).

Dr. Russell Andrews (ii, 196) read a paper on four cases of ovarian cysts in association with vesicular mole.

Dr. Frank Taylor and Mr. W. E. Fisher (ii, 239) described a case of actinomycosis of the ovary.

The President (i, 172) showed an ovarian tumour with a foot of adherent small intestine, successfully removed from a patient aged 58. Dr. C. E. Purslow (ii, 297) showed bilateral ovarian tumours, one of which had obstructed labour on two occasions.

PAROVARIAN OR BROAD LIGAMENT CYSTS.

Three of these tumours were shown by Mrs. Scharlieb (ii, 42), Mr. Alban Doran (ii, 88), and Mr. W. Gifford Nash (ii, 224), whose patient was pregnant; in all these cases the pedicle was twisted.

FALLOPIAN TUBES.

A specimen of tubo-ovarian abscess was shown by Dr. Macnaughton-Jones (ii, 154), and Mr. J. Bland-Sutton (i, 229) showed two examples of the encapsulation of sterile fluid in connexion with the Fallopian tubes and ovary.

A case of hydrosalpinx was shown by Dr. J. P. Hedley (i, 95) and two cases by Mr. J. D. Malcolm (i, 99, 241); in the first two cases the pedicle was twisted.

Two cases of hæmatosalpinx complicating fibroid uteri were exhibited by Dr. Macnaughton-Jones (i, 122) and Mr. Alban Doran (i, 174).

A communication on primary unilateral tubercle of the Fallopian tube was read by Dr. H. Macnaughton-Jones (i, 177), and a case of bilateral tuberculous salpingitis was shown by Dr. F. J. McCann (i, 259).

TUBERCLE OF CERVIX UTERI.

A specimen of tubercle of the cervix uteri was exhibited by Dr. Peter Horrocks (i, 66).

HÆMATOMETRA.

Two cases of unilateral hæmatometra were shown by Dr. Thomas Wilson (i, 70).

SUPPORTS OF PELVIC VISCERA; VULVA.

The Section has had brought before it two papers on the supports of the pelvic viscera, by Dr. W. E. Fothergill (i, 43) and by Dr. R. H. Paramore (i, 195). The papers gave rise to an important discussion, in which several distinguished anatomists took part.

Dr. Macnaughton-Jones (i, 325) described a case of acquired atresia of the introitus and vagina.

EXTRA-GENITAL AFFECTIONS.

A case of hæmorrhage into the great omentum and peritoneal cavity, due to malignant growth resembling a perithelioma, was brought before the Section by Mr. Gifford Nash (ii, 226), and a non-ovarian pelvic dermoid was shown by Miss Frances Ivens (ii, 349).

Having now completed the routine record of the work done I will say that, if in some respects it is capable of improvement, it is a record of which we as a Section have no reason to be ashamed, and it fully

justifies union of the Obstetrical Society of London and the British Gynæcological Society. The quality of the work is in no respect inferior to that of the constituent societies, while its amount has been increased. The directions in which the work of your Section admits of improvement are, I think, three :—

One is the need for more general and comprehensive papers from men of experience giving the whole of the writer's experience upon the subject dealt with. The secretaries have no difficulty in getting short communications and specimens, but the supply of *papers* is somewhat limited, and occasionally short communications have had to take the place of papers at our meetings. I appeal to the more senior members to supply this deficiency and to let the Section have the benefit of their work. The younger members are doing their part satisfactorily. It would be regrettable if the senior members as a body did not take that part in the work of our Section which their position and experience demand, for I do not think that the science of medicine can make the surest progress unless the experience of the senior members tempers the enthusiasm of the younger.

The second direction in which the Section's work would be improved would be by the publication of after-histories of patients operated on, especially in the case of malignant disease, in which usually the chief interest centres in whether the patient is cured or not, as to which the Section is not enlightened if the case is published a few days or weeks after the operation. I should be glad if the Section had a rule not to publish ordinary cases of operation for cancer unless they were fatal or had an after-history of five years from the date of operation.

Thirdly, I think it would be advantageous if discussions on set subjects were held at stated intervals, of which sufficient notice should be given to enable intending speakers to collect their material. I make these three suggestions for improvement lest anyone should suppose that our body, over which I have presided for two years, ought to remain completely content with itself as it is.

When I took office I confess I did so with an anxious desire and determination that our ship, provided with its new twin-propellers, should make a good passage on its first voyage, and I am happy to say that I have been able to bring her back into harbour without any accident, thanks to the loyalty of all the officers and the crew. The Committee of Representatives, the Publication Committee, the Referees, the Pathology Committee, the Editor and the Honorary Secretaries and Council have all worked admirably for the welfare of the Section, and to them its success is due.

Vol. II., No. 6.]

[April, 1909.

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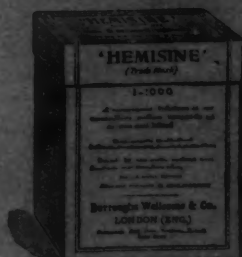
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